

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 the Article	Original Research Articles

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PART 1: Review Comments		
	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)
Compulsory REVISION comments	<p>This paper reportedly uses a 1 dimensional climate model to simulate Earth surface temperatures caused by the eruption of Mount Pinatubo to test various values of the climate sensitivity parameter. This issue has been looked at in the past and is of interest to the climate science community as well as the general public. However this paper is deeply flawed.</p> <p>First, there are many typographical and grammatical errors.</p> <p>Second, there are basic conceptual errors. For instance, the author(s) state that MSU temperatures measured from satellites are surface temperatures. I don't think this is correct. Typically MSU temperatures are from various layers in the atmosphere ranging from the lower troposphere.</p>	<p>I have corrected all errors as pointed out by the reviewer.</p> <p>It is true that the MSU temperature is not exactly the Earth's surface temperature but the conventional surface measurements are also carried out in the air above the surface. The two earlier studies (Hansen et al. 1996 and Soden et al. 2002) used UAH MSU measurement as a reference temperature. I have used this temperature mainly for this reason but also an average temperature of four temperature measurements. In the Appendix 1 is a comparison of surface measurements and satellite measurements. This graphical presentation shows that the difference between the temperatures is relatively small from 1979 to 1987. During the Pinatubo eruption the UAH MSU temperature declines a little bit more than the other temperature measurements.</p>

