**Michelle Ho**
PhD candidate
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 5404 F: +61 2 4921 6925
Email: Michelle.Ho@uon.edu.au

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Dear Dr. Wouter Buytaert (Handling Editor for this paper)

Thank you for providing the opportunity to address the comments made by the two anonymous Reviewers on the paper "The Southern Annular Mode: a comparison of indices" (Manuscript #: hess-2011-190).

Our responses to both Reviewers' comments are included below along with details indicating how the paper has been revised. The Reviewers both acknowledge that the topic and content of the paper is suitable for publication subject to their questions and comments being addressed.

The major concern arising from both Reviewers is the current pictorial representation of the differences between the SAM indices presented and this has been addressed in the revised version of the paper.

The second main concern (raised by Reviewer #2) was regarding referencing. In light of their comments, a thorough review and edit of the references made in the paper has been carried out with either references appropriately cited to the correct phrase or justification given for citing a particular reference (see response to comments from Reviewer #2 for details).

We thank the Reviewers for their comments which have improved the paper which we think makes a novel and significant preliminary step towards better understanding the interactions between large-scale climate drivers (e.g. the Southern Annular Mode) and Australian hydroclimatic variability.

If you require any further information please contact me.

Thank you

Michelle Ho

## Anonymous Referee #1

*1-1. Page 7469, section 3.1.3: It could be argued that if you are trying to link the SAM to a regional phenomenon, such as Australian climate, that it is better to use a regional SAM index. Hence the non-local, non-annular variability in a hemispheric SAM index can be removed. Perhaps the authors could construct a simple meridional pressure gradient over Australia and see how that compared with the other SAM indices examined.*

**AUTHORS' RESPONSE:** The Reviewer's comment is valid and has previously been investigated by Meneghini et al. (2007) where a regional index was used to investigate SAM impacts on Australian rainfall. However, this index is no longer available and the primary focus of the paper is to assess the SAM indices that are readily available as these would be the indices used by most practitioners in the hydrology field. The use of a meridional pressure gradient over Australia, or the region of interest is however a good suggestion as it would likely capture SAM behaviour relevant to the region, and this has been added to the conclusions as a recommendation for further study.

*1-2. My principal issue with the paper in its current form is with the pictorial representation of how the differences between the indices and their relationship with Australian rainfall are shown, which I think could easily be improved upon (see below).*

*1-3. Figure 3-10: Given that the main aim of the paper is to compare the SAM indices I would prefer that the authors replace Figures 3-6 with equivalents based on 1979-2002 and get rid of Figures 7-10. I think this would prove far more instructive to the reader. A number of papers have demonstrated that SAM-climate relationships vary over time: this might be for another paper, but if the authors wanted to include something here they could look at those SAM indices with greater time periods and split them into two halves to see how temporally stable the relationships between the SAM and the Australian rainfall are.*

**AUTHORS' RESPONSE:** We acknowledge, and in hindsight agree with, the Reviewer's comments and have revised the paper accordingly regarding the replacement of Fig 3-6 with ones based on the 1979-2002 period and removal of Fig 7-10. Additional analysis along the lines suggested by Reviewer 1 has been included to demonstrate that SAM-climate relationships vary over time.

We have used the longer of the two indices that accurately represents SAM (the Visbeck index) and shown the seasonal differences between the entire Visbeck record correlated to Australian rainfall (between the years 1900-2005) and the previously shown seasonal correlations between the Visbeck index and Australian rainfall over the period 1979-2002.. This analysis illustrates the changes that correlations between SAM and Australian rainfall vary over time but does not explain why – explanation of this non-stationarity is the subject of ongoing work and beyond the scope of this paper.

*1-4. Page 7464, line 15: The SAM has also been described as the high latitude mode prior to the AAO/SAM.*

**AUTHORS' RESPONSE:** The term "High Latitude Mode" (HLM) is indeed another term used to describe the SAM and this has now been included.

*1-5. Page 7468, line 6: It is more correct to say that the NOAA index is based on fields derived from a numerical weather prediction model that assimilates both satellite data and observations.*

**AUTHORS' RESPONSE:** Agreed. This has been revised to "The NOAA SAM index is based on data derived from a numerical weather prediction model that assimilates both station observations and satellite data."

