Anthony Kiem
Hydroclimatologist/Lecturer
Environmental and Climate Change Research Group
School of Environmental and Life Sciences
Faculty of Science and Information Technology
University of Newcastle
Callaghan NSW 2308 Australia
T: +61 2 4921 8656 F: +61 2 4921 6925
Email: Anthony.Kiem@newcastle.edu.au

Friday 22nd January 2010

Dear Dr. Bart van den Hurk (Handling Editor for this paper)

Thank you for providing the opportunity to address the comments made by the two anonymous reviewers on the paper "Towards understanding hydroclimatic change in Victoria, Australia – why was the last decade so dry?" (Manuscript #: hess-2009-211).

Our responses to the Reviewer's comments are included below along with details indicating how the paper has been revised. Both Reviewers acknowledge that the paper is interesting, however, it seems there is some concern as to what new contributions this paper provides. The major scientific contribution of this paper is the relating of regional scale synoptic drivers of SEA rainfall with large-scale climate phenomena and the demonstration that even though seasonal rainfall totals do not seem to completely explain the reduction in observed runoff, the changes to the daily distribution (caused by changes to the synoptic patterns) actually act to enhance the reduction in runoff. The implications of these results are an improved understanding of the non-linear rainfall-runoff process and the importance of taking into account antecedent conditions and the synoptic patterns that actually deliver the rainfall when investigating hydrology in this region. This paper also highlights the need for collaboration between climate scientists and hydrologists. Several studies exist on the issue addressed here but typically the work is performed by either hydrology focused research groups or climate science focused groups. What is needed to advance our understanding is more research, such as that presented here, which investigates the links between hydrology and climatology.

The second main criticism is that this paper is not "detailed" or "in-depth" enough. We acknowledge the work is preliminary and have changed the paper's title accordingly. We also acknowledge, and point out in the conclusions, that there is a lot more research to do on this issue. However, the fact that this is a preliminary investigation should not prevent it from being published in HESS. The results, conclusions and suggestions for future research are all significant contributions and represent a step towards improved understanding into hydroclimatic change (both in Victoria and elsewhere).

If you require any further information please contact me.

Thank you

Anthony Kiem

Anonymous Referee #1

1-1. General Comments: This paper investigates large scale drivers for Victoria, how they modulate synoptic patterns and in turn seasonal rainfall and runoff. It does so by a combination of literature review (in particular the authors prior work) and some new analysis. Unfortunately, in my opinion, the combination is muddled. Because the results and discussion are not separated, it is unclear to me what aspects are new. I am left with the impression that the majority of data and findings have already been published elsewhere by the authors, with the exception of some new analysis that appears to be inconclusive.

Author Response: The data used in this paper, and associated Fig 1 and Table 1, have been used in some of our other publications, however these papers have been focused on different aspects of hydroclimatology in Victoria. Some of the results presented in this paper have been presented orally at a conference in Dec 2009 and a workshop (at the Victorian Department of Sustainability and the Environment (DSE)). Therefore, while the Reviewer may have 'seen' the results and conclusions before they have not actually been published in any journal paper. Regarding the *'new analysis that appears inconclusive'*, it is assumed that this refers to Fig 7 and associated text and this comment is addressed on the first page of this letter (and also in our response to comment 2-7).

1-2. General Comments (cont): A review paper might still be publishable but for that to happen, the substantial body of literature (both peer-reviewed reports and journal articles) needs to be fully reviewed and new insights would need to result. I believe neither of these are currently achieved.

Author Response: This paper presents a useful review of recent work relating to hydroclimatic variability/change in Victoria (e.g. SEACI & MDBSY publications, papers surrounding the debate on the increasing temperature => increasing evaporation hypothesis, work on relationships between ENSO, IOD, SAM and SEA rainfall etc). In addition, this paper also presents significant new insights and suggestions that are of value to the hydrology-climatology research community (see responses to comments below for details). After revisions based on reviewer's comments it is hoped that the new insights are now clearer and the paper is now publishable.

1-3. General Comments (cont): I recommend the authors to reconsider what aspects of the analysis or synthesis in this manuscript are novel, and – if they are sufficient - use this as the basis for a new manuscript.

Author Response: We have done this. See below for details.

1-4. General Comments (cont): If they feel a comprehensive review is timely than they should take note of the sizeable amount of analysis contained in the reports produced by the SEACI initiative (of which they are aware) and the Murray-Darling Basin Sustainable Yields project (www.csiro.au/partnerships/MDBSY; which already reported on trends and projections for seasonal rainfall and rainfall-runoff relationships in northern Victoria) as well as related journal publications by Bertrand Timbal, Francis Chiew, and others scientists involved in these.