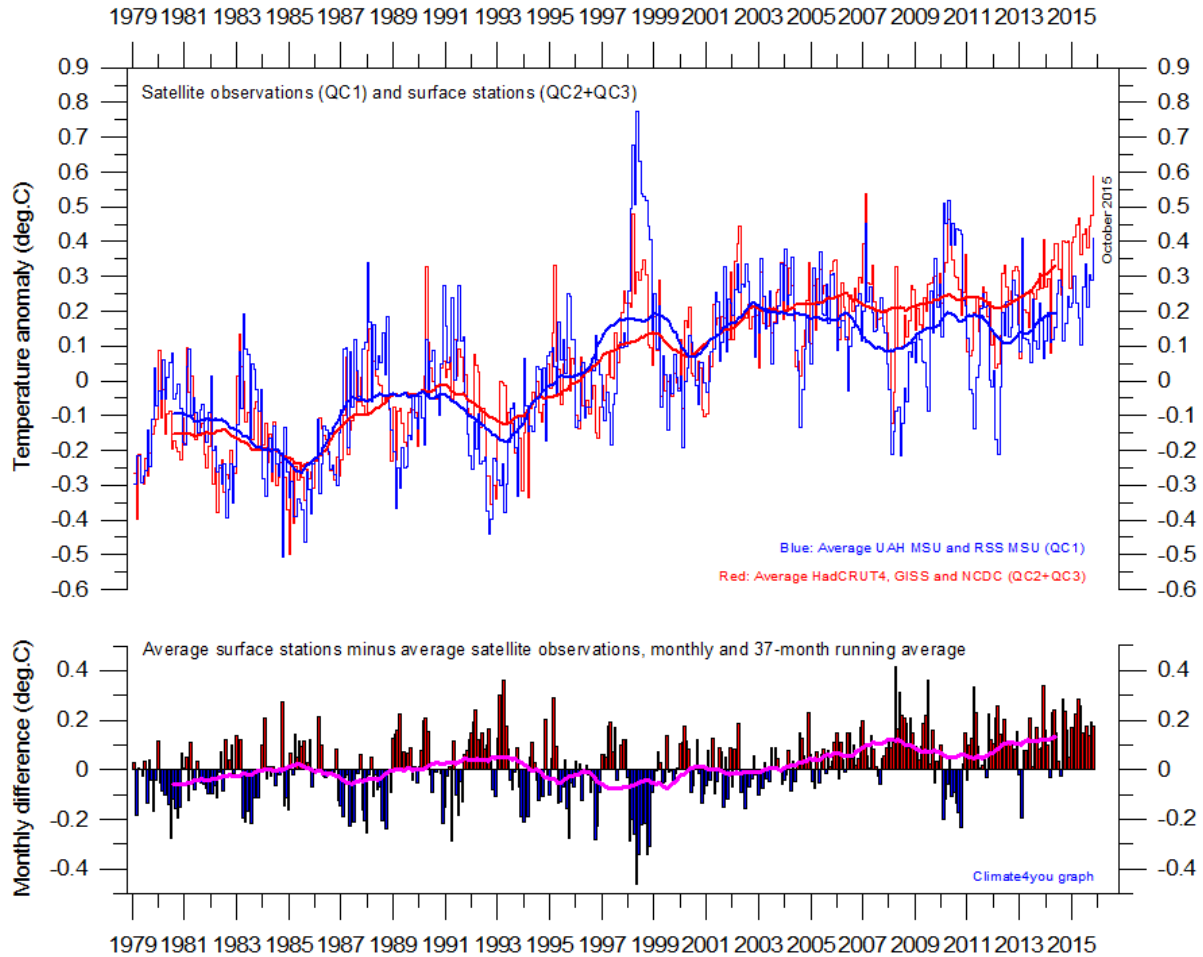
<p>The images are very poor quality and hard to read, particularly the text.</p> <p>Little or no measurement uncertainty is provided.</p> <p>How are the five sites from figure 2 representative of 85% of the northern hemisphere? With these differences, how can the author(s) state that what happened in the north is assumed to be in the south? Perhaps more importantly, why wouldn't the authors just utilize already available publications on apparent transmission?</p> <p>The author(s) comment about missed opportunities to measure downward flux changes following volcanoes but they should note that the 2010 volcano they cite is very different from one like Pinatubo which ejected large amounts of aerosols high into the atmosphere.</p> <p>The thermal fluxes in Figure 3 are too simplified and neglect some significant terms.</p> <p>Certain statements make no logical sense. For instance, on line 138, it is stated that "In the case of..." Do the author(s) mean to say that W/the difference in flux between the cases is 11 or are they saying something else?</p> <p>The author(s) make unsubstantiated claims, just as the comparison of ash clouds with water vapor clouds.</p> <p>On line 175 the author(s) claim that prior researchers included a positive water vapor feedback. There really is no doubt</p>	<p>Personally I keep the images clear and readable. Anyway I have increased the font sizes of the scales.</p> <p>I have added a separate analysis into the text.</p> <p>I have added a description about the spread of the aerosol cloud based on the study of Thomas (2008). The graphical presentation is available in Appendix 2.</p> <p>Because this is only a remark about the missed opportunity, and it has no added value for this study, I have removed this part from the text. The point of this remark was, that there was an opportunity to measure the downward flux change in order to get an idea about its behaviour.</p> <p>The purpose of Fig. 3 is to show only the main fluxes of the Earth's energy balance, which have the essential role in this study and which are repeatedly used. The complete figure of the energy balance would be too big without adding any essential information. Other earlier research studies of this subject (Minnis et al, Hansen et al. and Soden et al.) do not show any figures of essential radiation fluxes.</p> <p>I have changed this description to show that LWUP flux change at the TOA and the LWDN flux change at the surface are not equal. Using the LWUP change as a radiation forcing element is not the same as the real forcing change at the surface.</p> <p>There is an analogy between the normal clouds and ash clouds. Both clouds reduce the incoming SW solar radiation at the surface and both clouds increase the LWDN radiation at the surface. In the case of normal clouds the LWDN radiation increases from the all-sky value of 345 Wm^{-2} to the value of 359 Wm^{-2}. All the important earlier studies (Minnis et al. 1993, Hansen et al. 1992 and 1996 as well Soden et al. 2002) recognize that the LWDN flux increases during the eruption compensating a major part of the cooling effect caused by the decreased SWDN flux change.</p> <p>The question of positive water feedback is one of the argued properties of climate models. Just simply looking at the RH trends (a new Fig. 4), the</p>
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	<p>about this. Are the author(s) claiming the magnitude is unknown? The discussion related to Figure 4 appears to confuse the sign of water vapor feedback with short term changes in the term. This is like confusing a derivative with the value of a function,.</p> <p>The discussion associated with Eq 1 is incorrect. First, the author(s) make a math error. Multiplying .5 with 3.7 gives 1.85 (not 1.75 as the author(s) state). Citation [27] has two different years (2001) and (2007). The IPCC reports don't "use" a value of the parameter, they report ranges of the parameter based on multiple lines of evidence. It is not true to say the IPCC reports this as a nearly invariant parameter.</p> <p>The author(s) says that "there should not be any of IPCC's own climate models..." What does this mean? Of course, the IPCC doesn't have its own models. The IPCC is a summary of work done by various research teams. This statement is not logical. Why doesn't the author(s) use the more recent predictions from the AR5 report from 2013? Why go back to 2001 anyway?