**Response to Reviews of Stott et al Position Paper**

We would like to thank all the reviewers for the time and trouble they have taken over their detailed reviews of our paper. We are grateful to the many good points they have raised and as a result of taking their comments into consideration we think the manuscript has been considerably improved by clarifying our arguments, by avoiding repetition and in properly referencing statements.

Our responses are provided inline where the reviewers’ comments are in italics.

**Reviewer A**

We thank the reviewer for their detailed review and for their many insightful comments.

*1. This chapter reviews current research on climate event attribution. It provides an overview of the principal research questions, a discussion of applications of climate event attribution analysis, a description of current methodology, selected case studies, and pointers to current research issues. It is a position paper addressed to the broad WCRP climate science community and is meant to identify critical research gaps for WCRP to deal with in the coming years.*

*2. The topic of climate event attribution has emerged largely in response to societal demands for scientific assessments of the cause of individual extreme climate events. The importance of the topic, which also has implication for climate prediction, is clear. It is also clear that the assembled author team is at the forefront of research in the area and so is well suited to produce a useful contribution to the OSC volume.*

We are glad that the importance of this topic is recognized.

*3. That being said, the current submission requires major revisions before it can be included for publication in the volume. The core of the material in Sections 4-7 is solid and in generally good shape, but the introductory and discussion sections (Sections 1- 3 and 8) need substantial revision.*

We have deleted section 2 and made major revisions to sections 1, 3 and 8 which we believe has addressed the concerns of the reviewer. We have also made revisions to other sections in the light of the reviewer’s comments which are detailed inline below.

4*. The introductory material (Sections 1-3) never defines what is meant by a weather/climate event and for the first several pages is devoid of citations to the science or policy literature. (Even if the paper is a "position" paper with more of a policy focus than a research article, it still requires a solid foundation in the available literature.) The style is repetitive and sometimes obtuse. I think that much of the material before Section 3.1 could be shortened or even dropped. Given this, I recommend that the authors*

 *a. Begin by concisely defining (providing appropriate citations) what is meant by a weather or climate event (including its temporal and regional character) and then use consistent terminology to refer to such events throughout the chapter.*

We have added a new paragraph defining what is meant by weather and climate-related events and use this terminology consistently

 *b. Concisely clarify (providing appropriate citations) the extent to which event attribution is an application of existing Detection/Attribution and modelling methodologies. This would not take away from the message about research gaps, but would reassure the reader that the problem is in principle tractable and can be approached systematically. The introduction makes the topic appear more mysterious than necessary.*

The introduction has been rewritten to make the connection more clearly between event attribution and attribution more generally.

 *c. Significantly shorten the "review of attribution science", in Section 2; it is important in my view only to clarify that the very well known IPCC assessment conclusions on mean temperatures and extreme temperature events are based on established detection and attribution methodologies.*

We have deleted section 2 and folded discussion of IPCC assessment conclusions into the introduction as part of the motivation for why development of attribution of events is needed and to describe the relationship of this activity to more traditional detection and attribution approaches. Supportive references have been added to the text.

 *d. Clarify the distinct character of event attribution as a form of hindcasting. To my understanding these characteristics include: 1) a focus on highly time specific and localized/regional events, 2) a focus on hypotheticals (what would have happened in the absence of a given climate driver), and 3) a focus on the very tails of the distributions of most climate variables. These points are made in the review but could be brought out sooner.*

These points are made in a new paragraph in the introduction linking to a properly referenced paragraph on the current situation in which such attribution assessments are generally lacking. We also take the opportunity to state that attribution assessments are inherently probabilistic.

*5. Parts of Section 3 are reasonably good, but Section 3.2 on the public interest is poorly edited, full of generalized statements (e.g. "If people hear that a particular weather event . . .") without evidence based on citations or references to media articles. Although Section 3.5 on geoengineering is reasonably well written the connection to the rest of the chapter is not direct.*

Section 3 has been revised to tighten it up and include appropriate references.

