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Interactive comment on "Reconciling the signal and noise of atmospheric warming on decadal timescales" *by* Roger N. Jones and James H. Ricketts

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Authors' response

We would like to thank both reviewers for their positive and thoughtful comments, most of which we will accommodate in some way. It is a large paper, so we are grateful for the effort and attention the reviewers have given.

Since this paper was submitted, the introductory sections have expanded into a more philosophically-based paper, which is unfortunately only at the submission stage (although it exists as a working paper). This will be summarised in the revision in a structure that better relates theory to plausible physical mechanisms and statistical tests at

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Discussion paper

the beginning of the paper to make it quite clear what we are testing and why. This will also help to align the different uses of linear and linearity in the manuscript. The more philosophical passages would be removed from this paper.

If the paper is to be kept as single paper as recommended by the editor, this introduction is very important because we are arguing that a step-based statistical model provides a better explanation of the warming process on decadal timescales than a trend-based model and that physical mechanisms for step-like warming are both theoretically and physically plausible. This is not an argument based on statistical induction alone. We will edit section 2 to focus principally on this point, leaving the broader philosophical questions for other papers.

Response to Reviewer 1

POINT 1: My main concern is that it is not clear enough what exactly the hypotheses are that are tested. Sometimes the authors say that what is tested is whether (i) internally and externally forced components of the climate system are independent or not (page 3, page 29); sometimes they say that what is tested is whether (ii) the development of climate variables follows a trend or is step-like (abstract, Section 5). It is not the case that independence of internally and externally forced components of the climate system implies that there is a trend; this is possible, but there could also be independence and at the same time step-like behaviour. Also, it is not the case that dependence of internally and externally forced components implies that there is necessarily step-wise behavior. This could be the case, but there could also be dependence and a trend at the same time. As a result, it remains unclear what exactly is tested: (A) (Only) whether the internally and externally forced components are independent. (B) (Only) whether the climate variables follow a trend or not. (C) Whether the internally and externally forced components are independent AND whether there is a trend. (D) Whether the internally and externally forced components are dependent AND there is step-like behavior of the climate variables. Throughout the paper, the authors need to be clearer what exactly is tested. "but there could also be independence and at the

same time step-like behaviour"

The exact hypotheses will be clarified, though the testing of step against trend will be unchanged.

For H1, if the response to external forcing is considered to be independent of variability over shorter timescales (<50 years), the trend model will hold, despite often being obscured by variability. Such variability is generally represented as stochastic behaviour in annual to decadal phenomena, where teleconnections, lagged effects and regime changes all potentially interact. Alternatively, instead of a gradual line or curve, a segmented trend is sometimes proposed.

The potential behaviour of warming under H2 has many possible permutations because the signal may project onto the regime-like structures of decadal climate variability, or may dynamically modify those structures. Here, we deal with one such type of response, in the form of step changes. Step changes have been detected in warming and related climatic variables by several different methods. The purpose of this paper is to detect step changes in a range of temperature records and test these against trends to determine which carries the greater part of the warming signal. The results are used to determine whether H1 or H2 is the more viable hypothesis and, if the signal proves to be nonlinear, to explore the nature of the interaction between external forcing and internal variability.

It may be possible for internally-generated step-like behaviour on a random basis, but not sustained step-like behaviour in one direction (the exception could be a singularity, such as ice-sheet collapse). We will point this out. The H1 and H2 aspects can be redrafted to address the reviewer's points and to focus on the type of testing we undertake and why.

"it is not the case that dependence of internally and externally forced components implies that there is necessarily step-wise behaviour"

True, and we will make it clear that we are not claiming this and why we are using steps, given the amount of prior knowledge we have about their occurrence.

POINT 2: Related to this, if what is tested is (C) and (D) (as is often suggested; cf. in particular the hypotheses on page 5), then it is important to see that (C) and (D) are not exhaustive (because there are also the possibilities that there is independence and a step-wise development; or that there is dependence and a trend). The authors want to test an exhaustive set of hypotheses, but (C) and (D) are not exhaustive.

We are not testing an exhaustive set of either physical or statistical hypotheses and will make this clear; however, understanding the theoretical background and potential mechanisms that informs the testing environment is important. The existing research identifying step changes as a mechanism for warming change on decadal timescales needs to be tested in an environment that is dominated by an existing paradigm that says the opposite. Because paradigms are partly a sociological construct (though are informed by theory) only a strong case that can cover the theory along with addressing cognitive values has a chance of being accepted. The framework we are using is a theoretical-mechanistic – statistical induction framework where theory is used to distinguish plausible mechanisms that allow a clear choice to be made between alternative hypotheses. This will be described more clearly. Statistical testing provides the means to do this, so statistical hypotheses need to be developed that match the alternative mechanisms. So what we have to do is to make this clear in the light of the A, B, C and D possibilities that Reviewer 1 puts forward.