</p> <p>The author(s) make statements about forcing and temperature change but do not give time frames for these to occur. The author(s) makes a statement that suggests the IPCC does not show temperature changes and models in the most recent assessment but in fact they do. The writing of this paper is very confusing and difficult to follow. The</p>	<p>assumption of the constant RH is not correct and it means that the positive water feedback is not a scientific fact. I have added the reference from AR5, which states "that the positive water feedback can amplify any forcing by a typical factor between two and three" (The Physical Science Basis, p. 667). There are no references for this statement in AR5. I have a short analysis in which way Soden et al. have used the water content and it is highly questionable.</p> <p>The error of 1.75 has been corrected to be 1.85. Citations years of AR4 and AR5 have been checked and now they are correct in References and in the text. The AR5 was published in 2013 but the values in the report are from the year 2011. The descriptions about the IPCC way to use λ, has been corrected: In AR4 λ was still almost invariant parameter but in AR5 IPCC does not keep λ anymore almost a constant parameter. I have added an analysis about the linearity and variations of λ based on the spectral analysis calculations. IPCC does give any description about the value of λ in AR5. It seems to vary a lot based on each analysis in question. In RCP analyses the λ value is 0.37 K/(Wm⁻²) without any explanation (mainly CO₂ concentration changes). The average value of TCS, which is based on the CO₂ concentrations only, is still about 1.75 °C. The same results can be calculated using the λ value of 0.5 K/(Wm⁻²). There is a great conflict between these warming values and λ values. If a private researcher would use a parameter in this way, it would be questioned. The text has been revised based on these observations. See Appendix 3 and 4 for further evidence.</p> <p>Officially IPCC should not carry out its own research work and therefore this issue seems to be sensitive. But the fact is that IPCC has selected research results, which support their idea about the anthropogenic global warming (AGW) including the positive water feedback. The selected results have been combined to form a presentation by name "Radiative Forcing by Emissions and Drivers" which leads to the total forcing value 2.34 Wm⁻² in 2011. The corrected text includes a reference to AR4, because then the water feedback factor was two and in AR5 (The Physical Science Basis, p. 667) it is now in the range from two to three.</p> <p>I cannot connect this comment to any specific points in the text. I have not found in The Physical Science Basis of AR5, what is the temperature change in the year 2011, which is caused by the radiative forcing of 2.34 Wm⁻². I have asked this question from the Finnish Meteorological Institute, which is the solid supporter of IPCC: no answer. If this information can be found, I am pleased to</p>
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<p>statements in lines 203-206 are non-sense. The IPCC doesn't "use" a lambda value in this way.</p> <p>N line 211, the author(s) introduce a term EQS. What is this? No description. Also, the comments on line 210, "A so high lambda..." makes little sense. What is the term TSC? Is this a misprint of TCS?</p> <p>The author(s) claim that TSC (whatever that is) can be reached in less than a year. Please support this. If you mean the transient climate sensitivity (TCS), this is typically the value of the temperature change when CO2 increases by 1% per year. The temperature is obtained at year 70 (not in less than a year). If the temperature change occurred in less than a year, what is the ocean-layer thickness? How can oceans heat that fast?</p> <p>The author(s) states that there is no lambda value of the GCMs but it appears that they misunderstand that the climate sensitivity is an output of the GCMs, not an input.</p> <p>The author(s) cites some studies which report lower values of the climate sensitivity than the central estimate of the IPCC. But, the author(s) rely upon a paper that was published in 2011 and four following papers found errors in the 2011 article [35]. In fact, the authors of [35] have conceded the errors. Why would the author(s) not acknowledge this and also, why are they just listing selective papers which find a low sensitivity? There are many papers which find a higher sensitivity than the IPCC central estimate. Why aren't they mentioned? And even further, the papers cited here rely upon instrumental temperatures but they are all outdated with the two recent very hot</p>	<p>correct my text. I have corrected the formulation in which way IPCC uses the lambda value. As shown before, the lambda values vary a lot anyway. Equation (1) is still the only connection between the forcing values and the warming values. The total forcing value of 2.34 Wm^{-2} by IPCC does not make any sense, if there is no method to calculate the corresponding temperature increase.