*1-6. Page 7468, line 7: The statement that 'satellite data has been shown to be the most reliable data source for analyzing Antarctic meteorology (King and Turner, 2007, p. 63)' is rather all encompassing. Clearly for that statement to be true depends on the*

*type of meteorology being studied; e.g. boundary-level meteorology is better studied from the surface. What satellite data has done is to fill in the large data voids over the Southern Ocean and hence reanalysis products have improved since their assimilation. Also, there is nothing in the reference that supports the statement.*

**AUTHORS' RESPONSE:** Agreed. The reference was to several sections of the publication: on page 43, that satellites used by NOAA provided wide spread data coverage over the Antarctic region; page 59 that "With the introduction of Earth-observing polar orbiting satellites in the early 1960s very large quantities of observations of the Antarctic became available"; finally on page 63 that "NOAA maintain an archive of data from the TIROS-N/NOAA series of satellites extending back to 1978". In summary, the availability of satellite data for Antarctica in the late 1970s has increased the reliability of reanalysis data in this region - in turn the NOAA index (calculated from this period), is based on data less influenced by the previously sparse data in the region. This statement has been modified to:

"Satellite data has enabled large quantities of wide-spread observations in the Antarctic region since the late 1970s (King and Turner, 2007). It follows that gridded data, that incorporates satellite data, is less influenced by spurious errors encountered with earlier reanalysis data (Marshall, 2003). Hence the NOAA index, which is available from 1979 onwards and incorporates the use of satellite data, will be used as the baseline, or reference index, for the remainder of this study."

1-7. *Page 7469, line 24: There is no regression of the Marshall (2003) index with either temperature or precipitation in that paper. The relevant papers are Marshall (2007) in International Journal of Climatology for temperature and van den Broeke and van Lipzig (2004) in Annals of Glaciology for precipitation, using a climate model.*

**AUTHORS' RESPONSE:** Acknowledged and agreed. Temperature and precipitation are regressed with the Marshall (2003) index in later studies and this is now noted in the text with references to Marshall (2007) added in relation to temperature, while Silvestri and Vera (2009) and Feng et al. (2010) have been referenced in relation to precipitation. However, a search for papers by van den Broeke and van Lipzig in Annals of Glaciology only found the paper "Changes in Antarctic temperature, wind and precipitation in response to the Antarctic Oscillation", which uses the Thompson and Wallace (2000) SAM definition, referred to as the NOAA index in our paper, not the Marshall index.

1-8. *Page 7473, line 20: replace 'NCEP-NCAR reconstructions, whose errors increase at higher latitudes when compared with station data' with 'NCEP-NCAR reanalysis, which has a positive pressure bias at higher latitudes compared with station data that decreases through time.'*

**AUTHORS' RESPONSE:** Phrase replaced as suggested.

1-9. *Page 7474, line 19: I don't think it's important that no data points exist in the second and fourth quadrants. Surely it's the distance from the y=x line that matters. Whether the SAM is slightly negative or slightly positive is unlikely to have any meaningful physical manifestation, especially given that the period over which the data are normalized to calculate the SAM, and hence the value of the resultant mean' is arbitrary anyway.*

1-10. *Page 7479, line 16: Again, the sign of the SAM may not be important if the values are close to zero.*

**AUTHORS' RESPONSE:** For the above two comments: We agree with both comments ***if*** the SAM index in consideration is only slightly negative or slightly positive, ***and*** the method of analysis is to use composite data techniques that also consider a neutral SAM phase, all of these slightly negative or positive points will be grouped together as a "neutral" SAM phase. However, for some of the indices, e.g. the Fogt and JW58, there are points in the second and fourth quadrants that are located relatively far from the origin and these points would result in marked differences in how SAM is represented (i.e. positive instead of

negative and vice-versa). It follows that even if composite methods were used in hydroclimatic analyses, the threshold value used to define negative, neutral and positive phases would produce varying results.

