



SDI FINAL EVALUATION FORM 1.1

PART 1:

Journal Name:	Physical Science International Journal
Manuscript Number:	Ms_PSIJ_23242
Title of the Manuscript:	Climate Sensitivity Parameter in the Test of the Mount Pinatubo Eruption
Type of Article	Original Research Article

PART 2:

FINAL EVALUATOR'S comments on revised paper (if any)	Authors' response to final evaluator's comments
<p>Review of "climate sensitivity parameter in the test of Mount Pinatubo Eruption"</p> <p>This paper reportedly presents an estimate of the climate sensitivity following a volcanic eruption which caused a reduction in heat to the Earth. The results from the model suggest a lower sensitivity than previous estimates and the author(s) present some rationale that more complex and sophisticated climate models over predict the Earth's climate sensitivity.</p> <p>This is a very interesting topic for both scientists as well as the general public. If this paper were correct, it would be wonderful news for society but also shake up our understanding of the Earth's climate. The problem is, both the logic and the mathematical formulation of the method.</p> <ol style="list-style-type: none"> 1. In the abstract, the author(s) claim that "this confirms that theoretically...." No, this paper doesn't confirm that. These time constants are too short and the methodology in this paper does not provide evidence that these short time constants are correct. In the response to earlier comments, the author claims that his/her direct observation of ice melting off the coast of Finland is a confirmation. This is demonstrably false. The speed of ice melt IS NOT the time constant of the thermal response. 2. In many other instances when responding to prior review comments, the author does not provide a defensible response. For instance when a reviewer pointed out that the author had selectively used papers that supported their claim, the response was "Because this paper is critical for the high climate sensitivity, the papers showing lower climate sensitivity have a greater role in my analysis." This is non-scientific. It is indefensible to only use those papers which support your argument. Furthermore, the author chose papers which have already been shown in the peer-reviewed literature to be incorrect (e.g. Lindzen and Choi 2011). 3. The author makes technical statements which are not true. For instance, claiming that according to HadCrut4, 2010 was hotter than 2014. 	<p>This is not the first paper by no means that shows a lower sensitivity. I have referred to several papers, which show a lower sensitivity in the range from 1.0 to 1.3 °C, which means the climate sensitivity parameter of 0.27 - 0.3 K/(Wm⁻²).</p> <ol style="list-style-type: none"> 1. The sudden temperature decrease itself during the Pinatubo eruption reaching the minimum in less than one year only, is evidence that the time constants of the Earth must be short. The time constants are not my invention but they are based on the other peer-reviewed paper. The simulation shows that the model changes follow very well the real temperature change rate. Even a rough analysis shows that a total recovery from the minimum value to the normal values happens in about 4 years. In the first order system, the recovery of the step change happens 98 % in the time, which is 4*time constant = 4 * 1 year = 4 years. I have not written about the ice melt speed. I have written that after the ice melt in the Finnish Gulf, the temperature increases from 0 °C to 20 °C in less than 4 months. This is not imagination, it is a simple fact. This response is due to the irradiation change of the Sun and this is the subject of this paper. 2. I have noticed that it is a common practise in the scientific papers to use mainly those papers as references, which show already the same kind of results. This is true especially in the case, when there is commonly known theory like positive water feedback. There is no need to refer to a number of papers in favour of this theory. The essential feature of this paper is to use the real data of Pinatubo eruption, and that is why I have referred to those papers using Pinatubo as an example for supporting the positive water feedback. I have referred to the paper of Lindzen and Choi (2011), which was published after criticism for their earlier paper (2009); they did not change essentially the results. Dessler had critical comments also after the corrections. 3. This statement was not in the paper but in the author's response for the earlier comments. I checked the latest version of Hadcrut4 and it is true that 2014 was warmer than 2010. This observation has no impact on my paper. 4. I have used both the average temperatures of satellite and surface temperatures. I