</p> <p>I have removed this part of the text about the term Equilibrium Climate Sensitivity (ECS). In this study it is a question about changes happening during few years only and therefore ECS is not essential in this analysis. The TCS misprint has been corrected.</p> <p>I have removed the remarks about TCS concerning the time dependency of the temperature change. This is not relevant in this study, because I have used the time constants of the ocean and the land found reliable in earlier studies. The ocean's time constant is really 1.04 months. Which means that the output of the step input change has come to a new equilibrium after the time of $4 \times 1.04 = 4.4$ months (98 %). I live on the coast of the Finnish Gulf. In April there is an ice cover on the sea, in the beginning of May the surface temperature is 0°C and in the end of July the temperature is about 20°C. The whole change happens in less than about 4 months for a rapidly increasing insolation change. The ocean has a mixing layer of about 75 m deep, which is well mixed. Below is the intermediate and deep ocean without mixing properties. The temperature change from the mixing layer into the deep ocean is very slow indeed, because the main mechanism is conduction.</p> <p>The references for the lambda values have been removed except the statement that Hansen et al. and Soden et al. do not show any lambda values, which would be the outputs of the applied GCMs.</p> <p>The positive water feedback is a well-known and the essential feature of GCMs. Because this paper is critical for the high CS, the papers showing lower CS values have a greater role in my analysis. Lindzen and Choi published the first version in 2009 about the CS based on the ERBE data and they reported the CS value of 0.5 K. Three critical papers were published commenting the shortcomings of this paper. Lindzen and Choi published another paper in 2011 (reference in my paper) in which they admitted the four areas of critics and carried out new analyses. The final result was that the CS was 0.7 K. This paper is critical to the high CS values used by IPCC. Those papers finding lower CS – typically $1.0 - 1.2^\circ\text{C}$ – utilizes different methods including the measured temperature values. There</p>
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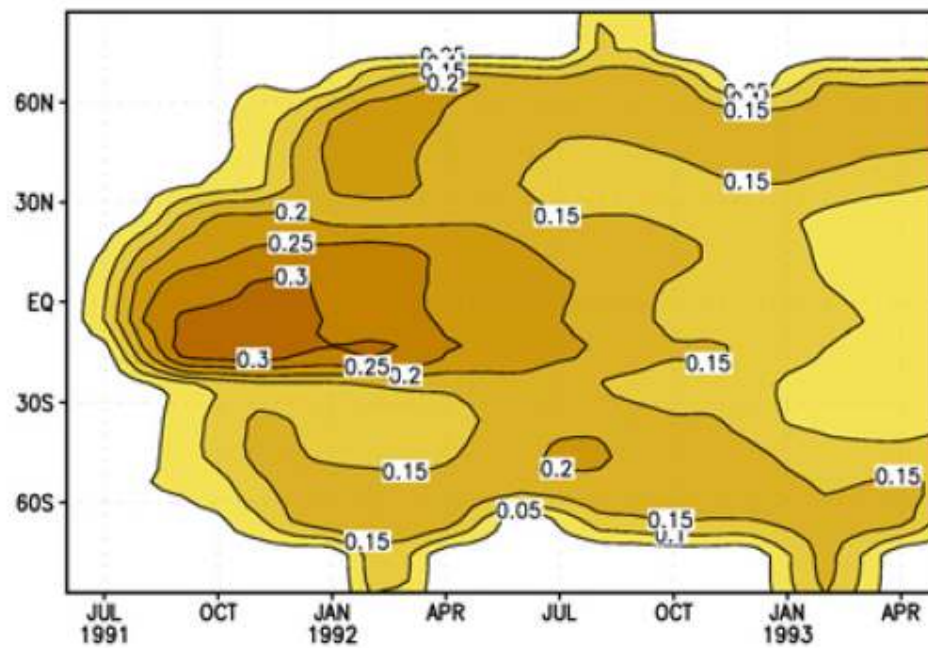
	<p>years. Why not include that in the discussion?</p> <p>There are many other tenuous claims that are made but it is noteworthy that the author(s) don't even begin to present their model until line 320. In Equation 2, the authors state that the "climate process is a combination of two parallel processes..." What does this mean? What is Equation 2? Saying something is a climate process is like saying nothing. The units don't work out in the equation. The denominators on the right have terms with different units. There is no description of justification of Equation 2. The numerical scheme treats inflows and outflows at different time steps. Regardless, this isn't a one-dimensional model anyway.</p>	<p>are about 20 published paper showing lower CS values but it is not practical to refer to all of them. The year 2014 was not hotter than year 2010 according to three temperature data bank sets of UAH MSU, UAH RSS, and HadCRUT4. The individual years are not decisive in the discussion of the climate change. The recent trend since the El Nino & La Nina of 1998-2002 is essential, because there has been no temperature increase leading into the great difference between the IPCC model values (also the majority of GCM values have the same error) and the measured values. The present El Nino started in February 2015 according to the ONI values and therefore it takes about 4 years (a typical duration of El Nino & La Nina period), before anybody can draw any conclusions about the real global temperature increase or decrease.</p> <p>The description of the dynamic climate process has been revised making also the units working together. The parallel processes mean simply that the Earth is the combination of the ocean (70 %) and land (30 %), which have different time delays. The temperature change happens simultaneously in the both elements. The details of these time delays can be found in the references. The results show that the values of the time delays must be very near the correct values. I would still call this model one dimensional, because there are no longitude, latitude or altitude dependencies. The only variables are the disturbance fluxes $\Delta SWIN + \Delta LWDN$ and the time has been expressed in the word "dynamic". The phrase multidimensional or two-dimensional dynamic model would give a wrong image about the nature of this model.</p>
Minor REVISION comments		
Optional/General comments		