*1-11. Page 7479, line 24: I'm not sure what the authors mean by 'SAM process'. You could argue that will vary depending on whether the SAM is defined as EOF1 or a meridional pressure gradient and as a hemispheric or local 'process'.*

**AUTHORS' RESPONSE:** Agreed. This sentence has been revised to "which index most satisfactorily represents SAM?"

*1-12. Page 7480, line 19: Actually it does mean that SAM indices based on early reanalysis data, and any trends derived using these data, are flawed!*

**AUTHORS' RESPONSE:** Yes, it is true that SAM trends derived using early reanalysis data (i.e. prior to the availability of satellite data) would be flawed and therefore, the sentence has been edited to emphasize this

*1-13. Page 7482, line 10: A point about the SAM is that is has been shown to be 'reasonably' approximated as a meridional pressure gradient. Clearly it would be possible to develop more sophisticated indices but I expect a law of diminishing returns would apply.*

**AUTHORS' RESPONSE:** Agreed. The SAM can be "reasonably" approximated as a meridional pressure gradient, however, a more sophisticated SAM index that included additional parameters (e.g. wind, cloudiness like the multi-variate ENSO index) may actually enable hydroclimatic variability in the Southern Hemisphere to be more clearly attributed to SAM variability with possible follow on implications of improved forecasting.

**Anonymous Referee #2**
*Received and published: 13 October 2011*

*2-1. The main premise of the study is to compare different definitions of SAM and their relationship with Australian rainfall. To attain this goal, all other differences apart from definition should be removed. Hence, rather than use existing SAM indices available online, it is suggested that the authors calculate the SAM indices directly themselves using a consistent dataset (eg. NCEP/NCAR reanalysis) and time period (eg. 1979-2002).*

**AUTHORS' RESPONSE:** It is stated in the abstract that the premise of the study is to compare different definitions of SAM *and* the different SAM indices. The importance of reviewing the indices is again stressed in the introduction. The purpose of presenting the differences between SAM definitions was to enable readers to develop background knowledge of what SAM is and comprehend why so many different SAM indices are being used in numerous studies of SAM and its impacts.

The SAM indices used in this study are readily available and research into SAM impacts in the future will more than likely continue to select a SAM index from the suite of indices available. Therefore, it is critical that our review is focused on the SAM indices that will most likely be used by researches and practitioners in the future and to highlight the differences between the indices including the data used, time period and definition on which it is based, such that these differences can be taken into account when interpreting results and making conclusions.

Rather than focusing on the development of yet more SAM indices, the purpose here is to review existing indices and assess their strengths and limitations. Further, we are reluctant to utilise the NCEP/NCAR reanalysis dataset as a basis for recalculating SAM indices due to the problems with the dataset prior to 1970 (refer to Reviewer Comment 1-12 and our Response).

*2-2. While this will not be possible for all indices, it is achievable for the indices using gridded data and would allow a cleaner comparison ensuring that similar base periods are used. It is also recommended that these indices are used for the correlation comparison figures (Fig 3-6) rather than mixing indices with different time periods. This will give a better visual comparison of the differences between the indices compared to the current Fig 7-10 which show small differences in correlations which are insignificant to begin with and hence could simply be noise. In fact, the major features of the SAM-rainfall correlations appear remarkably consistent across indices so controlling for time period and dataset will presumably increase this consistency.*

**AUTHORS' RESPONSE:** We acknowledge the point made by Reviewer #2 regarding a comparison to be made over similar base periods and the previous changes made to the figures, in line with Reviewer #1's suggestions, address this point.

We have also addressed the comment from Reviewer #2 regarding the use of a consistent dataset in the response to the previous comment.

*2-3. : From the analysis provided, the main result appears to be that the Fogt and JW58 indices are quite different from the others, yet no speculation on why this would be is given.*

*2-4. Presumably it is partly due to the use of ERA-40 and various station data compared to the NCEP/NCAR reanalysis used for the other indices. It is suggested that a comparison of the NCEP gridded indices with ERA-40 versions is carried out as this may help elucidate a root cause of the differences with the Fogt and JW58 indices.*

**AUTHORS' RESPONSE:** The results on p 7474, L 3-7 illustrate why the Fogt and JW58 indices are so different. Although we acknowledge that the explanation is not repeated and

emphasized in the conclusions, possibly resulting in the comment from Reviewer #2 that no reason is given for the difference. We have consequently included a discussion of the Fogt and JW58 indices in Section 6 in the revised paper.