*6. Similarly, the discussion, Section 8, is very general and repetitive. There is, for example, a lot of overlap between the need to define user requirements and the need to develop clearly defined questions. I also think this discussion would appear better informed with more references to existing literature, e.g. to the hydrology or agricultural literature (for example in flood risk work, where defining probability of recurrent rare events is I believe common practice) in Sections 8. Nor do the underlying research methodology requirements come out clearly in the discussion. For example, the question is raised on p.33 of Queensland flood risk in the presence of both AGW and ENSO variability. Would it not be possible to concisely phrase this question in probabilistic terms and so distill a specific research issue? Being concrete would help the reader understand what the authors aim to establish as their position.*

Section 8 has been much shortened, with un-necessary repetition deleted and merged with the concluding section to provide a section summing up the position paper.

*7. The issue of climate observations is raised in the introduction and in the discussion, but concrete examples of observational issues are not really provided (beyond a reference to Stott and Thorne). This is one area where interactions between modellers and observationalists within WCRP and various government agencies might really lead to progress, and so concrete examples here would be particularly useful for positioning this research. In populated areas of developed countries, I would be surprised if gaps in Earth Observations seriously constrain climate event attribution. In these regions, societally important events (high temperatures, droughts, etc.) are relatively easily observed and placed in historical context. But their precursors might not be and so there might be an argument that enhanced observations would help with that. More importantly, (to my understanding) poor observational capacity in the developing world is a serious obstacle, e.g. to carry out climate attribution Sahel drought events requires reliance on sparse precipitation networks in that data limited region.*

We disagree that improvement of the observational records in various ways are not a critical component of better attribution systems, even outside the developing world. We cite the paper by Trenberth to support this contention and elucidate further on the need for understanding and quantifying remaining observational uncertainties with citation.

*8. There are several terms that are used in problematic ways. For example:*

 *a. "authoritative": This term is used throughout the Chapter. Somewhere it needs to be clarified what is meant by this term. Appearing in the peer-reviewed literature? Approved by scientific panels? Government approved? Perhaps if the explanation of an event is timely, objective, and based on a clear and reproducible methodology this contributes to an authoritative explanation. But of course it does not guarantee an authoritative explanation. Alternative explanations could be mutually inconsistent, but could still be scientifically credible and authoritative, even if some are later proven incorrect.*

Authoritative fundamentally means that the results have credibility and can be trusted like weather forecasts (where there is an understanding of their uncertainties). Where “authoritative” is first used in the text we have elaborated what is meant.

 *b. Other examples: "robust" (is this "statistically robust", "unlikely to change soon", "authoritative"), "timely" (what is the required time of information delivery --- evening news? end of week review?).*

We have added a definition of what we mean by timely to the text. By robust we mean scientifically robust – as we state the first time the word is used, ie as eluded to in the text, the standard meaning of conclusions that are robust to current scientific understanding of remaining uncertainties.

 *c. Also, instead of variously using words like "early warning" (e.g. p.4, para.2) and "anticipated", why not use the technical terms like "forecast"/"predict"/"project"/"hindcast" that are well understood by the WCRP community? These terms are useful because they involve specific operational timescales and technical approaches that the readership will understand. For readers outside the WCRP community who might be unfamiliar with these terms, technical terms might be defined and explained in the chapter in a technical glossary.*

We keep the term “early warning” since it has a clear wider meaning of the application of a prediction system but we do clarify what is meant in the revised text by describing “early warning of any enhanced risk of such events a month or more in advance”.

 *d. "Scientific process", "regular attribution process": I think what is meant here is "methodology". But perhaps what is meant in some instances is an operational procedure.*

We have attempted to clarify in the text what we mean by a regular attribution assessment. In particular we now discuss the new adjunct to the BAMS State of the Climate report due to be published in July which aims to provide a regular (once a year) attribution assessment to sit alongside the monitoring product that is the State of the Climate report. This new development helps to explain what we mean here.

 *e. "Science": The authors should review their use of this term. To my mind, this is a term with a connotation of a broad or new field of scientific knowledge, e.g. "biological science", "climate science", "nano science", etc.. I would not say that "event attribution" qualifies as a "science" but as a methodology. The overused phrase "science of climate change" could be modified to "climate science" or the "scientific underpinning of climate change".*

The text has been edited so that we refer only to climate science throughout. In the revised version, event attribution is referred to as just that without the additional appendage, “science of”.