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<p>http://www.metoffice.gov.uk/news/release/archive/2015/2014-global-temperature</p> <p>4. The author admits, in responding to the earlier review that satellites are not surface temperatures. So my response is, why not use surface temperatures? Why continue to use satellites?</p> <p>5. Figure 1, the author claims that satellite temperatures are surface temperatures. No they are not.</p> <p>6. Figure 2, over what period is this? It doesn't say. What are the light and dark bars to signify?</p> <p>7. Line 72, the author claims that the same development is assumed to occur over the southern hemisphere. There is no justification for this.</p> <p>8. Line 74 ,the author claims that the zenith angle is likely important They could show this by showing results by month of the year, as the zenith angle changes.</p> <p>9. Line 79, the author claims a “global delay called a dead time is estimated to be 1.6 months.” First, how is this supported? Is it by comparison of the minima in Figure 2? Second, how can this be longer than the time constant of 1.04 months? This is logically inconsistent.</p> <p>10. Line 84 the author assumes that radiation changes happen simultaneously over the globe. This isn't true.</p> <p>11. Lines 97-98, the author states that only early studies that are relevant to this subject are used. What does this mean? Is the author saying that only papers which support his/her conclusions are included?</p> <p>12. Lines 117-118, there are other ways to measure surface imbalance, such as ocean heat content. The authors claim that LWDN fluxes cannot be estimated is not supported.</p> <p>13. Line 1378, shouldn't SWIN be SWSRF?</p> <p>14. Line 157, what is the basis for the statement that ash clouds and water-vapor clouds have the same effect?</p> <p>15. Line 164, why is the author only considering some of the literature?</p> <p>16. Lines 182-183. You don't need Pinatubo eruptions to make a conclusion</p>	<p>have used satellite temperature measuring the lower atmosphere, because the two earlier papers analysing the climate sensitivity based on the Pinatubo eruption have used the satellite temperatures.</p> <p>5. I have corrected the text of Figure 1 for the next version.</p> <p>6. I have corrected the text: “The percentage values show the maximum decreases of the apparent transmissions after the eruption and they are represented by light bars inside the normal apparent values (total bar length). The period is thus the total length of the eruption.</p> <p>7. I have already added more text in V2/V3 for supporting this claim: “The sites in Fig. 2 cover almost 85 % of the northern hemisphere. Thomas [8] has analyzed the global apparent transmission measurements after the eruption. The analysis shows that the aerosol cloud was covering the latitudes from 60S to 60N after three months and practically uniform over the hemispheres after six months. This is also the moment of the maximum temperature decrease. The main role in spreading the cloud had planetary scale waves in high latitudes, which transported the volcanic aerosol from the tropics to high latitudes. I had also Appendix 2 in my earlier response showing graphically that this claim is justified.</p> <p>8. The maximum zenith angle during the day is always smaller in the high latitudes than in the tropics or in the mid-latitudes. This fact is not affected by the tilted axis of the Earth, which changes the length of the day during the year. Anyway I have changed this sentence to be: “This phenomenon can compensate the effects of possible thinner cloud conditions.”</p> <p>9. The delay can be seen in Fig 1. This statement shows that reviewer does not understand the basic facts of the dynamic system. The delay time is caused by the development of the hazy cloud over the globe and it took about 1.6 months before the average impact of the eruption had its impacts on the temperature. The time constants of land and sea (1.04 and 2.74 months) describe the cooling of the Earth caused by the irradiation changes, which start to affect after the dead time. The simulation results show that these time constant are very accurate indeed. If the time constants of the land and sea would be longer – like the heat content change of the ocean – the dynamic response for this eruption would be much, much longer.</p> <p>10. There is further evidence in my earlier comment that after 6 months of eruption, the eruption cloud had uniformly spread over the globe.</p> <p>11. The statement is this form: “Therefore the survey of the earlier studies covers only the subjects which are relevant for this study”. This statement means that I have not covered all the papers written about the Pinatubo eruption. I have recognised the major fields of these papers and all of them are not really relevant to this paper. There is no indication in this statement that I would include only those papers, which are in favour of my study.