See above response. Also, we do not believe that the use of a different gridded data set is the primary cause of the differences seen and an alternate suggestion is made on p 7474, L 3-7.

*2-5. Significance of the correlations should also be assessed and noted on the figures with either contours or stippling. The fact that very few correlations are significant should be pointed out at the beginning of the discussion section.*

*2-6. It is difficult to see distinctions in the colour shading in the geographical maps. Suggest rescaling.*

**AUTHORS' RESPONSE:** The Reviewer suggests that the lack of statistical significance should be stated at the beginning of the discussion section, however, we note in section 5.2 (pg 7475 L20-23) that correlations are only used as a tool for comparing SAM indices, not to establish what the relationships between SAM and rainfall are. Therefore, correlation significance is not the focus, rather, the difference in the results is the important point in order to show differences in the SAM indices.

Additionally, our focus in the discussion is not centred on the actual correlations obtained in the results section; rather we focus on the differences in the correlations and what that represents (i.e. a difference in how each SAM index relates to Australian rainfall). Mentioning statistical significance here would interrupt the flow and focus of the discussion.
Statistical significance is mentioned in the fourth paragraph of the discussions.

With regard to the Reviewer's suggestion to include contours or stippling, as per the response above, the inclusion of statistical significance in the figures would imply that we are attempting to quantify the relationship between SAM and Australian rainfall, which is not the point of this paper though it is a worthwhile exercise for a follow up paper.

We acknowledge and agree with Reviewer #2's comment regarding the difficulty of viewing the current colour scheme and the figures have been revised accordingly.

*2-7. The other major comment is that references are often cited in one long list even though multiple points are made in a sentence and all the references don't necessarily cover all points. References are also often inappropriately cited, many of which are pointed out below.*

**AUTHORS' RESPONSE:** Revisions to the paper have been made to address Reviewer #2's comments regarding appropriate citing of references. A thorough check of all references in the paper has also been made to ensure that all statements are appropriately cited. We have also taken into account the additional comments regarding references and have addressed these comments below.

*2-8. The introduction requires a thorough revision.*

**AUTHORS' RESPONSE:** Introduction has now been revised significantly – in response to both Reviewers' comments.

*2-9. Some discussion should also be given regarding the potential predictability of the SAM, or lack thereof, compared to ENSO. The SAM arises due to internal atmospheric dynamics and is therefore thought to have little predictability past numerical weather prediction timescales. See eg. Watterson (JGR, 2001). This limits its usefulness for climate prediction compared to eg. ENSO.*

**AUTHORS' RESPONSE:** We agree that potential predictability of SAM, or lack thereof, is an important topic, however, this was deliberately not mentioned in this paper because (a) the Reviewer is correct in that there is limited usefulness is SAM predictions currently and (b)

our ultimate purpose is the better understand SAM and most importantly its relationship with Australian hydroclimatic variability. In order to understand SAM we reviewed the existing literature on that subject and in order to assess SAM teleconnections you first have to have an indicator of SAM. Thus the first step, and the aim of this paper, is to compare the various SAM definitions and indices and determine the pros and cons of each. The consequences of not performing this first step properly is, as we have shown, conflicting results as to what the effects of SAM are on the hydroclimate of Australia (and perhaps this is contributing to the lack of predictability of SAM and its related impacts – but that is another topic altogether and beyond the scope here).
We also wish to point out that although Watterson (2001) warns of the limited predictability of temperature or rainfall using Southern Ocean variability, important advances into the predictability of SAM have been made and published (see Marshall, A., D. Hudson, M. Wheeler, H. Hendon, and O. Alves, 2011: Simulation and prediction of the Southern Annular Mode and its influence on Australian intra-seasonal climate in POAMA. Climate Dynamics, doi: 10.1007/s00382-011-1140-z.) Whilst currently limited to skilful predictions of approximately 2 weeks, there appears to be potential for "extended-range predictability in a dynamical forecast model" in the future.