*9. Following on from 8c, if the authors are going to discuss climate prediction in detail, this raises issues of climate state estimation and data assimilation (especially land surface data assimilation), which are surely beyond the scope of this chapter.*

We agree that these are beyond scope but we do not discuss climate prediction in detail.

*10. Section 8.6-8.7 could be expanded and made more concrete. For example, it might be useful here to talk about different climate regimes: high latitude/temperate latitude/subtropical/tropical and cold versus warm versus transition season, where different process understanding and expertise might be required. This would give the WCRP readership more material to work with and assist in development of research strategies.*

Given other review comments we have had that the text is too long we have not lengthened this section but rather shortened it and combined it with the concluding section. There are concrete examples of attribution studies given in (revised) section 4 which provide specific examples of why physical understanding is essential and why event attribution requires modeling which we have more carefully elaborated at appropriate points, also in response to other reviewers’ comments.

*10. Detailed comments:*

 *a. p.2, para. 2: the events listed parenthetically are not precisely defined --- please identify seasons when they occurred for each event.*

Done.

 *b. p. 3, para. 2: This requires major revision. The points are in my view exaggerated and suggest a lack of focus in the chapter. Please cite a credible policy or science source that claims that "all weather and climate-related extremes could not have occurred without a human influence on climate, or that no single event can evert be attributed unequivocally to a particular cause". Is it the job of this chapter to deal with producing a "comprehensive inventory of the net cost of climate change"? In this one paragraph, the main topic of the chapter --- climate events --- are referred to as "events", "weather and climate related extremes", "extreme weather and climate", "weather events".*

This section has been substantially revised to include appropriate citations to support the statements made with a focus on the current potential for confusion, the demand for better information and the relatively immaturity of the science. Throughout the paper we now refer only either to “weather and climate-related events” or else “events” for short.

 *c. p.3, next para.: please provide an example (with citations) of a situation where an assessment is lacking or incomplete*

In the revised text we refer more helpfully to the contrast between the current state of monitoring science and attribution science, as exemplified by the mature State of the Climate report in BAMS and the initial attempt to produce a companion attribution piece for BAMS (Peterson, Stott, Herring, eds).

 *d. p.4, para. 2: As I stated above, no need to "provide an overview of how climate science has developed", but instead to make a more precise connection between climate event attribution and current methodologies in Detection/Attribution and climate modelling.*

Agreed. This has been done in the revised text.

 *e. p.6, last two lines: this sentence repeats previous material.*

This has been deleted and this section revised to relate traditional attribution studies to event attribution studies rather than an “overview of how climate science has developed.”

 *f. p.7: Intro to Section 3 is poorly written, especially the last sentence. Overall, it is open for debate whether attribution assessments as discussed in this paper (typically, responses to societal demands for quick information after events have occurred) could contribute usefully to improving models. Unlike hurricanes, many climate events of societal interests are "one-offs", which are challenging to use for model improvement. Are there any examples so far that can be cited?*

The introduction to this section has been rewritten and shortened and citations inserted supporting the text in the first part of the section.

 *g. p.8, l. 3: "conditions" -> "events" (?)*

Edits made to refer to “events”, or “weather and climate-related events” throughout

 *h. p.8, para.3: I really don't know if it is necessary to discuss in such a roundabout and perhaps deliberately naive way that model realism is important to predict climate events. No climate model can perfectly capture the statistical distribution of climate events. But even poor ones are used extensively in detection/attribution of past extremes and projection of future extremes. The important question is whether this new application presents a special technical challenge for modelling. I would guess that the answer is "no" --- attribution of climate events and FAR analysis should be an extension of existing analysis methodologies but does not require new modelling capabilities. But if the answer is yes --- which would imply that we should exclude poor performing models from this kind of analysis --- this case should be made more explicit and relevant literature provided. In addition, this paragraph muddies the waters between being able to explicitly predict events and being able to assess their likelihood.*

The key point we were trying to get across here is that a crucial aspect of using a model for attribution, as it is for prediction, is to assess the model’s reliability as this is needed to assign a level of confidence or trustworthiness to the output of the model. An attribution assessment that does not also include an assessment of the model’s reliability and therefore an assessment of the extent to which the results from the model can be trusted, would be a poor assessment. This is the central point we wish to make, a point also valid for predictions. We have amended the text to make this point clearer with the addition of appropriate references.

