"This review was prepared as part of graduate program course work at Wageningen University, and has been produced under supervision of Prof Wouter Peters. The review has been posted because of its good quality, and likely usefulness to the authors and editor. This review was not solicited by the journal."

Review of 'Assessing the impact of large volcanic eruptions of the Last Millenium on Australian rainfall regimes' by Blake et al.

This paper is about the responses of the El Niño Southern Oscillation (ENSO), the Indian Ocean Dipole (IOD) and Australian precipitation to tropical volcanic eruptions. 9 ensembles from the NASA GISS ModelE2-R were analysed and run for the six largest tropical volcanic eruptions between 850 and 1850 CE. Anomalous conditions in ENSO, the IOD and Australian rainfall as a result of these volcanic eruptions were explored. Results show that large tropical eruptions during the last millennium indeed impact the large-scale IOD and ENSO systems and the Australian rainfall regimes. Larger mean atmospheric sulfate loading results in more persistent and more extreme positive IOD conditions and a stronger ENSO response. A positive response of Australian precipitation to volcanic forcing was found, although this response is stronger in NW Australia than in SE Australia.

The still relatively unclear relationships between tropical volcanic eruptions and the El Niño Southern Oscillation (ENSO), the Indian Ocean Dipole (IOD) and Australian precipitation were thus successfully explored with your research. Since you give a clear overview of these relationships and your approach can also be applied for exploring the impact of time-evolving forcings, such as volcanism, in other regions, this research strongly contributes to this field of research. Moreover, the paper fits the scope of the journal 'Climate of the Past', since the impact of historic volcanic eruptions on climatic variables is evaluated.

You start the paper with an elaborate introduction where a lot of references to previously published literature on this topic are made. You compare the results of several papers, which provides the reader already with some idea of the relationships that can be expected to be found in this paper and an overview of the state of the art of this field of research. A clear objective of the study is stated at the end of the introduction, which provides a concrete overview of the content of the paper. The results of the research are well-structured and have a logical order, since the results of all relationships between the variables are discussed one by one. Moreover, the figures of the results are clear and provide a good overview of all final results. The text in the results section and the result figures match and complement each other. The statements that are made in the discussion are well-funded on the results or on information from previous papers. I like the fact that differences between the results of this paper and of previous research are compared and possible explanations are given. Besides, the discussion and conclusion fit well to the relationships that were going to be explored as stated in the introduction, so the circle of the paper is closed. The paper contains a nice discussion on the limitations of the approach. Based on other literature some improvements are stated that should be made in future modelling in order to improve the accuracy of volcanic eruption model simulations. The improvements stated here form actually a small summary of previously published literature that can be consulted in order to figure out the exact adaptations that will improve the modelling of volcanic eruptions and corresponding processes.

In conclusion, I think your paper is well-written and a valuable contribution to this field of research. However, there are three major weaknesses that I think need to be solved before your paper can be published. These are explained below in this review and I also included some minor points that need to be improved in order to clarify some points in your paper.