Author Response: We acknowledge and agree with this comment. References to the MDBSY work (e.g. CSIRO, 2008; Potter et al, 2008) have been added at relevant points throughout the revised paper. Several references to SEACI publications were already included in the original paper, however, the detail associated with these references has now been increased and text summarising the work has been added. References relating to southward movement and increased intensification of STR and its impact on SEA rainfall have also been added (e.g. Drosdowsky, 2005; Timbal et al., 2007; Larsen and Nicholls, 2009; Williams and Stone, 2009). We have carried out an ISI Web of Science search for journal articles by Francis Chiew and, apart from the ones already included that Francis is a co-author on, we could not find any relevant to this paper – if there is some we have missed that should be included please direct us to them.

1-5. Abstract:
- *"Lowest on record" – mention length of record*
- *"While severe decreases" – subjective, delete 'severe'*
- *"which takes into account insights into.." –> "that takes into account.."*

Author Response: Revisions made as requested.

1-6. Section 1: "extremely low" – compared to what? Be specific.

Author Response: Sentence revised to clarify that by *"extremely low streamflow"* we mean several years of below average flow have occurred since the mid-1990s.

1-7. Section 1: (~60%) -> (~60% of the total decline)

Author Response: Sentence revised to read "*Many studies (e.g. Murphy and Timbal, 2008; Pook et al., 2008; Cai and Cowan, 2008a, 2008b) have pointed out that the majority (~60%) of the total SEA annual rainfall decline is due to drier autumns (March-May…."*

1-8. Section 1: "drier autumns" This has been well published – refer to SEACI and MDBSY reports.

Author Response: We acknowledge that the finding that the majority of the SEA annual rainfall decline is due to drier autumns is well published. The original paper referenced several papers which show this (including Murphy and Timbal (2008) who provide a comprehensive review and cite several further references). SEACI publications were also cited in Section 3 of this paper. However, we agree that both SEACI and MDBSY work should be cited in Section 1 and have revised the paper accordingly.

1-9. Section 1: 'rest of the year' -> mainly 'following winter' I would have thought?

Author Response: Yes mainly following winter is true…but if you don't get the autumn break then the chance of below average winter flow is increased which in turn increases the chance of below average spring flow and so on throughout the rest of the year.

1-10. Section 1: 'is not totally explained by' -> Pls review what other factors they suggested or speculated on. See also SEACI and MDBSY reports on this.

Author Response: Cai and Cowan (2008b) analyse the impact of two factors (reduction in seasonal rainfall totals and increases to seasonal temperatures) and conclude that *"a rainfall reduction alone is unable to explain the observed inflow reduction trend, and that there is a contribution from rising temperatures*". No other factors are analysed or discussed by Cai and Cowan. References to MDBSY reports, SEACI and Murphy and Timbal (2008) have now been included – these publications contain comprehensive reviews on this topic. Refer also to paragraph 2 in Sect 5 where other factors that could explain the excessive decrease in runoff are listed and other publications relating to this are cited.

1-11. Section 2.1: 'historical records .. representative of 'natural' streamflow': (1) define natural here or elsewhere (2) further on you mention the Goulburn d/s Eildon. How is this natural?

Author Response: The following sentence has been added: *"...(i.e. observed streamflow with minimal upstream diversion or regulation or modelled streamflow where upstream diversions have been quantified and re-added to 'naturalise' the flow data)…".*

Flow at Goulburn is "naturalised" data as discussed in the following point.

1-12. Section 2.1: 'except Goulburn and Yarra. . .REALM' – Does this mean you used modeled rather than observed data? Use of modeled data would be unacceptable in a study like this.

Author Response: Yes the flow data for Goulburn and Yarra is modelled or 'naturalised' data (as discussed in Section 2.2). That is, REALM was used to convert observed (i.e.

human impacted) flow at the Goulburn and Yarra sites into 'natural' flow by calculating, and re-adding, extractions due to reservoir operations, farm dams, water allocations etc. Since the observed flow at these two sites is so heavily impacted by human activities it is not possible to use observed flow data for any meaningful climate impact analysis as any impact due to climate variability/change is significantly outweighed (and clouded) by the impact of extractions upstream. For the other 7 sites the observed flow was considered to realistically represent natural flow (i.e. minimal extractions upstream) and hence modelling or naturalisation was only required at Goulburn and Yarra.