*2-10. p7462, l5 - It could be argued that Hendon et al (2007) have robustly quantified the relationship between the SAM and Australian rainfall, assessing significance in the relationship with Australian rainfall over the satellite era*

**AUTHORS' RESPONSE:** We agree that the study by Hendon et al. (2007) was indeed a comprehensive quantification and test of the significance of SAM on Australian rainfall (as well as max/min temperature and pressure at sea level and 850GpH) in the period 1979 2005. However, a study by Silvestri and Vera (2009 in J. Climate) showed a change in the relationship between SAM and Australian rainfall in around the late 1970s, early 1980s. In addition, the paper suggested for citation by Reviewer #2, L'Heureux and Tompson (J Climate, 2006) describes an interaction between the El Niño Southern Oscillation and SAM, the former of which has been shown to be modulated on inter-decadal timescales resulting in non-linear, non-stationary relationships with the Australian hydroclimate. Thus an analysis of SAM impacts on Australian rainfall over a period of less than 30 years would not sufficiently capture the full variability (decadal and interdecadal) in the relationship between SAM and Australian rainfall. We therefore maintain that the relationship between SAM and Australia's hydroclimate has not previously been robustly explored or quantified – as evidenced by the conflicting results and conclusions surrounding, and ongoing investigations by groups like the South East Australian Climate Initiative into, the SAM relationship with Australian hydroclimate issue.

*2-11. p7462, l8-9 - it's not clear that the strengths and weaknesses of the various indices are assessed in the paper, in fact it seems an impossible task given that all are valid depictions of the SAM*

**AUTHORS' RESPONSE:** Indeed all the indices are valid depictions of the SAM depending on the definition of SAM that is chosen. Our intention here was to communicate that we would be reviewing the strengths and weaknesses of each SAM index with respect to its application in assessing hydroclimatic variability. We acknowledge that this sentence is ambiguous and have edited it accordingly.

*2-12. p7463, l1 - refer to Risbey et al (2009) here*

**AUTHORS' RESPONSE:** Reference inserted.

*2-13. p7463, l8 - I don't think 'confusion' is the right word here, each SAM index was developed with a specific focus in mind and will highlight a certain aspect of the annular mode. Rephrase.*

**AUTHORS' RESPONSE:** We acknowledge the comment from Reviewer #2 that each SAM index was indeed developed with a specific focus in mind and as a result a different

definition and hence index for the SAM was developed. As stated in the sentence in question, the “confusion” lies in which of these SAM indices hydroclimatologists are to use for their studies of quantifying Australian hydroclimatic variability with respect to SAM? We maintain that there is a “confusion” given the numerous indices available and evident in the fact that these indices have all been used to attribute hydroclimatic variability around the world. e.g. Hendon et al. (J. Clim. 2007) use the NOAA definition of the SAM, Meneghini et al. (2007) use the AOI and AOIR as described in our paper, the Marshall index is used in Cai et al (GRL 2006) and Evans et al. (Clim. Dyn. 2009)

*2-14. p7463, l12 - L’Heureux and Thompson (J Climate, 2006) should be cited here for a study that examined the link between SAM and ENSO.*

**AUTHORS’ RESPONSE:** Citation included.

*2-15. p7463, l18 - References are again lumped together for papers that don’t cover both regions. Also, Hendon et al (2007) did not link the recent rainfall declines to the SAM, in fact they specifically avoided doing so given there was no trend in the wintertime SAM during the period they looked at. Reference should be removed from this statement.*

**AUTHORS’ RESPONSE**: References have been split to appear directly after the relevant locations mentioned. Reference to Hendon et al. (2007) has been removed.