POINT 3: Throughout the paper the assumption seems to be that "trend-like" and gradual as opposed to step-wise and non-gradual means that there is a linear relationship (e.g. on page 7). It is unclear why gradual implies that there is a linear relationship. There can be gradual behavior with various kinds of relationships (a quadratic relationship etc).

Here linear is referring to whether the temperature signal follows a secular trend there-

fore can be defined by a line and refers to the linear transfer relationship between forcing and temperature. Nonlinear response implies the transfer is nonlinear and is modified by nonlinear physical processes. This will be more tightly defined initially and the different between physically linear response and linear trends, for instance, made clear through-out. The usage in the paper is physical and when statistically linear is used is will be specific, e.g., linear trends.

POINT 4: On page 7 the hypothesis states that there is a "(probably monotonic)" trend. The brackets are confusing. Is it now tested that the trend is monotonic or is it allowed that the trend is not monotonic?

Related to the previous point – we will clarify earlier that the underlying signal of atmospheric warming is assumed to be monotonic (while we accept that total global warming is monotonic). This is later clarified on page 6. This section will be rewritten as per the overall response to both reviewers 1 and 2.

POINT 5: on page 5 six tests are described. It should be clearly stated which tests test which hypotheses (becomes clear later, but should be stated clearly early on).

It will be simpler if just H1 and H2 are referred to previously – all six tests are designed to assess which has the better explanatory power and more detail will be added to make this clear up front.

POINT 6: The beginning of Section 2: here it is argued that the gradualist thesis is derived from induction. Yet, as the paper later argues, the data actually do not support the gradualist thesis and the gradualist thesis rather seems to be often adopted for no empirical reasons (convenience, simplicity). Hence it seems that the gradualist thesis is not justified by induction after all. It is probably more accurate to say the gradualist thesis is sustained by statistical inference (as a form of induction).

We have done some more work around this to inform other papers on the same theme. In the 1970s, various theoretical arguments were put to suggest if a system received

a small internal forcing the response would be proportional (e.g., Leith 1973, 1975). Much of the subsequent work was based on vectorizing the forcing-response relationship, which will produce a linear outcome. This was based on statistical physics.

There are two types of induction therefore: one is largely analogical, based on statistical physics, proposed by scholars like Leith. This is now generally accepted as an approximation that only holds if the residuals are Gaussian (e.g., Palmer, 1999) and they are not in every case. There have been two styles of statistical induction. The first in the 19th century based on, which is truth-centred. The other is modern statistical induction, which is often reduced to a mechanised form based on establishing a sufficiently small probability for the null hypothesis. For some, the trend has again become truth-centred, but this can is only be the case if warming is gradual. Otherwise, it is a statistically-derived approximation of the relationship between forcing and response. This issue is at the core of the paper.

Other cognitive values such as convenience and simplicity are applied, because they are mentioned in the literature frequently, often as an escape clause to bypass some acknowledged but unknown complexity. They are also used as a defence against over-parameterisation and overfitting. Their use is mainly sociological rather than scientific. A full discussion of all of these is not appropriate for this paper, so we will pare it back to the basic issues – we acknowledged these other issues because readers will realise that the story is incomplete but a more comprehensive exploration is not feasible.

This issue has been complicated by the climate wars, where trend analysis has been used as a defence, so if not strictly truth-centred, it had to be defended as scientifically correct. This has hampered the consideration of alternatives. This comment is largely background because most of the philosophical content would be removed in a revision leaving the severe testing component.

POINT 7: The beginning of Section 2: "The application of linear trend analysis to atmospheric warming is invariably justified as inference to the best explanation". I am

puzzled by this sentence. Why is there suddenly a reference to the inference to the best explanation (previously the matter of concern was induction).

This point is about simplicity as a cognitive value – invariably is also too strong a word. Some of the confusion can arise because there are multiple frames around this issue, as there will be around any long-held view that has evolved over time. The literature may describe the theoretical developments around an issue but it will also reveal sociological aspects of how and why a community of practice believe certain things. These can conceivably be quite different – because the latter will contain assumptions of convenience and workarounds. Most of the discussion about reasoning will be removed except for that which relates directly to the testing approach.