While Reviewer #1 is correct in saying that modelled streamflow data is not ideal in a study such as this, it was decided to include the 2 modelled flow sites (along with the caveats that it is modelled data (see Section 2.2)) rather than leave out the Goulburn and Yarra sites and have zero information about two key Victorian catchments. Furthemore, the REALM simulations of 'naturalised' Goulburn and Yarra flows have been rigorously checked and similar data has been utilised in many previous studies (e.g. Cai and Cowan, 2008b; MDBSY project etc).

1-13. Section 2.2: 'infilled'- > 'gapfilled' is the more common term?

Author Response: 'Gapfilled' is not a common term for the authors when referring to replacing missing flow data with some calculated value. In our experience 'infilling' is the more common term. Since the two words mean essentially the same thing we request guidance from the Editorial Staff as to which term is more commonly used in HESS?

1-14. Section 2.3: pls provide a table with gauge codes and indicate their location in the map pls

Author Response: A table has been inserted as requested. The location of the stations (indicated by the latitude and longitude in the new table) is very close to the streamflow gauges already shown in Fig 1 is left as is for the sake of clarity.

1-15. Section 3: 'As mentioned. . .'- > indeed already mentioned, I still remember. Pls delete

Author Response: 1st sentence of Section 3 deleted.

1-16. Section 3: 'long term average'-> list years pls

Author Response: Long term average years have been included in the revised paper (1920-2006).

1-17. Section 3: '(only two shown here)'- > delete

Author Response: Deleted.

1-18. Section 3: 'elevated and suppressed' -> rephrase?

Author Response: Changed to *"…above or below average rainfall…"*

1-19. Section 3: 'far east changes' - > can this be connected to the earlier climate shift further west in WA?

Author Response: Possibly. This is the subject of recent and continuing research we (e.g. Verdon-Kidd & Kiem, 2009) and others (e.g. SEACI researchers, Matt England & co at UNSW etc) are carrying out. This question also emphasises the point we make at the end of Sect 4.2 that *"understanding into impacts associated with large-scale climate drivers (and their interactions) is in its infancy"*. This question also confirms that this work is novel, interesting and represents a step along the way into improving our understanding into the hydrological implications of climate variability and or change.

1-20. Section 3: 'the 'post-1997 climate shift' initiated' Why the '' ? What is the significance of these 4 words? Probably requires a reference to an earlier use or an explanation as to why it is a commonly used concept.

Author Response: This sentence has been revised to include references and clarification as to the significance of these four words. Our point is that the current SEA drought is commonly referred to as the 'post-1997 climate shift' when in fact the rainfall statistics suggest that the shift occurred a few years earlier (i.e. 1994) for the majority of Victoria (as represented by our 9 study sites which are assumed to give an acceptable statewide coverage).

1-21. Section 3: 'Importantly,. . .Fig 2,' -> unnecessary, delete.

Author Response: Deleted as suggested.

1-22. Section 3: '~1935 switch to dry'- > not very readable - suggest rephrase as 'drier conditions around to 1935'and so on. How about colour coding wet and dry epochs in the figures?

Author Response: The text has been revised as suggested. Fig 2 already includes red lines which indicate the dry epochs beginning in the mid-1930s and mid-1990s.

1-23. Section 3: 'the Federation drought'- > insert 'so-called'

Author Response: Revision made as requested.

1-24. Section 3.1: 'Importantly, not only is there'-> 'There is'

Author Response: This is an important finding and one of the key contributions of this paper. That is, as previously discussed (in this paper and many others) there is an obvious decrease in mean (or median) autumn rainfall since the mid-1990s. Fig 3 demonstrates that one reason for this is because of the lack of extremely wet seasons in the 1994-2007 period. This is important in its own right, but is particularly important when considering the hydrological implications of such a significant change in seasonal rainfall distributions – hence the marked reductions in runoff since the mid-1990s (as further discussed in Section 5).

1-25. Section 3.1: 'extreme rainfall events'- > or do you mean extreme seasonal rainfall? Not the same.

Author Response: Acknowledged and revised accordingly throughout the paper.

1-26. Section 3.1: Fig 4b and associated text - > I am getting confused here. The source & processing needs to be explained here. If it is from another publication, it needs proper referencing in text and in figure and should be removed from what appear to be results and brought to either introduction or discussion. If it is new material, the methods need to be described properly.

Author Response: The source for Fig. 4a and Fig. 4b is cited in the Fig 4 caption (i.e. NCEP/NCAR Reanalysis Data, http://www.esrl.noaa.gov). There was no data processing or manipulation performed other than to use the Graphical Representation tools available on the website indicated and therefore there is no method to describe. Fig 4a and 4b are just direct graphical representations of the widely used NCEP/NCAR Reanalysis Data. Fig 4a and Fig 4b have never been published before and are included because it is possible that the post-1993 reduction in autumn observed at our 9 study sites may have been site specific. Fig 4a demonstrates that the post-1993 autumn rainfall reduction does in fact extend across the whole SEA region and Fig 4b confirms that, as expected, this is largely due to persistent high pressure conditions (i.e. dry synoptic types).