Appendix 1. Comparing surface and satellite temperature estimates: <http://www.climate4you.com/>

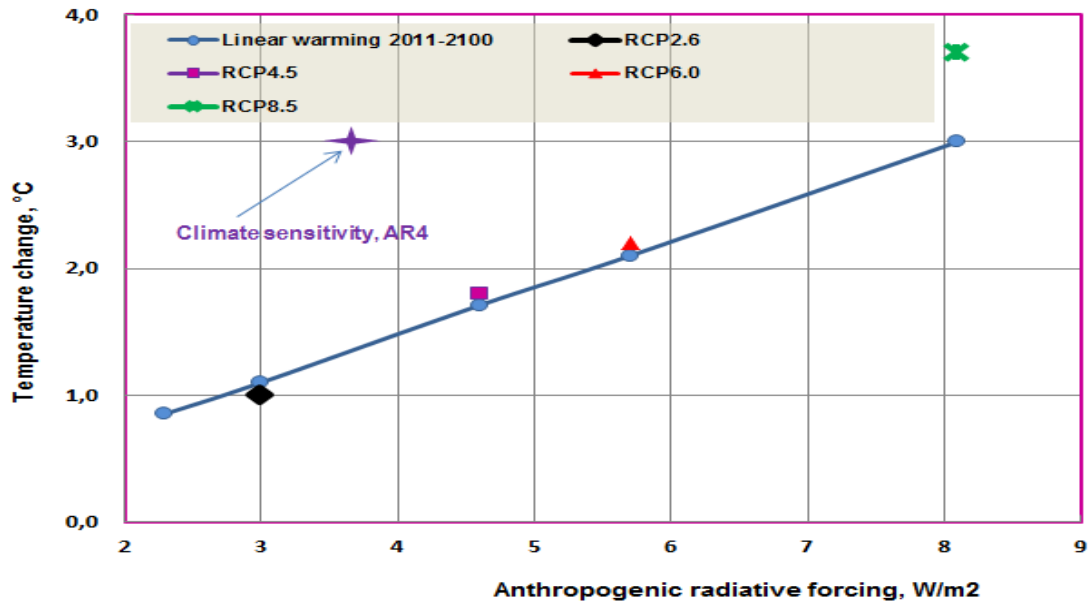


Plot showing the average of monthly global surface air temperature estimates ([HadCRUT4](#), [GISS](#) and [NCDC](#)) and satellite-based temperature estimates ([RSS MSU](#) and [UAH MSU](#)). The thin lines indicate the monthly value, while the thick lines represent the simple running 37 month average, nearly corresponding to a running 3 yr average. The lower panel shows the monthly difference between surface air temperature and satellite temperatures. As the base period differs for the different temperature estimates, they have all been normalised by comparing to the average value of 30 years from January 1979 to December 2008. Last month included: October 2015. Last diagram update: 21 November 2015.

Appendix 2. Zonally averaged Pinatubo aerosol optical depth at $0.55\ \mu\text{m}$ for two years after the eruption according to Thomas, 2005, page 15, <http://www.atmos-chem-phys.net/9/757/2009/acp-9-757-2009.pdf>



Appendix 3. The global warming values according to anthropogenic radiative forcings of RCPs. The RCP6 means the radiative forcing of 6 Wm^{-2} . The graph named as “Linear warming 1750-2011” has been calculated using the linear coefficient of $0.85 \text{ K} / 2.29 \text{ Wm}^{-2} = 0.37 \text{ K}/(\text{Wm}^{-2})$. This graph shows that IPCC still uses the linear equation $\Delta T = \lambda * \text{RF}$ but λ values vary case by case.



Appendix 4. Global warming values according to the different models and estimations.