All technical corrections can be addressed.

Response to Reviewer 2

General comments

The single paper option will be chosen as recommended by the editor and the introduction and Section 2 rewritten to focus on the statistical testing. However, it is important to preface the statistical tests with the probative conditions as to why those tests were chosen. It is also possible to simplify the relationship between theoretical propositions of independence and interacting externally-forced and internally-generated change (as per the response to reviewer 1), the mechanisms reflecting those theoretical propositions and why they were selected and the statistical hypotheses. The most important aspect of this is to test between sustained incremental change as would be manifest in gradual warming and episodic change. For that reason, retaining Page 5 lines 6–19 is important because it explains what the statistical hypotheses represent.

Another manuscript that expands on a theoretical-mechanistic and statistical-inductive framework the informs the testing environment and expends greatly on Section 2 has been written, so this section will be tightened to serve as a platform for interpreting the

test results.

Specific comments

Abstract – test numbers can be bracketed

Introduction – see above

2.3 line 23 – ill-posed inverse problem will be given a plain-language description. The upshot of that is that a great deal of statistical testing treats mean global warming as a one-dimensional problem, which it is not. This relates to part of test 1: regional stratification – we can nominate this better in the test description.

External forcing – linear – the explanation referred to in the response to Reviewer 1's comments will clarify the relationship between physical and statistical linearity.

Decadal scale – the most widespread usage is around timescales, not means. We don't see how this can be confusing. CMIP time series are all annual – we're not sure where the 5-year figure comes from. The only place where five-year averages are used is for estimating total warming over a given time period. We do compare steps within set decades to ECS but that is a special case.

Page 6 – I do not see why entrainment of heat energy into the various heat reservoirs of the Earth and especially the hydrothermal system need always be nonlinear

Perhaps this is a short-hand argument, but the transport of heat from the equator to the poles is fundamentally nonlinear at the global scale. Some of the transport into the heat reservoirs (e.g., the west Pacific warm pool and the deep ocean is more gradual). However, we would argue that the atmosphere-ocean processes involved in the largescale transport of energy in the system is nonlinear.

Linear behaviour in specific processes or locales is of course possible. Alternatively, there is no reason why the first-order assumption in a nonlinear system should be to assume linearity – it may be, but the assumption it just is, is habit and a social construct.

This section will be rewritten but the nonlinearity of the hydrothermal system will remain a feature.

Page 6 – Lines 2 to 6 outline a number of alternative approaches to determine 'shifts', 'change points', 'step changes', but there is no discussion of the advantages/disadvantages of these different approaches and why they were not used in this study. See also: Drijfhout et al. (2015) and Reid et al. (2016) Reid PC, Hari RE, Beaugrand G et al. (2016) Global impacts of the 1980s regime shift. Global Change Biology, 22, 682-703.

Because it was judged that an objective rule-based version of the bivariate test was the best tool we could use, based on our previous use of the test and the effort that has been put into developing the multi-step rule-based model. This has been discussed in past papers and we will be a little more emphatic about it. This discussion will be held in another paper currently being prepared by Ricketts, who does test some of the alternatives.

There is a good summary in Rodionov (2005) and the bivariate test is on a par with the Alexanderssen test (Rodionov does not mention the bivariate test but colleagues at the Australian Bureau of Meteorology tested both in the 1990s when developing homogenization strategies and judged them to have similar performance). The bivariate test has the advantage of being able to use different reference time series. The multi-step procedure was developed to overcome the problems with multiple steps, where the test results do not hold – whereas they will for sequential testing.

Page 7/8 – acronyms

We will put a Table for acronyms in the Supplementary Information – ECS is given in the text previous to the acronym on Page 7.

Page 7 – ECS

It is explained later, used as an independent variable against which to measure time-

series components through correlation to determine which carries more signal and which carries more noise. We will slightly expand the explanation of the tests as also requested by Reviewer 1

Page 9 lines 22 to 28. A diagrammatic representation of the different terms used for the analyses is needed. A descriptive expansion of what is meant by each of the terms would be helpful. The word 'shift' has been used in a different way in previous papers and a different word would be more appropriate here for this characteristic. Is the text in brackets at the end of the last bullet correct?