</p> <p>12. In the temperature anomaly caused by the Pinatubo eruption, the heat content change of the ocean is limited to the mixing layer of the ocean (time constant of 2.74 months), which has a depth of about 75 meters. I have referred to GEWEX project and the results show that LWDN flux at surface could not be estimated by LWUP flux measurements at TOA.</p> <p>13. This comment is correct and I have changed SWIN to be SWSRF.</p> <p>14. I think that I have described the ash cloud effect to water cloud effects in the paper quite well. In both cases a cloud reduces the incoming solar radiation but increases at the same time the downward LW radiation.</p> <p>15. I have added two other references into this section.</p> <p>16. Definitely not but this is the best of those experiments offered by the nature itself.</p> <p>17. It is a global and I have added this word into the text.</p> <p>18. I think that it is correct that IPCC has its own model, because the model giving the total RF value of 2.34 Wm^{-2} cannot be found in any single research paper.</p>
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<p>about the sign of the water vapor feedback.</p> <p>17. Is Figure 4 global? It isn't clear.</p> <p>18. Lines 194-195, I don't think this is correct.</p> <p>19. Line 199, from what year is the 1.17C temperature change begun? Preindustrial?</p> <p>20. Why is this author citing predominantly third-rate journals with extremely light reviews when papers published in top-tier journals give the exact opposite conclusion. You simply cannot choose to cite poor research and selectively ignore higher quality work that contradicts your conclusion. Examples are references 10, 11, 29, 35...</p> <p>21. Line 210, First, there is a grammatical error in this sentence. Second, what does it mean?</p> <p>22. Line 212, the author claims that a TSC (should be TCS) can be reached in less than a year. This is not true. What definition of TCS would make this statement true?</p> <p>23. Line 214, the author incorrectly states that the only essential feedback in a TCS calculation is water vapor. This is not true.</p> <p>24. Lines 218-224, first the author has chosen low-sensitivity outliers and ignored middle and high-sensitivity studies. Second, Lindzen and Choi 2011 has been shown to be incorrect (Dessler). Third, most of these are based on short term temperature records which, when you include the most recent years (like 2014 and 2015) are already known to be underestimates.</p> <p>25. Line 223, what does this mean "the two most common values"?</p> <p>26. Lines 263-264, why do you assume that these are reflected the same way?</p>	<p>19. IPCC states in its summary graphical presentation that the value RF values are referred to the year 1750.</p> <p>20. The simple reason is that the papers, which are critical for AGW theory, are very difficult to get published in the top-tier journals. The reason is well-known, which can be found in the Climategate emails. I have some experiences from these journals. The editors (not reviewers) have sent a negative decision by justifying the decision that "This subject is not interesting enough for our readers...". The subject was the climate sensitivity, which one of the key issues in the climate change science.</p> <p>21. I am not sure about what sentence is addressed by this comment, because the line numbers are not the same as in my revised papers received from the publisher. Anyway I suppose it is a question about the magnitude of the positive water feedback. In AR 4 the factor was 2 and now in AR5 it is from 2 to 3. Conclusion: IPCC's knowledge of water feedback has become more inaccurate.</p> <p>22. There is no TSC in version V2/V3 anymore, because I have checked this by using the tool included in Word. In the version V3 there is no this kind of claim. The sentence claiming tha TCS can be reached in less than a year has been removed from the versions V2 and V3.</p> <p>23. The λ value of $0.5 \text{ K/(Wm}^{-2}\text{)}$ includes the positive water feedback, and it gives the CS value of $0.5 * 3.7 = 1.85 \text{ }^{\circ}\text{C}$ for CO_2. The λ value of $0.27 \text{ K/(Wm}^{-2}\text{)}$ does not include any water feedback, and it gives CS value of $\sim 1.0 \text{ }^{\circ}\text{C}$ found in some papers. The Technical Summary of AR5 states on the page 82: The water feedback/lapse rate, albedo, and cloud feedbacks are the principal determinants of equilibrium climate sensitivity (ECS). The changes of albedo and cloud feedbacks are explanations for the double value of ECS in comparison to TCS. So far there are no observations about these changes. If the reviewer knows other essential feedbacks than water, it should be mentioned in this comment and the reviewer should give a reference.