 *- In the same paragraph: "Many classes of extreme events have a strong weather (rather than climate) context" --- please be more precise about what is meant here.*

Text has been deleted here as part of tightening up the text.

 *- In the same paragraph, tangent on weather forecasting makes it unclear what the scope of the article is.*

Text has been deleted here as part of tightening up the text. The focus here is on predictability a month or more ahead rather than weather forecasting predictability of a few days. This has been clarified.

 *- In the same paragraph, p. 9: "a common view of the climate context" . . . unclear.*

Text has been deleted here as part of tightening up the text.

 *i. p.10, para. 1: The example of the cold and hot spells in the UK is useful, and it would be useful to cite specific media reports on these events.*

References to UK Met Office climate statistics and to media reports have been inserted.

 *j. p. 10, para. 2: This paragraph really has the feel of opinion rather than a solid fact-based position statement.*

We have made major revisions to this part of the text to include relevant supporting references. *k. p. 11, Section 3.3: This interesting section stands out from its neighbours in that it discusses specific peer reviewed literature with societal implications. There are no citations in Section 3.2 (which is much longer) and one briefly mentioned citation in Section 3.4, which presumably has a large literature to draw from.*

We have made substantial revisions to both these subsections including adding relevant supporting literature.

*l. p. 13, Section 3.5: The geoengineering discussion is potentially relevant but its connection to the chapter is not entirely clear. Perhaps this is because climate events are never really defined in the chapter.*

Discussion here has been clarified by reference to events and therefore a potential requirement to attribute events to geoengineering should there be extreme weather or climate-related events that get blamed on a geoengineering intervention.

 *m. p.14, l.-2: It is true that models are required but does it really need to be said (again)?*

The requirement for models is stated more clearly in the observations and the text in this section has been deleted.

 *n. p.15, para.2: delete "or drivers of interest" for consistency with first sentence.*

Done

 *o. p.15, para.3: "world that might have been . . .absent" -> "absence of the climate driver", "were there no . . . climate" -> "in the absence of the climate driver"*

The text has been simplified along the lines suggested.

 *o. p.15, starting line -2: "departure" -> "threshold" (?), "influence" -> "driver" (for consistency with previous paragraphs?), [next page] "human influence" -> "anthropogenic radiative forcing" (more specific driver).*

Changes have been made in the spirit of the review comment to simplify the text. “Human influence” left in as this is a commonly used phrase.

 *p. p.16, same paragraph as previous point: The last sentence is confusing, since the threshold has already been used as a condition for the magnitude of the event.*

Text amended to clarify with reference back to the definition of a weather and climate-related event introduced earlier

 *q. p. 17: "precise value of FAR" --- what does this mean? Last line: "may be just" -> "is"*

Text refers to uncertainty in FAR. Text has been simplified in both places.

 *r. p. 18, para. 2: "can only be justified . . .": all models and our understanding are approximate so absolute standards like this don't make sense. Operational met. and ocean prediction is still of practical value despite flawed models and understanding. Precisely which modelling and understanding gaps prevent us from carrying out attribution of climate events?*

The text has been amended to make the point that we were intending to make, namely that the level of confidence you should have in attribution studies depends on their reliability at capturing processes etc.

 *s. p.19, para.2: "does in fact" -> "is observed to occur", "immaterial" -> "a separate from the question of reliability", "are, and always will be," -> "are intrinsically"*

Text amended.