1-27. Section 3.1: 'This is consistent. . .rain?" -> delete, adds nothing.

Author Response: Deleted.

1-28. Section 3.1: 'There is limited understanding. . .. (e.g. Cai and Cowan, 2008b)' The authors fail to review a sizeable number of analyses that have been published about this (certainly the reference given is not the best either). Please review journal and report literature: SEACI, MDBSY and derived papers by Chiew and others.

Author Response: References to MDBSY reports, SEACI and Murphy and Timbal (2008) have now been included – these publications contain comprehensive reviews on this topic. Refer also to paragraph 2 in Sect 5 where the other factors that could explain the excessive decrease in runoff are listed and other publications relating to this are cited. Journal papers by Chiew on this issue could not be found – please direct us to them and we will include.

1-29. Section 4: 'Vernon-Kidd and Kiem (2009) identified 20 . . .. Climate of Victoria' OK so this is already published. Why does this need to be repeated then?

Author Response: The reader is referred to Verdon-Kidd and Kiem (2009) for:

(a) explanation of the SOM methodology;
(b) details on how SOM was used to identify 20 key synoptic types for Victoria;
(c) evidence to back up the claim that the SOM technique adequately represents the key synoptic patterns that are important influences on Vic climate

This paper is an extension of the work of Verdon-Kidd and Kiem (2009) and provides an analysis of the frequency of these key synoptic types pre- and post-1993 and during different phases of ENSO, IOD, SAM. Therefore, some background on how these synoptic types were derived is required. As Reviewer #1 points out, the derivation of key synoptic types is not new work (and we do not claim that it is) and therefore rather than going into detail in this paper we refer the reader to Verdon-Kidd and Kiem (2009) and the references therein. Also, the SOM notation used in Fig 5, Fig 7b, Fig 7c, and Table 3 (e.g. type 3D, 4D, 5A etc) is meaningless without the background information provided in Verdon-Kidd and Kiem (2009).

In saying that, we acknowledge that the number of references to Verdon-Kidd and Kiem (2009) is excessive and have removed *"(refer to Fig. 3 and associated discussion in Verdon-Kidd and Kiem, 2009)*" and *"(refer to Fig. 5 in Verdon-Kidd and Kiem, 2009a)".*

1-30. Section 4: 'consistent with. . .SEACI'. Indeed, which raises the question as to what is novel about these results.

Author Response: The analysis referred to here is important given that, via an entirely different method and independent of SEACI research, we have come up with the same conclusion – therefore increasing confidence in our understanding into the drivers of SEA climate.

1-31. Section 4: 'rain producing troughs' I am very surprised that the work of Timbal and others is not referred to here.

Author Response: References relating to southward movement and increased intensification of STR and its impact on SEA rainfall have now been added (e.g. Drosdowsky, 2005; Timbal et al., 2007; Larsen and Nicholls, 2009; Williams and Stone, 2009).

1-32. Section 4.2: 'Refer to Kiem and Verdon-Kidd (2009)' OK, so given that it is already published, why does it need to be repeated here? This reference is repeated several times, which suggests to me that there is little point to include it all again here.

Author Response: The references to Kiem and Verdon-Kidd (2009) were included for the same reasons given in our response to 1-29 (i.e. to give the reader some background on SOM and the 20 key synoptic types that were derived). However, we acknowledge there is some overkill here and have removed the references to Kiem and Verdon-Kidd (2009) from Section 4.2.

The results presented in Table 3, and discussed in Section 4.2, are an extension of those presented in Kiem and Verdon-Kidd (2009) because they:

- concentrate solely on Autumn;
- indicate which synoptic types are typically "wet" and which are "dry" for Victoria;
- indicate exceptions to the expected "wet" or "dry" response at each of our 9 study sites.

It is necessary for Table 3 results to be presented and discussed here because they lead into one of the two main contributions of this paper (i.e. that dry Vic conditions since mid-1990 are due to a decrease in autumn rain which in turn is due to a decrease/increase in wet/dry synoptic patterns which in turn is due to abnormally high SLP across SEA which in turn is due to persistently positive SAM and lack of La Niña during autumn).

1-33. Section 5: "However, the rainfall-runoff . . . underestimated" There are several flaws with the Cai and Cowan analysis and indeed this is one of them. However, again this is already addressed in various SEACI and MDBSY publications.