Finding terminology to go with nonlinear change and defining and measuring it is difficult, whereas there is so much language associated with trend analysis and framing around this that we are used to. We retain steps for the bivariate test because that is what it measures (as is the case for the STARS (Rodionov) test). After much consideration, shift was chosen as the most representative measure of visually what can be seen and measured across the gap produced by displaced trends. A diagram will be produced. Note that the term regime is still being debated (e.g., Overland et al., 2008) – a scientific language for nonlinearity needs to be developed. We cannot do that here. Can do a figure.

Page 23 Section 5.1. This section would be better drafted as the conclusions of the paper rather than as a summary of severe testing.

We would prefer to leave this here and focus on this summary in the discussion – it is a long paper and we see a summary and the conclusions as being slightly different.

Page 25 line 2. Again I do not like the use of the word decadal here. Table 6 does not show that hstep is better at a decadal scale the steps are occurring within a year, but may continue at a new level or develop a trend afterwards for more than a decade.

Can be changed to decadal timescales, but the use of decadal scale to signify timescales of decades is almost ubiquitous in climatology and if decadal means are sig-

nified, decadal mean is the term usually used (Google scholar confirms this if “decadal scales” and climate are search terms). We are quite puzzled with these objections to using decadal in this sense. The only exception might be for ocean sediment coring, where sampling horizons can be decades and centuries.

Page 27 line 1-2. The hiatus is now thought to be due to an increased storage of heat deeper in the ocean and is not a continuing event considering the warming of the last few years. See Reid 2016 and references included.

We disagree with this interpretation because we view the hiatus as a regime, and the steady state in between step changes a normal part of climate. In another manuscript ready for submission, we go into this in greater detail interpreting the deep ocean mixing as contributing to the length of the regime because it takes longer for the heat in the shallow ocean to build up to critical levels (Of the 58 CMIP5 models that underwent a step change in 1996–98, the longest interval was 26 years and the shortest 7 years). If heat is being stored in the deep ocean, less is available for the western Pacific warm pool, which acts the heat engine for global climate.

The so-called hiatus and previous mid-century pause (Wally Broecker coined that term) was clearly related to a La Niña phase of the Interdecadal Pacific Oscillation, however, for the 1977 – 1996 period of the El Niño phase, we suggest there were two steps rather than a trend. We are likely to be in the next step change and Peyser et al. (2016) have identified the trigger for this in dynamic changes in sea level in the warm pool, leading to an outburst of heat (They interpret this as variability, but our view is that it is a nonlinear expression of the climate signal).

In the revision, we will reinterpret two recent publications (Peyser et al., 2016; Meehl et al., 2016) that came out after the initial submission to explain the trigger for the recent warming and that which occurred in 1996–98 (Peyser et al., 2016). Meehl et al. (2016) suggest that the IPO may be switching from negative to positive that they interpret as the resumption of an increased warming trend, largely similar to the comment above.

Page 18 line 4. Lack of predictability. How can the authors be so definite that this might be due to aerosols?

Because of the negative correlations associated with decades of negative steps responding to volcanic eruptions and sulphate aerosols of the immediate post WWII period and the positive correlations cancelling each other out – as discussed on page 17 – not sure why this is contentious – will look at making it more obvious.

Page 14 lines 23-25. Sea level steps are said to be ubiquitous in local tide gauge time series, Table 3 in Jones et al. 2013, but were not checked or analysed by Jones et al.

Misreading of the table therein – model sea level not checked – that statement was for observations. Tide gauge records are illustrated in Figure 36 (Fremantle, San Francisco) of that reference. We are considering putting in a panel of four non-temperature step changes in the revised version to better illustrate Test 5: two tide gauge record (San Francisco, Fremantle), rainfall (northern Australia and south-west Western Australia) and shallow ocean heat content.

Technical corrections – all corrections addressed unless otherwise indicated

Page 2 Line 2. Abstract. Change to: 'variations that extend over decadal scales of time'. See later comment on use of decadal.

See responses above

Page 2 Line 13. 'the correlation'

2.2 Line 21. First mention of H1 and H2 together. They were used separately in the introduction.

Not sure what the point is here – these sections to be rewritten as per both reviewers' comments.

Page 6 Line 7. Start 'For H1: : :.'.on a different line to make it comparable to H2 below. There are no citations to back up the statements made in the H1 section.

This is because it's our reasoning – will make this clearer in the rewrite – there is another ms that describes this in detail

Line 18. Decadal again. The transfer from one regime to another is evident at an annual level and not decadal.

See comments above

Line 17. Should not be numbered 3 or indented.

No. We think there a three distinct points, rather than two choices. Wording amended to clarify this.

Lines 25 to 30. This text should be part of a discussion and not here.