 *t. p.24, para. 1: "strong ocean" -> "strongly anomalous ocean"*

Done

 *u. p.29, para. 1: this could be cross referenced to the material in section 5.*

Done

 *v. p.29, para. 2: "and as well as" -> "as well as", insert space before "Regions" on last line.*

Done

 *w. p.33, Section 8.2: Title and first sentence could be reworded using standard terminology.*

Done

 *x. p. 34, Section 8.3: The text is very general.*

The text has undergone major revision and shortening to avoid excessive duplication.

 *y. p. 35, Section 8.5: This area was not discussed in the text; it would be helpful if a concrete example of lack of observational records being a problem for event attribution could be supplied.*

A concrete example has been added.

 *z. p.36, l.-3: "Many extremes . . .processes" -> "The climate extremes that have been the focus of this chapter typically reflect regional climate controls as well as remote linkages (teleconnections) to global climate."*

Text amended.

 *aa, p.39, last para.: "manage expectations" -> "have realistic expectations", "sufficient well" -> "sufficiently well"*

Amended as suggested.

 *bb, p. 49, Fig. 6 is hard to see.*

Figure has been replaced with one with fewer panels.

**Reviewer B**

Review of Stott et al: Attribution of Weather and Climate-Related extreme events

*This is a very nice and extremely well written piece summarizing the status of event attribution and the challenges ahead. I have a few suggestions which I think may improve the paper further. I recommend publications after the authors have considered my suggestions.*

We are grateful for the positive comments on the paper and the suggestions for improvement which we have taken on board as detailed below.

1. *While the paper is very well written, there are some segments that are a bit drawn out, and there is some duplication. For example, the summary reiterates things said just right before in recommendations. Also some of the framing sessions early on appear twice with similar words, particularly with the discussion of rapid response vs slow robust response, this seems a recurring topic early on and I think could be brought together and tightened.*

We have merged the last 2 sections in order to avoid repetition in section 8, a point also made by Reviewer A.

1. *Not all sections sound like all authors have read them (some minor contradictions/shifts in balance), e.g. on the Russian heat wave. For a community paper such as this, it would be great to consolidate views. Also, a good proof reading for typos would be good.*

We have amended the text on the Russian heatwave to incorporate additionally new literature since we submitted the paper which reconciles the results of Dole et al and Rahmstorf and Coumou (Otto et al, 2012) and checked for typos.

1. *One omission in my view is mentioning analogue methods, such as Cattiaux et al. I think it would be good to relate to those as well, as while they can never span out the statistics as well as modelling, they may be very nice for cross validation.*

We have added an additional short section to describe analogue methods such as Cattiaux et al.

1. *In some places the paper is thin on references. As I am signing my review I am also, but not only, suggesting some of my own papers :) It is probably also worth discussing Kevin Trenberth’s point of view which I think came out in climatic change recently.*

We have now included appropriate references to more fully support statements made in the paper and these references include by Trenberth and the Hegerl and Zwiers paper.

1. *In some places the paper makes excellent connections between predictability and model evaluation. This could pull through a bit more, in other places in the paper the same topic is treated a big vaguely. Also, unclear in my mind: does it evaluate the modelling framework when they can with some lead time predict the magnitude of the event? Conversely, when these events get predicted but tend to be not of the right magnitude, is that information on bias?*

In tightening up the text we have attempted to clarify the discussion of predictability, in particular making clear what type of predictability we are talking about and that information from hindcasts of this sort can help to improve models.

1. *A common comment on the ability to simulate things such as the European or Russian heat wave is that the models dont block correctly. This topic I think is quite relevant here. I am looking at this as a non-synoptic expert, and find it confusing – can you shed some light by addressing this issue head on?*

Blocking is not the only phenomenon that can challenge models and hence limit our abilities in attribution. Other examples include ENSO, the Madden-Julian Oscillation and more broadly tropical convection, and land surface feedbacks for most extreme summer heat waves. Under-representation of such phenomena can lead to low sensitivity (false negatives or failure to attribute if these are important factors). Conversely, models that over-amplify such phenomena or processes can have the opposite effect, with over-attribution to specific causes (false positives). For example, a model that produces excessive land surface drying in a region may produce spurious heat waves as well. Adequate representations of forced responses and internal variability are therefore both essential. There is a need to state limitations of models, but this is also an area where attribution studies can be very valuable in suggesting needs for model improvements.