Author Response: Acknowledged and agreed. Reference to the MDBSY report has been added. However, apart from that report we could not find a journal paper (or widely available report) which points out the flaws in the work of Cai and Cowan (2008b). We feel that it is important to highlight some of the limitations associated with this work as it is known to be flawed yet has been (and continues to be) widely cited and accepted by many researchers and industry stakeholders. If there are other publications that also address this issue then we would appreciate it if you could direct us to them.

1-34. Section 5: 'Fig 7a shows that. . .' -> here there seem to be some new findings, but unfortunately they appear open ended.

Author Response: See response to comment 2-7.

1-35. Section 6: All three conclusions are already well and truly out there in the peer-reviewed as well as grey but electronic literature. Refer Bureau of Meteorology, DSE, SEACI, MDBSY, papers and reports by Timbal, Chiew etc. The next statement about the changed seasonal distribution is not novel but more interesting, however probably rightly not included as a conclusion here because the analysis was inconclusive.

Author Response: It is true that "some" of the results presented here have been presented previously at various workshops over the last 12 months and also may be available in various sources of grey literature (e.g. reports for DSE, BOM newsletters etc). However, we feel our results and conclusions (along with the issues raised) warrant publication in a peer-reviewed international journal as the implications extend beyond Australian researchers and industry stakeholders (i.e. these are the only people who read the "grey" literature referred to). In addition, we disagree that the conclusions are "well and truly out there" and stress that these findings are new, the issues raised are significant, the hypothesis as to why the last decade was so dry is valid, and the criticism of Cai and Cowan's previous work attempting to explain the excessive decrease in runoff is warranted and requires publication so that Cai and Cowan's work can be reviewed and/or clarified (as per suggestions also recently made by Lockart et al., 2009). Refer also to our response to 2-1 for further details as to why we disagree with the statement that this work is "well and truly out there".

1-36. Table 1: last date is 2006 – not very up to date. This info should be available to a much more recent date.

Author Response: Acknowledged. However, we were restricted by the unavailability of 'natural' flow data post 2006. Given that since 2006 the SAM during Autumn has remained mostly positive and that the 'autumn break' has not occurred we are confident that incorporating data up to end-2009 would not change the results or conclusions (in fact it would strengthen our case).

1-37. Figure 2: not very clear. In particular, please indicate missing years.

Author Response: The quality of Fig. 2 has been improved. Missing years are indicated by the lack of a bar.

1-38. Figure 3: please increase detail in vertical direction (by changing axes or figure size)

Author Response: This has been done.

Anonymous Referee #2

2-1. Review: "Towards understanding hydroclimatic change in Victoria, Australia-why was the last decade is dry?" by A.S. Kiem and D.C. Verdon-Kidd. In this study the authors investigate the characteristics of the most recent changes in Victorian rainfall and stream flow. Although some interesting results are presented the article fails to provide a substantial contribution towards the understanding why the last decade was so dry. Some of the conclusions are not really new and others are not supported by the data. In addition the paper is not very well focused and the different topics are not investigated in depth. I will discuss these issues more in detail below. My conclusion is that it cannot be published in its present form.

Author Response: We have addressed Reviewer #2's comments and, where necessary, have revised the paper. We agree with Reviewer #2 that these results are interesting but do not understand why Reviewer #2 does not think they are worth publishing in HESS. Contrary to Reviewer #2's opinion, the 3 concluding points are new:

1) we demonstrate that the step change in annual Vic rainfall occurred ~1994 (not ~1997 as is commonly reported) and demonstrated via rigorous statistical tests that similar multidecadal regime shifts have occurred previously in SEA history This is an important finding given the large number of recent claims that the "post-1997" drought is "unprecedented". The drought began in 1994 and in terms of rainfall deficits it is not unprecedented – this is a significant finding that is contrary to what is commonly accepted and therefore should be published.
2) We demonstrate, as others have, that the majority of the annual rainfall decrease in the Big Dry is due to reduction in autumn rain. We extend the previous studies, and the work contained in Verdon-Kidd and Kiem (2009a) by demonstrating (via the SOM technique) that the post-1993 autumn rainfall decline is due to changes in the frequency and seasonality of synoptic patterns.
3) We extend the work further by, for the first time, linking the changes in frequency and timing of synoptic types to the **combined** impact of ENSO and SAM.