Major arguments

1. Your methodology contains in line 102 the statement that the Coupled Model Intercomparison Project Phase (CMIP5) is used in the NASA GISS ModelE2-R. However, Taylor et al. (2012) explain that the CMIP5 strategy can be used for long-term (century time scale) and near-term integrations (10-30 year). You explored anomalous conditions in the ENSO, the IOD and Australian rainfall for 7 years in total and only five years after a volcanic eruption, since this minimizes the effect of trends or low-frequency climate variability, which is a good argumentation. However, I wonder if the use of the CMIP5 gives reliable short-term model results for this short time period.
   Since you make you use of the NASA GISS ModelE2-R General Circulation Model and you refer to Schmidt et al. (2014) in line 100, I assume that your model contains all components that are taken into account by Schmidt et al. (2014) and that it includes an interactive representation of the atmosphere, ocean, land and sea ice. I expect that for most atmospheric processes the shorter timescale of your research will not be a problem, since most of these atmospheric processes are fast. The influence of aerosol injection into the atmosphere after a volcanic eruption will quickly have a noticeable influence in the model on for example atmospheric temperature, incoming shortwave solar radiation and cloud formation. However, ocean and land, domains that are also taken into account in the model, will have slower responses to volcanic eruptions. For example sea surface temperature, ocean currents and permafrost presence will take longer to adapt to the aerosol injection and corresponding atmospheric changes. For these variables the five year time scale that is investigated in your research might possibly be too short in order to explore the trend that occurs after a volcanic eruption.
   My recommendation is to validate the model results of these 7 year runs with available data and add these results to your paper. Is it a possibility to gather data of the ENSO, IOD and Australian rainfall anomalies for the times following the six eruptions used in you research and compare these with your model results? If this data is not available, because your eruptions occurred a long time ago, it might also be possible to use more recent data of the ENSO, IOD and Australian rainfall anomalies in years with more recent volcanic eruptions and compare these with new model results of these more recent volcanic eruptions. These volcanic eruptions are maybe smaller in size and have a smaller sulfate aerosol injection, but at least a validation of the model can be made in this way in order to check the use of CMIP5 for the relative short time period.
2. I do not think that the methodology, mainly section 2.1 Simulations, contains enough information to understand your exact process in order to set up and make use of the model. Information that is missing is which data you used and what its source is, why you chose to make use of the specific NASA GISS ModelE2-R and the CMIP5, which variation of this model and which configuration were used and which values were for example used for the effective radius of the sulfate droplets. Schmidt et al. (2014) discuss different model configurations of the NASA GISS ModelE2-R, different ocean models and global annual mean features over the period 1980-2004 for the different models, which gives me the idea that you also made these kind of choices before you started modelling. Miller et al. (2014) show that there are three versions of the atmospheric model (NINT, TCADI and TCAD) which treat the atmospheric

constituents and the aerosol indirect effect differently. I assume that you also used one of these models, but it is not described which one you chose and why.
Moreover, you do not explain why you chose to analyse nine ensembles from the NASA GISS ModelE2-R, as stated in line 100, and not more or less. It is also not explained why five ensembles were forced with volcanic forcing, while four were not. Of the five run with volcanic forcing, four were forced with Crowley and Unterman (2013)'s aerosol optical depth data and one with double the Ice-core Volcanic Index 2 by Gao et al. (2008), but why are these not equally divided? Is it not more logical to force for example three ensembles with Crowley and Unterman (2013) and three with Gao et al. (2008)?
I would recommend to expand the methodology section of your paper with a more elaborate description of the exact methods. It will improve the paper if an overview of the steps that were taken is added, including the data that is used, model configurations and parameter values. Also an argumentation for the choices that were made will result in a more complete understanding of the methodology. A more extended discussion can also be added to the paragraph starting at line 264 then, discussing whether the chosen methodology turned out to be appropriate or if other choices should have been made.

3. A lot of relationships are stated in the introduction between tropical eruptions and the ENSO, IOD and Australian rainfall. For example, volcanism leads to negative global precipitation anomalies, large tropical eruptions can increase the likelihood and amplitude of an El Niño event in following years and a negative IOD occurs immediately after an eruption and a positive IOD one year later. For the relationship between volcanic eruptions and the ENSO, two possible mechanisms are mentioned in lines 49-53, although not in much detail, and the other relationships are not explained at all. However, you already refer to quite some papers that contain a more elaborate description behind the relationships.
   If the mechanisms behind the processes would have been incorporated in the introduction, these mechanisms could also have been used in the discussion and conclusions section to explain the results that were found in your study. It could be checked whether the results correspond to these processes or if other processes are needed to explain the results. An example is that it would be useful if the processes that are taking place can be used to explain the difference between the timing of the peaks in figure 3 and 5, since this is currently not discussed in the paper.
   There are two specific results mentioned that I think will definitely become more understandable if a discussion in which the processes are taken into account is added. Line 239 in the discussion and conclusions section tells us that tropical volcanism leads to positive precipitation anomalies over SE and NW Australia. In line 34 in the introduction it was stated, however, that sulfate aerosols result in negative global precipitation anomalies. I am puzzled by this contradiction, could it be caused by different processes that are occurring at different scales?
   Moreover, in line 232-233 of the discussion it is stated that an El Niño-like pattern in the eastern Pacific is most visible in year 4, but also in year 0, 1 and 3. However, an explanation for this occurrence is not given, while I am wondering what occurred during year 2 that no El Niño was observed.
   I would recommend to include a broader overview of all mechanisms behind the relationships in the introduction and take these mechanisms into account in the discussion of your results. The references in your paper about the mechanisms can be used for this adaptation, for example Clement et al. (1996), Mann et al. (2005), Pausata et al. (2015), Cheung & Abram (2016) and Meyers et al. (2007). Adding these explanations to your paper would really help the reader with understanding the physical processes and consequently the