A short discussion has been added to section 3 of the revised paper (Development of the science of event attribution).

1. *I think it might be useful to discuss somewhere correctable biases (eg as done in Otto et al for surface temperatures being biased high given blocks) vs things that are harder to correct, such as a tendency for models to not, or not often enough, produce particular synoptic situations*

Text has been added to section 3 of the revised paper to elucidate better the difference between post-hoc correctable biases and biases that require fundamental improvements to the model (Development of the science of event attribution)

1. *Selection bias creeps up in the discussion now and then. Obviously, using a multi-step approach where attribution includes observed changes impacted by extreme events introduces such biases. However, if your approach is a purely odeling approach based on SSTs for example – I cant really see selection bias being a problem? Can you just clarify your discussion of this problem a bit? This would also link to sections 8.4 and 8.7.*

The main point to make about selection bias is also made in the BAMS assessment of extreme weather events of 2011 (Peterson, Stott, Herring, eds), namely that if you make choices for your thresholds that are selected based on a few specific instances of what occurred and your choice of analysis technique is based on what occurred this could introduce selection bias. This argument is discussed in the concluding section now merged with the preceding one to produce a final drawing together of the position the paper is taking.

*Detailed comments:*

*p. 3: ‘that all weather and climate-related extremes could not have occurred without a human influence – this is hard to understand – do you really mean physically impossible? Or are you paraphrasing Trenberth’s climatic change (I think – well worth referencing!) piece saying the atmosphere is so changed all events have a human contribution? Please clarify*

The text here has been amended (also in response to Reviewer A) and includes ref to Trenberth paper.

*p. 3 ‘prevented from occurring’ that is very strong wording. Do we have any case where that has been shown?*

This sentence has been deleted.

*p. 5: Some of what the paper of Stott et al., 2010 refers to moving beyond analysis of global mean temperature records has also already been said in AR4 – maybe rephrase to clarify this is a movement that has been happening since a while (a very self serving comment: I thought IPCC chapters get cited as chapters, not as entire report unless its spread over several chapters? I find it a bit surprising to be totally circumnavigated in this paper....).*

This section has been deleted but references to Hegerl et al 07 rather than IPCC are now included where appropriate.

*p. 6: Morak et al., J Climate and GRL also show that the frequency has changed. Same page ‘assessments’ typo with 6 s.*

Reference have been included to Morak et al papers (now in introduction section)

*p. 8 end of 2nd paragraph ‘processes’?*

Spelt correctly

*end of page 8: can we really predict to several weeks ahead? The discussion 2nd paragraph page 8, firsts page 9 seems to recur later again, maybe it can be shortened here?*

Text here amended, also in response to Reviewer A comments.

*Page 8 section 3.2: typo in first work. Also, a lot of this section reiterates stuff that was already discussed in the introduction. Can this be all moved to the introduction? Or moved entirely here?*

The text has been tightened up in both introduction and section 3.2, with section 3.2 now being properly referenced, thereby avoiding unnecessary repetition.

*p. 10, bottom paragraph: This again seems to duplicate some things said earlier. I like the end of that section though on p. 11.*

The relevant statement to make which appears only here is that assessments can be made even in the face of remaining uncertainties as long as the confidence in the assessment is properly communicated. This is left here but the text is shortened.

*End of P. 14: Hegerl and Zwiers, WIRES discusses the use of models/need for models/constraints by model uncertainty in detection and attribution*

Text has been amended here, also in response to reviewer A, and a reference to Hegerl and Zwiers has been added.

*p. 16: this is nice, can you add just a sentence or two explaining how the errorbars are derived as well for attribution statements? I noticed getting wobbly when explaining this to people so it would be good to explain at this opportunity – eg is it based on analytic estimates or sampling the fraction from the histograms?*

We have inserted an additional sentence to explain this later in the paper when discussing Figure 2.