We then present a hypothesis, backed up by some innovative and novel analysis (Fig 7c), as to why the decrease in SEA runoff does not seem to be explained by the decrease in SEA rainfall. We do not claim that this is the only answer (see Sect 5), and acknowledge there is more work to do, but feel this paper (Fig 7c and the related discussion) should be published in a hydrological journal such as HESS to emphasise the need for robust hydrological modelling to be performed if we are truly to get to the bottom of why the decrease in runoff is not explained by the decrease in rainfall. Simple linear rainfall-runoff regression type analysis (e.g. Cai and Cowan, 2008b) is clearly inadequate, and prone to false conclusions and misunderstandings, given the numerous possible causal factors (and currently poorly understood complex non-linear interactions associated with the impacts of large-scale climate drivers and the rainfall-runoff process) associated with

the Big Dry (and other SEA droughts). The hydrology-climatology research community needs to be made aware of this and review previous studies into causes of SEA drought accordingly.

2-2. They argue that the recent reduction in the mid 1990's in Victorian rainfall is not unusual and has happened before. However, according their table 2 only during the mid 1990's there was a significant step towards drying for all sites. For the other dry periods there was much more variation among the different sites. This is not discussed.

Author Response: The reviewer appears to have misunderstood Table 2. The main purpose of Table 2 was to demonstrate that, based on statistical analysis of annual rainfall time series, the most recent drought began ~1994 as opposed to ~1997 as is commonly reported. A secondary result emerging from Table 2 is that other statistically significant step changes (wet to dry or dry to wet) have also occurred. As noted by Reviewer 2, Table 2 indicates significant steps towards a dry epoch across Vic in the mid 1990s (9 out of 9 sites for which there was data) but Table 2 also shows a shift to dry in the mid-1930s (3 out of 4 sites for which there was data) and, for part of the state, in the mid-1970s (2 out of 9 sites). The reason why the mid-1990s shift to dry stands out in Table 2 is because there is no data for the period corresponding to the Federation (~1895-1902) drought and only 4 sites had data for the WWII (~1937-1945) drought. Numerous studies exist (e.g. Watkins, 2005, CSIRO, 2008; Murphy and Timbal, 2008; Potter et al., 2008; Verdon-Kidd and Kiem, 2009b) which demonstrate that the WWII and Federation droughts were as bad or worse, with respect to rainfall deficits, than the recent mid-1990s to present drought (Big Dry). Table 2, at least for WWII drought, supports these previous studies. We have altered the text in Section 3 relating to Table 2 to clarify this. See also our response to 2-4.

2-3. In addition they argue that SAM index is one of the drivers of the rainfall fluctuations. Fig. 6 shows a large trend of the SAM from 1950 towards the end, being the main signal. This is not discussed.

Author Response: We agree that, if you do a linear regression, Fig 6 indicates a +ve trend in autumn SAM since 1950. The inference would be that if this trend is 'real' (e.g. not spurious due to limited record length) and were to continue (i.e. continued +ve SAM during autumn) then dry autumns would also continue (unless the +ve SAM effect is cancelled out by an autumn La Niña). However, it must be considered that this timeseries is very short and indeed longer timeseries of the SAM show the "trend" since 1950 may just be part of a multidecadal cycle (overlayed on the higher frequency changes). The apparent positive trend in the SAM since 1950 is actually enhanced by the fact that the timeseries starts during a period of negative SAM and ends during a period of positive SAM (see Fig 3 in Verdon-Kidd and Kiem, 2009b for further details). In any case, in this study we only concentrate on the Big Dry (i.e. mid-1990 to present) drought and feel that linear trend in autumn SAM since 1950 is irrelevant. The important fact is that autumn SAM has been persistently positive since 1994 unlike conditions prior to 1994 where

SAM alternated between positive and negative phases on a much more regular/higher frequency basis (overlayed on the longer term cycle). We include detailed discussion of the implications of persistently positive autumn SAM in Sect 4.2 and Conclusions (and also in the referenced paper Kiem and Verdon-Kidd, 2009).

2-4. *Thus although the authors claim that the 1990's are not unusual, their own analysis point towards a different direction. Any possible connection with the recent global warming is ignored. This hypothesis should be tested much better before it can rejected. It might well be that the trend in the SAM is related to global warming.*

Author Response: Refer firstly to our response to 2-2. Also, we are not saying that the Big Dry drought is not unusual (all droughts are different as we demonstrated recently in Verdon-Kidd and Kiem (2009b). With respect to the spatial signature and degree of reduction in autumn runoff the Big Dry is totally different to previous iconic droughts affecting SEA (i.e. WWII and Federation). However, what we are saying is that in terms of rainfall deficits and/or significant shifts towards a dry epoch the most recent drought is not unprecedented in SEA. This claim is well supported by the results presented in this paper (e.g. Fig 2, Table 2) and many previous studies (e.g. Watkins, 2005, CSIRO, 2008; Murphy and Timbal, 2008; Potter et al., 2008; Verdon-Kidd and Kiem, 2009b).