Ok, but section and whole discussion will be rewritten.

Line 33. At the end it is important to note that regime change is precipitated, but to a new level or a trend.

Will consider this, but probably best for discussion – Peyser et al. and Meehl et al. (2016) allow us to better identify the mechanism.

Line 31 to Page 7 line 4 repeated below. – will remove

Page 7 line 12. –H1 and –H2 mentioned for the first time. Define what they mean in general language.

Section to be rewritten and -H1 and -H2 to be removed to simplify.

Page 7 line 13 onwards – Six tests are identified. It is not clear if the first two are the same as the two tests mentioned on page 9 lines 31-32. Please make this section clearer.

They are the same – will expand this section slightly to say what the tests do in more detail.

Page 8 line 19/20 "MYBT is considered reliable". Is this remark necessary without some backup? You could refer to page 13 line 2 in the Supplementary Information

We will be more direct about why the bivariate test is being used. Twelve months' development went into the rule-based multi-step component adapting the original test with a great deal of testing to ensure that the results were robust, consistent with known steps and the test could be as reliable as possible with data that contained real trends and lagged autocorrelations (ENSO-like). There is no doubt that redness itself will produce shifts in the data, which is why theory is so important when trying to interpret the results. Some of this is documented in Ricketts (2015), but unfortunately the final paper describing the model has not yet been finalised.

Page 9 line 2. Put in a heading Data and distinguish between the observed and modelled time series by putting them in different paragraphs. It would have been helpful to leave a line space between each paragraph. Page 9 line 13 Again provide a new sub-heading

Page 9 line 32. Again, does the reference to Test 1 and 2 refer to the first two tests of the six mentioned earlier?

Page 11 below line 13 put in a heading: Shift/Trend Ratios

Page 12 lines 1 and 2. An important result. Missing full stop after warming.

Page 12 line 7. Suggest change to "Annual and seasonal anomalies were investigated". And edit next sentence so not starting with Annual.

Page 12 line 12. Why are quarterly anomalies only examined for the satellite temperatures? This needs explaining.

All ok to do

Page 12 line 23. Confirmation of the results from Reid et al. 2016 that the 1987 regime shift is evident at a global scale and yet on the next line it is said to be only evident at

a regional scale.

Will explain why those results come about – mainly through using different area averages – however, not confident that all the step changes/shifts identified by Reid et al., (2016) should be allocated to 1987/88 (e.g., Australia).

Page 12 line 25 and 26. An important result. "When all four records are plotted on a common baseline of 1979–1998, the surface and satellite temperatures display similar shifts but different internal trends (Fig. 3)".

Page 12 lines 333-34. An important observation. "Unless substantially contaminated by artefacts, these changes do not reflect gradual warming in the atmosphere, but instead may reflect regime-like change controlled from the surface". As is the subsequent comment on heat release from the ocean during El Niño. See commentary in Reid 2016 on this issue.

We will go into this into the discussion, especially through Peyser et al. (2016)

Page 13 line 5. Which timescale? – ok *Page 13 line 14. Insert 'out' after carried.* – ok *Page 14 line 18 An important observation. "indicates that the onset of the warming signal in these broader regions is abrupt (Jones, 2012)". Page 14 line 21 Use year (2016) of hard copy publication for Reid et al. (2015).* – ok *Page 14 lines 23-25. Sea level steps are said to be ubiquitous in local tide gauge time series, Table 3 in Jones et al. 2013, but were not checked or analysed by Jones et al.*

Misreading of the table therein – model sea level not checked – that statement was for observations. Tide gauge records are illustrated in Figure 36 (Fremantle, San Francisco) of that reference.

Page 17 line 4. Why are 5 year averages used here, the first mention that the data has been treated in this way.

Not sure why this is an issue – it's a simple difference using 5-year averages to avoid single-year variations. The IPCC often uses 10-year averages in a non-stationary sys-

tem for simple differences, we use five to minimise the sampling errors with one year but wanted to keep this interval as short as possible. Because it has been done consistently for an ensemble it will provide consistent results.

Page 18 line 33. Lable bullet A1 and at top of next page the bullet A2.

Not sure what A1 and A2 signify

Page 19 lines8-9. "peaking in the 2080s: : :.." does not fit with the figure 5f. What should be Fig. 5f is a duplicate of Fig. 5d.

Not sure how that happened – will fix

Page 20 line 23. Is the first part of Section 5 essential to the paper? Would it be better to label it 'Sensitivity testing'.