relationships that are discussed. Besides, if these physical processes are more discussed in the introduction, they can also be used to explain the results of your research, for example the two specific results whose causes were unclear to me, as I mentioned above.

Minor arguments

1. A result of your research that is not mentioned in the abstract is that volcanic aerosol cooling dominates the precipitation response, while this is, in my opinion, an important result that should also be stated in the abstract. I would recommend to add this result to your abstract after the other results that are mentioned.
2. In the introduction the research question(s) is/are not clearly stated, although this would help with providing the reader with a better overview of the contents of the paper. I am also missing a broader aim of the paper and societal relevance, since it does not become completely clear what you actually want to achieve with your research and how you think your research will contribute to society. I would recommend to add the research question(s) and societal relevance to the introduction of the paper and the societal relevance might also be mentioned in the conclusion, stating what the results and conclusions can be used for.
3. In line 127-128 you explain why you chose to examine the IOD over the July-November period and you refer to Weller et al. (2014). However, their paper and also their results are only about positive IOD's. Negative IOD's are mentioned only twice in the whole paper, so I am not sure you can refer to this paper when your research examines both positive and negative IOD's. If you think you were right to still use the statement of Weller et al. (2014), I would like to see your explanation about this and otherwise you might consider taking into account a different period.
4. You state in line 134-136 the reason why you chose to analyse precipitation anomalies in southeast and northwest Australia and you refer to Ashok et al. (2004). However, most results of Ashok et al. (2004) are only about India, Pakistan and the monsoon trough and I do not find any mentioning of Australia. It would be good to check this reference and, if it turns out to be still the right one, to mention which results of their research you used. If the reference is not correct, please change the reference into the one that you based your statement on.
5. In line 133 in the methodology it is stated that the rainfall anomalies were examined over the July-November period, but it is not explained why you chose this period. It could be that the precipitation results are completely different during the other part of the year, for example that precipitation anomalies are negative instead of positive. I think it would be interesting to also take this into account in your research in order to have a more complete yearly overview, so maybe you could also perform these model simulations. Otherwise you could explain in your methodology why this was not necessary or possible.
6. You chose to do your research for the six largest tropical eruptions between 850-1850 CE as stated in line 114 in the methodology, while previous research, for example Cheung & Abram (2015) and Maher et al. (2015), took also smaller eruptions into account. Would it not be useful to also include these smaller eruptions in your research, since a better comparison between your study and these other studies can be made then? If you have a specific reason why you only chose the six largest eruptions, I would recommend to explain this reason in the methodology section after you mention which eruptions you analysed.

Minor issues

- Page 3, line 126: Please add a reference for the use of the NINO3.4 index and its calculation.
- Page 6, line 236: ‘Increases’ should be ‘increase’.
- Page 15, caption figure 10: ‘Mean SE’ is missing in the caption of this figure if I compare it with the caption of figure 8, so please add this.

The rest of the paper is very well-structured and does not contain any mistakes.