We agree that the trend in SAM may be related to global warming (as may the increased frequency of El Niño events since mid-1990). However, in this paper we only seek to demonstrate that autumn rainfall distributions have changed since mid-1990, resulting in significant hydrological implications (i.e. unprecedented decreases in runoff), and that these changes in rainfall distribution can be related to a lack of "wet" synoptic types (and increase of "dry" synoptic patterns) which in turn are linked to autumn SAM being persistently +ve combined with a lack of La Niña. What actually drives the large-scale climate modes (e.g. SAM, ENSO etc) and how anthropogenic climate change influences them (and how much of the post-1950 trend in SAM can be attributed to human-induced global warming) definitely warrants further research but is beyond the scope of this paper.

2-5. *The connection between the change in circulation patterns and the three large-scale indices is presented in table 3. I found this table hard to interpret. I would urge the authors to present the results in a more graphical way in which the connection can be better grasped. In this way the connection between the circulation patterns and these indices is rather vague. This is realized by the authors when they state that: "It seems that both SAM and ENSO play a role in modulating synoptic patterns and therefore rainfall during autumn". The connection between SAM and ENSO and rainfall in south east Australia is not new. This connection should be explored more in detail to be valuable.*

Author Response: A more graphical presentation of the results in Table 3 is included in Verdon-Kidd and Kiem (2009) which the readers are referred to in the caption for Table 3. The main point of Table 3 is to demonstrate how the occurrence of synoptic types which drive Victorian weather is dependent on the phase of the large-scale climate

processes (e.g. ENSO, SAM, IOD). As discussed in Sect 4.2 "dry" synoptic types (e.g. 3D, 5D) are much more likely when autumn SAM is +ve and the wet "autumn break" synoptic types (e.g. 1A to 2D but not 2A) rarely occur unless autumn SAM is negative and/or there is a La Niña event.

It is acknowledged that various studies exist which have linked SEA rainfall to either ENSO or SAM and the readers are referred to Kiem and Verdon-Kidd (2009) where this previous worked is summarised. However, to date the previous work has focussed on linking SEA rainfall to individual large-scale drivers and has overlooked possible interactions between large-scale climate modes that may enhance or suppress the impacts on SEA rainfall. Risbey et al. (2009: On the remote drivers of rainfall variability in Australia, Monthly Weather Review, 137, 3233–3253) showed that *"for most Australia regions individual drivers, when treated as a single process, account for less than 20% of monthly rainfall variability*" therefore stressing the point made in this paper (last paragraph of Sect 4.2) and also Kiem and Verdon-Kidd (2009) and Risbey et al. (2009) that interactions between climate mechanisms in the Pacific, Indian and Southern Oceans, and their relationship with the local scale synoptic patters that actually deliver weather to SEA, must be further investigated in order to better understand (and predict) SEA climate variability and/or change. Therefore, even though it is true that numerous studies exist which examine links between SEA rainfall and either ENSO or SAM or IOD, this study is one of the first to attempt to investigate the **combined** influence of ENSO and SAM and IOD on SEA rainfall.

In addition, this study is novel because rather than simply correlating SEA rainfall with indices of ENSO, SAM, IOD etc an attempt is made to understand the mechanisms by which large-scale ocean-atmospheric climate variability (e.g. ENSO, SAM etc) transmits its influence via synoptic systems to SEA rainfall (i.e. what is the relationship between large-scale climate modes and the local scale synoptic patters that actually deliver weather to SEA). This is something that is poorly understood and therefore inadequately simulated in climate models and has been identified (e.g. Pook et al., 2008; SEACI publications, Indian Ocean Climate Initiative publications etc) as an area requiring extensive further research. In this paper we present a preliminary 'first pass' investigation into this issue while at the same time acknowledging that it is preliminary work and there is a lot more to do.

2-6. They argue that the drying since the 1990's is due to a combination of El-Nino and SAM. This is based on hand-waving arguments. There is no quantification given how much ENSO and SAM have affected the rainfall in south east Australia since 1990's.