Yes, this is essential and is a comprehensive way of testing whether the time series are steplike or trend-like on the timescales of interest. The section will be edited slightly to sharpen it.

Page 20 line 25. Insert 'and' after 'warming'? Page 21. Line 30 change to: 'performs the best' Page21 line 31-33. Duplication 'into the' and 'test'. Change to: 'at a global scale when each model is' Page 22 line 30. '21st' Page 24 line 9. Spelling 'are' not 'area' Page 24 line 18-19. Edit sentence beginning: 'Warming is not: : :.' Page 24 lines 24-25. Make sure this statement is backed up by appropriate citations in the results section

ok

Page 24 lines 9-10 and 30-32 repetition. Is this necessary.

Section to be edited and this removed

Page 25 lines 5 to 10. Delete 'In summary' and draft as the final paragraph of the conclusions.

Will move into the conclusions and edit

Page 25 line 7 in situ in italics. And, 'or as a gradual'. Page 25 lines 9-10. Edit to: 'where climate change and variability interact rather than varying independently.' Page 25 line 13 Discussion. Include a discussion of how the results of Drijfhout et al. (2015) compare to those presented in this paper. Drijfhout S, Bathiany S, Beaulieu C et al. (2015) Catalogue of abrupt shifts in Intergovernmental Panel on Climate Change climate models. Proceedings of the National Academy of Sciences, 112, E5777-E5786.

Ok

Page 25 line 15 change 'earlier' to 'before'? Page 25 line 17. 'gradualism' and 'as a key tool to understand how'? Page 25 line 23 'to explain climate' Page 25 line 24. Change' 'covering methods' to 'applying procedures'? Page 25 line 25. Delete 'and its application to understanding climate processes'. Page 26 line 5. 'analytical'. Page 26 line 12 a priori Italics Page 26 lines13-14. Important observation that needs to be included in the conclusions. 'the processes involved are timescale invariant indicate that the meaning of seamless has not really been thought through'. Page 26 line16. 'would likely be'. And change 'considerable' to 'sizeable' as repeated on the next line. Page 26 line17. Change 'under' to 'that have'. Page 26 line 19. First sentence of bullet. Something is missing. Page 26 line 20-21. 'physics, understood as being primarily linear and hydrometeorology with its substantial nonlinear behaviour; both remain largely unreconciled.

All the above to be removed except for the bullet point on decadal prediction, retained in discussion

Page 27 line 9. 'stated'

To be removed

Page 27 line 20. Somewhere in the text above it is worth stating that both Cahill and Foster consider that the hiatus was a non-event.

This passage is to be removed from the discussion

Page 28 lines 7-9. Delete: 'As we discussed in a related paper where H2 is examined in greater detail' and the reference to Jones and Ricketts, 2016 as this paper is only 'in preparation'. Edit the sentence without the above text except for H2.

Text to be removed and discussion rewritten

Page 28 lines 11-18. An important paragraph. You might also cite Roemmich's recent papers and Reid 2016 to back up this paragraph.

They are cited already

Page 28 lines 19- 22 repeated below on lines 23-26.

removed

Page 28 line 31, The word 'extraordinary' is perhaps a bit too strong.

Edited

Page 28 line 32. 'to either side' Page 29 lines 1-2. Leave out the sentence: 'Elsewhere : : :..'., but, raise the possibility that we are undergoing another shift at present.

Substantially rewritten with new research to suggest that we are undergoing another shift at present

Page 29 lines 3-5. Poor ending to this section. Edit and improve as a statement to round off the discussion.

Thank you and will be done

Page 29 line 13. See earlier comment on >50 year climate change.

The context for this will be made much clearer – we state that it is a complex trend over the long term – this is physically important as it relates to changing boundary conditions

Page 29 line 17. Delete sentence beginning: 'We discuss this : : :..'

Done

Page 46. Figure 4. I don't know what the journal policy is for sub-figures, but I prefer the lettering, a, b, c to be in the top left hand corner, inside the enclosing border of each sub-plot. It would also help if the respective sub-plots were labelled: England, Texas and Australia within the enclosing border. Insert at the beginning of the legend: 'Regional temperature change'.

Page 47. Same comment as for Figure 4. Label a, b, c, d, e, f in the top left corner of the subplots and in the top right in order: Add in sequence in the top right corner of a: 'observed', of b: simulated, of c: '2.6', of d: '4.5', of e: '6.0' and of f: '8.5'. In the legend add downward blue and upward red as for Figure 1.

Figures can be edited as per suggestions

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