Response to Referee #2.

Thank you very much for reviewing our manuscript.

Interactive comment on "Global temperature response to the major volcanic eruptions in multiple reanalysis datasets" by M. Fujiwara et al.
Anonymous Referee #2
Received and published: 23 June 2015

The paper focuses on the important scientific problem of quantification of climatic responses to volcanic eruptions in the second half of the 20th century using nine available reanalysis data sets. The authors study zonal mean latitude-altitude pattern of temperature response. The text is quite condensed and in parts could be more explanative. Despite an interesting work was done, the major objectives are not clearly formulated. They are not collected in one place but scattered throughout the paper. The conclusions are weak and not really informative. Please see the specific comments below.

Abstract: Please outline what is the major purpose of the study.

The major purpose of this study is to investigate the global temperature response to the volcanic eruptions using all available reanalysis datasets by highlighting common and different response signals among older and newer reanalysis datasets. An atmospheric reanalysis system provides a best estimate of the true atmospheric state and is an operational analysis system at a particular time (e.g., 1995 for the NCEP-1 system and 2009 for the JRA-55 system; see Table 1, the fourth column, of Mitchell et al. (2015)). The operational analysis system has been continuously improved at each reanalysis centre, with the main motivation to improve the tropospheric weather prediction (at least for the ECMWF, JMA, and NOAA). The consistency and difference among different reanalysis datasets will provide a measure of the confidence and uncertainty of our current understanding of the volcanic response. Finally, the intercomparison results of this paper can also link studies using only a single reanalysis dataset to other studies using a different reanalysis dataset.

We will add these points to the Abstract of the revised manuscript.

P 13318, L 17-20: Did you make any conclusions regarding data quality and reanalysis improvements?

The recent four reanalysis datasets, i.e., JRA-55, MERRA, ERA-Interim, and NCEP-CFSR, showed similar signals for the El Chichon and Mount Pinatubo eruptions from the 1979-2009 analysis. Thus, these four reanalysis datasets are equally good for studies on the response to these two eruptions. The NCEP-1, NCEP-2, and JRA-25 showed different tropical stratospheric signals particularly for the El Chichon eruption. The use of older analysis systems may be the cause of these different signals. For the JRA-25, the known stratospheric cold bias in the radiative scheme of the forecast model should be part of the reason. The 20CR has no QBO because upper-air observations were not assimilated, and thus is not suitable for the study of this kind. However, the 20CR applied volcanic aerosols in the forecast model and showed volcanic signals at least qualitatively. For the Mount Agung eruption from the 1958-2001 analysis, three out of the four reanalysis datasets analyzed, i.e., the JRA-55, ERA-40, and NCEP-1, except 20CR, showed similar stratospheric warming signals with somewhat varied magnitude and spatial extent. It is found that the ERA-40 showed unknown, warming signals in the mid-1970s, which are probably not realistic. Considering the discussion for the 1979-2009 analysis above, currently JRA-55 would be best for studies on the response to the Mount Agung eruption.

We will add these points in Conclusions of the revised manuscript.

P 13318, L 27-29: I disagree, there multiple examples of using reanalysis for comparison with model simulations.

We will rephrase this sentence as:
"Investigation of climatic response to individual volcanic eruptions using multiple reanalysis datasets for the purpose of comparison and evaluation of reanalysis datasets is rather limited."

There are several studies showing one or two reanalysis datasets to compare model simulations. But, most of the cases, they are the NCEP-1 and/or ERA-40 (e.g., Eyring et al, 2010; Karpechko et al., 2010). More recent studies used the ERA-Interim (e.g., Arfeuille et al., 2013; Toohey et al., 2014). But, more recent reanalysis datasets such as the JRA-55, MERRA, and NCEP-CFSR have not been used for the volcanic studies (except for our previous study by Mitchell et al. (2015)) to the knowledge of the authors.

Arfeuille, F., Luo, B. P., Heckendorn, P., Weisenstein, D., Sheng, J. X., Rozanov, E., Schraner, M., Brönnimann, S., Thomason, L. W., and Peter, T.: Modeling the stratospheric warming following the Mt. Pinatubo eruption: uncertainties in aerosol extinctions, Atmos. Chem. Phys., 13, 11221–11234, doi:10.5194/acp-13-11221-2013, 2013.

Eyring, V., Shepherd, T. G., and Waugh D. W. (eds.): SPARC CCMVal report on the evaluation of chemistry-climate models, SPARC Rep. 5, World Meteorol. Soc., Geneva, Switzerland, 2010.