*p. 17, end of firsts paragraph: I think this would be an excellent opportunity to introduce and discuss the question about blocking. – or maybe on p. 18...*

Text has been added referring to the issue of blocking as an example of a model inadequacy that needs to be assessed for its impact on attribution statements.

*p. 17 middle: is it a good idea to use ‘very likely’ for estimates coming directly from statistical analyses? The usual IPCC assessments are expert assessments filtering those studies....*

This was the wording used in the original paper.

*p. 19: Predictability of the Russian Heat Wave: Julia Slingo on a visit showed us Met Office predictions of the Russian Heat wave I think in the seasonal predictions. Does this agree with the statements on top? Or are those based on a particular forecasting system?*

That was the conclusion of Dole et al. Statement clarified to make clear the basis of this conclusion based on the inability of models looked at by Dole et al to predict an increased probability one month or more in advance.

*p. 19 bottom: I like the discussion of predictability vs reliability: its an excellent point. This paragraph could use some sharpening though I think saying this more succingctly. It would also be good to discuss this topic some more, eg in the situation shown for rainfall in the reliability diagram, is there any way around this or does this mean one needs to wait for a better model?*

Thanks. We have amended the text to sharpen it up a bit and inserted the crucial point about reliability.

*end of page 20: not really sure if using a different threshold would really introduce selection bias as you are looking at frequency of events in models anyway?*

This statement is taken from the paper.

*p. 23 end of first paragraph: this is nice and very interesting. For a reader of this paper only it might be a bit hard to digest the green bars and why there are so many more green than blue dots. Maybe you can say a bit more about the modeling setup and tracing the SST uncertainty.*

The text has been amended to more directly refer to the figure and therefore explain more directly where the uncertainty in the green diamonds comes from.

*End of d1rsts paragraph on page 24: the conclusion that this is short term is only if you are sure that internannual, not interdecadal variability is responsible – it seems reasonable given its ENSO but do you know that for sure (eg that the AMIP runs have this pattern only due to ENSO)? Maybe more can be said here*

Good point but given space considerations we are reluctant to elaborate too much here. Text has been amended to make clear that this was what Perlwitz et al concluded.

*End of p. 24: ‘not experiented signficant long-term warming: This is treated differently in Otto et al which shows that a linear warming trend is not a good match. Also, Christidis et al (all authors on this paper!) has a study on seasonal attributable warming – does that extend to Russia? Did anybody publish a prediction for summer temperature changes – I have on in my 2011 nature geoscience paper in the supplement where one single model (HadCM3) has summer temperatures going up only at the end of the 20th /early 21rst century and continuing to show trends well into the 21rst, very much in agreement with observations and indicating that a long-term trend there would be small but the short term trends and future trends may not be – my paper isnt a good quote for this but maybe somebody else has shown this nicer?)*

We do point out at the end of the paragraph studies like that of Jones et al showing a rapid increase in the frequency of extremes consistent with what Dole et al said about Russia being on the cusp of a period in which the probability of heatwaves will increase rapidly. Also this section has been revised to bring in the Rahmstorf and Coumou and Otto et al studies and explicit reference is made in the revisions to the trends in more recent decades seen in Rahmstorf and Coumou.

*P, 25: ‘very unlikely that increasing greenhouse gases contributed’ – is that IPCC language? If yes what does ‘substantially’ mean? As there is attributable warming on the continent, it would be hard to discount ‘a’ contribution just its size is hard to pin down and may be small compared to the anomaly. Again discussed differently in Otto et al... can you consolidate?*

Discussion of Otto et al (which was not available to us when the paper was submitted) has been added and the language here modified to avoid IPCC language.

*p. 27: I read with interest about the coordinated experiment at the end of the page- is there a timeline and a status update? (oct 2010 is a while ago!)*

This will be discussed further at the ACE meeting in September, 2012.

*p. 31 top and paragraph leading up to it: this is interesting but quite dry and theoretical. Can you give examples or quotes?*

We do not have cited literature to quote from here.

*p. 34 top: the point about clarity is important and an excellent one to make.*

Thanks

*p. 35 recommendation 8.5: long-term observations can also evaluate the models’ ability to get the right statistics of rare events – thus even long-term observations are very important.*

Agreed and we say this.