Author Response: The readers are referred to Kiem and Verdon-Kidd, 2009. In this paper quantification and further detail is given on the combined impact of ENSO & SAM on autumn rainfall in Victoria. While these results are sufficient to make the points we make in this paper we also agree with Reviewer 2 in that more work is needed. As stated in the paper (end of Sect 4.2) *"understanding into impacts associated with large-scale climate drivers (and their interactions) is in its infancy"*. We acknowledge in the Conclusions that attribution of dry autumns since mid-1990 to combined SAM-El Niño

impact is based on preliminary findings only (i.e. those presented here and in Kiem and Verdon-Kidd, 2009) and agree with the reviewer that further research is urgently needed to better quantify and understand the role that interactions between climate mechanisms (i.e. both large- and regiona/synoptic-scale drivers) have in driving SEA (and wider Australian) climate. Identification of this knowledge gap, and its significance, is one of the major contributions of this paper.

2-7. They criticize the arguments of Cai and Cowan for explaining the reduction in stream flow, without putting forward new hypothesis. Figure 7 presents interesting results but is only a starting point. Therefore the whole topic of changes in stream flow is not investigated in depth and gives hardly any new information.

Author Response: Our criticism of Cai and Cowan (2008b) is based on the fact, as mentioned in the paper (Sect 5) *"the physical mechanisms by which rising temperatures contribute to enhance the reduction in streamflow are not clear"* and therefore we question Cai and Cowan's (2008b) hypothesis. We feel that our criticism is valid given that many researchers, including Reviewer #1 (see comment 1-33), have recently pointed out that the work of Cai and Cowan (2008b) is flawed in a number of areas:

- As stated in Section 5, the hydrological modelling (i.e. rainfall-runoff regression relationships) used by Cai and Cowan is too simplistic and doesn't account for antecedent conditions. Cai and Cowan (2008b) acknowledge this in their own conclusions (see also the numerous SEACI and MDBSY publications on this issue);
- As stated in Section 5, other studies exist which show a decreasing evaporation trend across SEA since 1950. This is at odds with claims by Cai and Cowan.
- Recently published work by Lockart et al. (2009) suggests that the work of Cai and Cowan (amongst others who have also published similar work – see references within Lockart et al., 2009) represents a *"significant misunderstanding of known processes of land surface – atmosphere interactions that has led to the incorrect attribution of the causes of the anomalous temperatures, as well as significant misunderstanding of their impact on evaporation within the Murray-Darling Basin"*. Lockart et al (2009) also go on to state that it is imperative that work such as Cai & Cowan (2008b) is reviewed urgently – which is what we attempt to do here and which is another major contribution of this paper.
- Last but not least, there are numerous other potential factors (listed in Sect 5) besides or in addition to temperature that could explain the runoff decrease not due to rainfall – including changes to the timing and compilation of seasonal rainfall which, as we demonstrate, at least can be linked to known and plausible physical mechanisms (where as the rising temp=>increased evaporation hypothesis put forward by Cai and Cowan (2008b) is tenuous (see for example Lockart et al. 2009)).

Contrary to Reviewer #2's comment, we do offer an alternate hypothesis (see Conclusions: point 2, point 3 and the final paragraph). We also put forward suggestions as to how this work can be extended so as to better understand drivers of SEA climate

and to enable improved studies focussed on attribution and/or future climate change impacts.

Also contrary to Reviewer #2's opinion, Fig 7c presents, for the first time, results demonstrating the impacts of "changes in seasonal rainfall makeup". Further, although it is well known that Victorian streamflow is highly dependent on antecedent conditions what Fig 7c demonstrates (as discussed in last paragraph of Sect 5) is that if antecedent conditions are "dry" leading into Autumn then below average autumn flow is almost certain unless a pre-frontal trough occurs, which Table 3 reveals is unlikely in SAM+/El Niño phase. The novel insights gained here must be accounted for in attribution studies and have the potential to significantly improve seasonal streamflow forecasting ability in SEA. We acknowledge that Fig 7a & 7b are simply alternative ways of demonstrating points made earlier in the paper, however, we feel that Fig 7a & 7b are still useful as background to help understand the results, and process followed to obtain the results, presented in Fig 7c. As stated in the Conclusion we acknowledge that the results behind Fig 7 are "preliminary" but include them to demonstrate that even though seasonal rainfall totals do not seem to completely explain the reduction in observed runoff, the changes to the daily distribution (caused by changes to the synoptic patterns) actually act to enhance the reduction in runoff. Therefore, a lot more of the reduction in runoff since mid-1990 could be explained by changes in rainfall **<u>if</u>** changes in rainfall are accounted for properly (i.e. via robust hydrological modeling at least at the daily scale that incorporates, as a minimum, antecedent conditions).

2-8. The most interesting aspect of this article is the connection to the change in circulation patterns (fig. 5). I would suggest the authors to focus on this aspect.

Author Response: We are currently looking into this aspect in detail.