</p> <p>24. The question of climate sensitivity is still under debate in the climate change science and my paper is an example about this situation. The question of cloud feedback is also one of these unresolved issues, even though Dessler (2011) shows in his paper that Lindzen & Choi (2011) as well Spencer & Braswell (2011) have calculated their results on wrong bases. Even the sign of cloud forcing varies among scientists. Clouds can be a cause and an effect. There are many papers about cloudiness (Svensmark for example) showing that cosmic rays can affect the climate by changing the amount of clouds. ONI shows that El Nino started in March 2015 and therefore the measured temperatures of 2015 cannot be included into the temperature trend before the La Nina is over somewhere late 2018...</p> <p>25. "The two most common values" means that the values of 0.27 and 0.5 are those, which can be found in the climate change science literature. If the reviewer has another opinion, it should be mentioned here.</p> <p>26. There are two references (Russell and Minnis) who state that the LW warming effect is in maximum after few months of the eruption and it is related to the larger particle sizes. The greatest particle size graph is the best estimate about the LWDN flux. It follows very well the graphs of LWUP fluxes but the magnitude of the change is greater. This can be expected, because the LWUP flux is not the same as LWDN but is delayed and depending on the surface temperature change (the very phenomenon, we are studying...)</p> <p>27. I do not call UAH temperatures as surface temperatures and Fig. 8 just illustrates the differences between various data sets. I have added a comment that UAH temperature measure lower atmosphere temperature.</p>
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<p>27. Figure 7, UAH is not a surface temperature.</p> <p>28. Line 300, what does the author mean by saying the effects of ENSO are controversial? Which effects are controversial?</p> <p>29. Studies on the effect of ENSO on global temperatures have already been one.</p> <p>30. The model used in this paper is not explained, is not one-dimensional, and is not correct. Where is Equation (3) from? This is not a one dimensional equation. Typically, one dimensional means that there is a single space dimension (like an atmospheric column or an ocean column). This model doesn't have that.</p> <p>31. What is the physical basis of Eq (2) and (3)? What are "out" and "in" referring to? Heat flows?</p> <p>32. What is the physical basis of equations (4) and (5)?</p> <p>33. Figure 12 doesn't provide a logical physical feedback process because there is no physical mechanism.</p>	<p>28. I have shown with two examples that different papers give different results about the the actual size of ENSO during the Pinatubo eruption. I have added the word "quantified" for showing that the problem is in numerical values only. Actually by comparing the temperature graphs of Soden et al. (UAH direct and UAH with ENSO correction), one can estimate that the correction is about -0.2 °C in maximum and my estimate has a maximum value of -0.23 °C – about th e same.</p> <p>29. I refer to the point 28.</p> <p>30. I have changed the name of the model to be just a dynamic model. Equation (3) is the same as Eq. (1) including the dynamic effects, i.e. the time constants of land and sea exactly determining the delays in the temperature caused by the flux changes.</p> <p>31. Equation (2) has been described quite well in the text. The SWIN and LWDN fluxes are the same fluxes as described in Fig. 6 and 7 varying according to the time. In eq. (2) are only fluxes. In eq. (3) the input is a net flux change, which is transformed into temperature by multiplying by lambda and thus the output is temperature change varying according to the time. I have changed equations (2) and (3) into time domain because the Laplace domain may be pretty odd for those not having experience of dynamic analyses.</p> <p>32. As explained in the text, the eq. (4) is the discrete form of eq. (3) applicable for dynamic analyses by computers, when the input signal varies along the time. Eq. (5) is a well-known mathematical formula of a PI-controller (proportional-integral) and also in this case I have changed the formula into time domain.</p> <p>33. It is right that Fig. 12 does not provide explanation but the text does so. I have explained that this is a theoretical approach, which can be found in the natural processes, because that is how the self-regulating systems work. This example shows that the results are pretty good even though we should not know the mechanism exactly. In this case we do know that the LWDN flux has a vital role and finally the ash cloud vanishes along the time.</p>
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