*2-16. p7463, l20 - McGowan et al 2010 do not show any projections of the SAM under global warming. Remove reference from this statement.*

**AUTHORS’ RESPONSE**: Reference to McGowan et al (2010) removed.

*2-17. p7464, l16-20 - the definition of the SAM given here seems quite confused and it is difficult to understand what the authors are trying to say. The SAM describes shifts in atmospheric mass north and south or, alternatively, movements of the westerly jet north and south with corresponding changes in the pressure pattern. The description of an alternating pattern needs to be put in context with these movements.*

**AUTHORS’ RESPONSE**: Agreed. We have made some amendments (more detail below in responses to 2-19 to 2-21). The inclusion of change in the pressure pattern has now been included in the description as, in line with Reviewer #2, this is an important feature of the SAM.

*2-18. p7464, l16-20 cont’d - Also the SAM is defined as longitudinally symmetric so describing it as a wave-like structure is incorrect*

**AUTHORS’ RESPONSE**: We acknowledge that the use of the term ‘wave’ is confused here as it has been used to describe zonal patterns of circulation in the Southern Hemisphere where a wave 1 structure is zonally symmetric, which is often the case of SAM in summer, whilst a wave number 2-3 is often observed in winter (Kidston et al. J. climate 2009). The term “wave-like” has been removed.

*2-19. p7464, l16-20 cont’d - and the winds weaken north of 40S in only one phase of the SAM.*

**AUTHORS’ RESPONSE**: We agree that a positive SAM is associated with winds weakening or turning easterly north of 40°S (Gillett et al. 2006) and this was not obvious in the context that it has been written in. Given the description of wind movements associated with SAM in the previous sentence, the repeated reference to wind behaviour influence by SAM has been removed.

*2-20. p7464, l16-20 cont’d- Since the rest of the paper focuses on definition based on the pressure and geopotential height fields it is suggested this section is rewritten with a focus on those fields.*

**AUTHORS' RESPONSE**: A description of SAM based on pressure fields is already included in the introduction (see p 7465 L15-16). The inclusion of a definition of SAM in terms of geopotential height fields would complicate the definition as this would introduce variations in different layers of the atmosphere and seasonality of these variations in relation to SAM (and hence our reason for a simplified explanation of SAM also addressed in comment 2-24), which is described in detail in Thompson and Wallace (2000). Thompson and Wallace (2000) is later referenced in our paper under the description of the NOAA index.

*2-21. p7464, l19 - remove "(west to east)"*
**AUTHORS' RESPONSE**: removed

*2-22. p7464, l23 -> p7465, l14 - these two paragraphs are too text-book-like even for this journal*
**AUTHORS' RESPONSE**: We agree that the explanation given in these two paragraphs is quite detailed and have edited it to make it more concise. The information conveyed in these paragraphs is however necessary as the detailed investigations into SAM (i.e. Thompson and Wallace, 2000, Rogers and van Loon 1982) are aimed at climatologists, not hydrologists and therefore the descriptions of SAM are complex. Here we are attempting to present the reasons for why the SAM occurs in a way that is easily comprehendible.

*2-23. 7464, l17->21 - The shift of the storm track and phase of the SAM correspond, but it has not been shown that one leads to each other. Suggest rephrasing. Also, Marshall (2003) did not look at storm track shifts so this reference needs replacing.*
**AUTHORS' RESPONSE:** It was not our intention to communicate that the phase of SAM *caused* a shift in the storm track and this has been rephrased to remove any suggestion of this.

Marshall (2003) was cited in reference to "strengthening circumpolar vortex and zonal (westerly) winds that circle Antarctica"

Kidson and Sinclair (J. Climate, 1995) has been cited in reference to the "shift in the storm track towards the South Pole" associated with a positive SAM.

*2-24. p7465, l25 - McGowan et al (2010) do not look at seasonality in SAM teleconnections. Replace reference.*
**AUTHORS' RESPONSE**: Agreed. Reference and sentence removed.

*2-25. p7466, l5-9 - Gillett et al (2006) do not isolate the relationship of the SAM with different seasons.*
**AUTHORS' RESPONSE**: Gillett et al. (2006) discuss the likelihood that the summer SAM trend found in other studies was likely to influence trends in precipitation in the south-west Australia region. Combined with their findings of significant decreases in precipitation resulting from positive SAM behaviour in this region, we feel that this reference is valid. However, we recognize that the wording of the statement may be unclear and have rectified this.