*Section 8.6: could use some cites about importance of land surface processes, eg one of Semeviratne’;s papers or a Fischer paper (or Schaer ete al., 2003)*

Done.

*p. 37 last line: Do all authors agree that there was no substantial human influence on the magnitude of the 2010 Russian heat wave? Maybe for balance add a statement on frequency?*

This sentence has been reworded to reflect a more consensus statement. But note that we are using this as an example where much of the *magnitude* is attributable to human influence.

**Reviewer C:** David Karoly (signed and attached)

We thank the reviewer for their positive comments about our paper.

The point about repetition is well taken and we have made some major revisions to avoid duplication and inconsistencies in the text including to the introduction, section 3.2 and the discussion. As part of these revisions we have deleted section 2 and merged and shortened the last 2 sections.

The point about the discussion about what constitutes agreement between model simulations and an observed event is also well taken. We have made some revisions to text in 5.3 (numbering in submitted paper) and in section 8.7 to try to clarify the text on this point.

Detailed comments :

Page 3 line 3 : Text revised

Page 4 2nd para, 2nd sentence : This sentence has been deleted as part of shortening and improving the text

Page 7 line -7 : Text revised

Section 3.5 : Text revised to take account of these comments and to fully reflect the point that some people may be wanting to sue others for geoengineering interventions.

Section 5.1 : Text revised to make this point.

Page 27 line 5 : Text revised to take this into account

Page 29 line -6 : Done

Page 35 Section 8.5 : Text revised to make this point with inclusion of supporting reference.

Section 8.7 Last sentence : Text revised.

Fig 7 : We think the reviewer meant fig 6 not fig 7 (since the latter is not in color). We have simplified fig 6 by reducing the no of panels (also in response to another reviewer) which makes it a lot clearer.

**Reviewer D:** Arun Kumar (signed and attached)

We thank reviewer D for their interest in our paper and their comments.

1st Comment on abstract : We have deleted this sentence

2nd comment : incorporated thank you

Comment top page 3 : This is a good call : we have clarified that we are talking about climate predictability, ie the prospects for early warning a month or more in advance – what we didn’t have for the Moscow heatwave for example.

Comment half way down page 3: We have inserted the “risk of” to reflect the fact that this will be a probabilistic statement – decision makers need to know about enhanced risk

Page 5 comment : This text has been deleted in revision in response to other reviewers’ comments.

Page 6 ¾ way down comment : We have added the clarification at the start of the paper on what is meant by “early warning”

Page 8 near top : Inserted “statistics of occurrence”

Page 8 last para : The sense meant is whether the model has reliability in capturing the real world predictability, ie whether when there is a probability of 0.2 of something happens it happens in those cases 20% of the time. Text has has minor revisions to clarify this discussion.

Page 11 : there is already discussion around attribution being used in litigation eg David Adam’s article in Nature. Regardless of whether one supports its use or not it seems wrong not to consider it so that at least if/when attribution information is used some of the issues (eg see Allen et al, 2007) have been discussed and expert scientific evidence can be presented in a useful fashion.

Page 12 bottom : “vulnerability” added

Page 13 Top : sentence deleted

Page 13 5 lines down : Keep as it has a common sense interpretation

Page 18 middle of page comment : We have clarified what we mean by predictability – ie from conditions internal to the climate system. Like you say external forcings can affect things but predictability from forcings is not what is meant here.

Page 18 : Has been clarified

Page 19: lead time clarified. We stick to seasonal forecasts because that is the type of predictability we are interested in as clarified earlier; “immaterial” word replaced and sentence revised.

Page 26 : type corrected

Page 27 top : We do mean “operational” in the same sense as “operational” seasonal forecast systems. We discuss the benefits of regular timely attribution assessments using pre-defined methodologies (ie given that implies the delivery of output on a particular timescale, this is operational) in the revised paper.

Page 27 bottom : C20C reference included.

Page 30 bottom : text revised to be clearer

Page 32 top : citation added

Page 35 : climate quality reanalyses