Karpechko, A. Yu., Gillett, N. P., Dall'Amico, M., and Gray, L. J.: Southern Hemisphere atmospheric circulation response to the El Chichón and Pinatubo eruptions in coupled climate models, Q. J. R. Meteorol. Soc., 136, 1813–1822, doi:10.1002/qj.683, 2010.
Toohey, M., Krüger, K., Bittner, M., Timmreck, C., and Schmidt, H.: The impact of volcanic aerosol on the Northern Hemisphere stratospheric polar vortex: mechanisms and sensitivity to forcing structure, Atmos. Chem. Phys., 14, 13063-13079, doi:10.5194/acp-14-13063-2014, 2014.

P 13320, L 1-5: Please discuss your corresponding findings in the conclusion section.

We assume that you are referring to P 13319, L 1-5.
We will do this. Please see our answers to your question at P 13318, L 17-20.
In the revised manuscript, we will clearly describe this in the Conclusions section.

P 13321, L 1-21: There are number of other indexes, e.g., NAO, Indian Monsoon, why they are not included? Could you comment on this?

This is because we focused on the climate indices that are the forcing, not the response, and are relevant to the zonal mean response, not to the regional response. In an early phase of this study, we tested to include the Arctic Oscillation (AO) index, Antarctic Oscillation (AAO) index, and Indian Ocean dipole mode index (Saji et al., 1999), but the obtained volcanic response was found to be quite similar to the one without considering these indices. Also, there was discussion within the coauthors that the AO and AAO should be considered as response, not as forcing. We did not consider the Indian Monsoon index, but we think that it is more related to regional response, not zonal mean response.

We will add a note on this in the revised manuscript.

Saji, N. H., Goswami, B. N., Vinayachandran, P. N., Yamagata, T.: A dipole mode in the tropical Indian Ocean, Nature, 401, 360-363, 1999.

P 13322, L 9: It is really not clear and has to be explained.

This means that the regions evaluated as statistically significant are smaller than those in Mitchell et al. (2015) particularly for the solar and ENSO signals in the tropical lower stratosphere. In the revised manuscript, we will add this explanation.

P 13323, L 15: In linear approximation, bias should not affect a response to external forcing.

The cold bias of the forecast model was “not fully” corrected by the observations. This means that depending on the situation (e.g., at large volcanic eruptions or during a specific period of time) the correction by the observations might worse (or better) than other periods. It is possible that the bias was not constant over time, in particular when unusual, volcanically affected temperature measurements came into the JRA-25 system. So, we think this could be a part of the reasons. We will add this explanation. However, this is only a speculation, and thus we rephrase this sentence as follows:
“may” will be changed to “might”
“due to” will be changed to “related to”

P 13324, L 18-19: Repetition

We will insert the word “again.”

P 13326, L 15-25: It is most important that the Agung period is not covered by satellite observations. Could you please comment on this?

The weakness of the radiosonde dataset in comparison with the microwave and infrared sounders on operational satellites is its inhomogeneity in spatial distribution and their limited height range. The radiosonde stations are very limited in the Southern Hemisphere, and the typical balloon burst altitude is ~30 hPa (e.g., Seidel et al., 2011, their Figures 1 and 2). Therefore, the uncertainty is greater for the Agung signals than for the Mount Pinatubo and El Chichon signals, although we cannot quantify it easily.

We will add these points in the revised manuscript.

Seidel, D. J., Gillett, N. P., Lanzante, J. R., Shine, K. P., and Thorne, P. W.: Stratospheric temperature trends: our evolving understanding, WIREs Clim. Change, 2, 592-616, doi:10.1002/wcc.125, 2011.

P 13326, L 27-28: Why the surface temperature response is good then?

We did not say anything about surface temperature response.
To clarify, we will rephrase this sentence as:
“The modelled aerosol loading was probably too weak to simulate the lower stratospheric warming signals.”

P 13327, L 9-10: Disagree, the El Chichon plume was mostly in the northern hemisphere.

We will rephrase the sentence as:
"The aerosol loading due to the Mount Agung eruption extended primarily to the Southern Hemisphere, that due to the El Chichón eruption was very large in the tropics and extended primarily to the Northern Hemisphere, and that due to the Mount Pinatubo eruption was very large in the tropics and extended to both hemispheres."
and move this to the last paragraph of Section 3.2.

P 13328, L 15: Could you compare the optical depth of small eruptions with one of mt. Pinatubo.

We will completely remove the discussion on the temperature response to the three smaller-scale eruptions. See also our response to the comments by Reviewer #3.

P 13328, L 28: There are no physical reasons for small eruptions to produce qualitatively different response. It is probably an artifact of your signal-extracting procedure.

See above.

P 13330, L 12-14: Same as the previous comment.

See above.