*2-26. p7466, l5-9 cont'd - Meneghini et al (2007) use a regional index which should be noted here.*
**AUTHORS' RESPONSE**: In the paragraph following this, we note that different indices are used. Additionally, the regional Meneghini index is presented later in section 3.1.3. Also the findings in Meneghini et al. (2007) for positive SAM-rainfall correlations in summer were found using both the regional and zonal indices.

*2-27. p7466, l19-27 - it is just as likely that the different indexes and time periods used in the various studies lead to their different conclusions*

**AUTHORS' RESPONSE**:
Both issues raised (different indices and different time periods) are likely causes of the inconsistencies found in the conclusions of the various studies investigating SAM relationships and impacts. These points are the primary motivations for our study and our aim to propose an index that will be suitable for use in attributing hydroclimatic variability to SAM.

*2-28. p7467, l5-7 - point out that the time periods analysed are also often different in these studies*

**AUTHORS' RESPONSE**: Reference to time periods has been included in the revised paper as suggested.

*2-29. p7468, l1-15 - the NOAA SAM index is based on NCEP-NCAR Reanalysis, not direct data as indicated here*

**AUTHORS' RESPONSE**: Rephrasing of the description of data used for the NOAA index has been made in line with the recommendation made by Reviewer #1 also clarifying the ambiguity identified by Reviewer #2.

*2-30. p7472, l4 - a reference is needed for the rainfall dataset*

**AUTHORS' RESPONSE**: We have stated in our description of data used in the analysis of this paper that the rainfall data is obtained from the Australian Bureau of Meteorology. We state the period over which data is available and also reference the technique that was used to generate the gridded rainfall dataset.

*2-31. p7480, l24 - why is the Marshall index the most accurate? This statement is speculative and contradicts earlier statements that the NOAA index is the most reliable.*

**AUTHORS' RESPONSE**: The earlier statement that the NOAA index is the most reliable is due to it been calculated from a period when more reliable data in the high latitudes of the Southern Hemisphere became available. The emphasis here is on station data providing the most accurate, consistent representation of SAM prior to 1979 given the spurious trends seen in reanalysis data (especially prior to 1979) as discussed in our paper.

To avoid mis-interpretation, we have included the caveat "SAM indices based on station records of pressure at 40°S and 65°S (Gong and Wang, 1999) are most likely to have the most accurate representation of SAM ***prior to 1979***. . ."

*2-32. p7481, l8 - the choice of the Marshall index as the most reliable appears very subjective and not based on any of the calculations produced in the paper. More justification is required or this statement should be removed.*

**AUTHORS' RESPONSE**: We have shown in our analysis that SAM indices based on reanalysis data are similar, as are SAM indices based on station data. We avoid basing our selection of a SAM index, suitable for use in hydroclimate attribution studies, on which index gives a strong relationship. Rather, given a clear divide between indices is shown to depend on whether station data or reanalysis data is used, we then debate the merits of using station based data opposed to reanalysis data with the conclusion that the use of station-based data will give more accurate long term information on SAM variability.

*2-33. Cai et al 2011 and Feng et al 2010 are also relevant studies and should be cited.*
*Cai, W., P. van Rensch, S. Borlace, and T. Cowan (2011), Does the Southern Annular Mode contribute to the persistence of the multidecade-long drought over southwest Western Australia?, Geophys. Res. Lett., 38, L14712, doi:10.1029/2011GL047943.*

*Feng, J., J. Li, and Y. Li (2010), Is there a relationship between the SAM and southwest Western Australian winter rainfall?, J. Clim., 23, 6082–6089.*

**AUTHORS' RESPONSE**: A reference to Feng et al 2010 has been included. However, the reference to Cai et al 2011 is not appropriate here because (a) in the context it is used in this paper Cai et al 2011 only reaffirms what was already established in Cai and Cowan 2006 and (b) several issues exist with the Cai et al 2011 work and these have been raised with the Editors of Geophysical Research Letters and also Cai and his fellow authors. There are also several independent papers currently in the publication process that are critical of the work contained in Cai et al 2011 for a variety of reasons. Until these issues are resolved it is premature to reference Cai et al 2011.