New York City
16 September 2020

Dear Dr. Palm:

Just a note to draw your attention to the comments provided by the Referee #2. As you must have read, apart from a few small corrections to the text, that referee makes no claim whatsoever regarding possible mistakes or flaws in the analysis we have performed. He simply thinks that our "interpretation" is incorrect.

In case you have not followed the debate on this subject, many colleagues working on volcanoes and climate were convinced by a few papers in the early 1990's (based on little data, and low resolution models), that volcanic aerosols cause WARMING at high latitude in winter. We think those papers are not of good quality, and that the proposed "winter warming theory" is simply not supported by the more recent models and observational studies. As you can imagine, our claim is meeting with resistance: it is always an uphill battle to "change the paradigm", as the old guard is heavily invested in the old view.

We have published one paper in ACP last year, with a lot of evidence that there was no such warming caused by the 1993 Pinatubo eruption. Our manuscript is now adding more strong evidence from the 1993 Krakatau eruption. Needless to say, those who for many decades were claiming the existence of a volcanic warming find it difficult to accept our new work. Hence our strongly worded reply to Referee #2, whose interpretation we vigorously rebut.

Just to be clear: there is absolutely nothing personal here. We simply hold opposite views. We hope you will agree that we are entitled to our own interpretation of the results, as long as the results are not incorrect. If the colleagues wish propose an alternate interpretation of our results, they are more than welcome to do so by publishing their own studies offering a different view: we will be delighted to read their papers, and offer our thoughts in reply. That is how a healthy and fair scientific debate is carried out in the peer-reviewed literature.

We trust you understand the situation.

Our very best regards.

Lorenzo Polvani and Suzana Camargo
Columbia University

(The authors' replies to the referees are in bold)

REPLY TO REFEREE #1

The Authors present a case study of the the Krakatau eruption of August 1883 and its impact on wintertime temperatures in the Eurasian region. Compelling evidence from observations and simulations is provided to support the conclusion that the warming in the area following Krakatau was unrelated to the eruption.

The paper is very interesting, well-written and to-the-point, and a pleasure to read. It is well suited for ACP and provides enough new scientific information to justify publication. Overall, the paper presents a nice piece of work.

I have one specific comment. In the Summary/Discussion, the Authors generally note that each eruption is unique, and list some affecting factors. However, the impact of these in the presented case is not discussed. This should be done because the Authors are making rather general conclusions based on a single event. I would be particularly interested in some elaboration on the effect of QBO and ENSO, because there are studies that seem to point out their importance for the "stratospheric pathway" and NAO modulation, e.g. for the top-down solar influence. At least, the Authors should state the conditions during the Krakatau eruption and make a comment on the possible implications regarding their conclusions. Also, were these conditions similar during the 1992 Pinatubo eruption? I am looking forward to the Authors' response on this.

We thank the referee for the kind words. Following his/her suggestion, we have added a new paragraph to the last section, discussing the potential QBO/ENSO effects on both the Krakatau and Pinatubo eruptions.

Minor corrections:
a) Page 3, line 77: check the years, 1978 should be 1878.

Yes, this was a typo. We have fixed it. Thank you.

b) Page 8, line 240: "uncorrelated correlated" should be "weakly correlated".

Corrected. Thank you.

REPLY TO REFEREE #2

This paper focuses on the "volcanic winter warming" theory that stratospheric aerosols from volcanic eruptions cause changes in atmospheric circulation, that lead in turn to warming over the northern Eurasian continent in the 1 or 2 winters after the eruption. The study looks specifically at the 1883 eruption of Krakatau, and presents analysis of surface temperature reconstructions, reanalyses and climate model output. The conclusions of the study are very similar to those of an earlier study (Polvani et al., 2019) which focused exclusively on the 1991 eruption of Pinatubo. The main conclusion of the paper is that "the observed warming over Eurasia in the winter of 1883/84 was, in all likelihood, unrelated to the Krakatau eruption". Taken together with the prior paper on Pinatubo, the authors argue that volcanic winter warming is not a real phenomenon for eruptions of this magnitude, and that the warm Eurasian temperatures in the winters after these two eruptions were chance occurrences resulting from natural climate variability.

The paper is provocatively written, and indeed a major promise of the study is its direct challenge of the winter warming theory. Where earlier studies have raised doubts about selected components of the overall theory, this study aims to call into question its very validity. The topic is certainly open for scientific debate, and there is room for critical perspective.

However, this study contains numerous fallacies which undermine the logical argumentation. Most generally there are two main problems. First, the authors disregard the observational basis of the winter warming theory--it is mentioned in passing only once in the introduction. The fact that models do not reproduce the expected winter warming signal is perplexing, even disappointing, but it is no reason on its own to disbelieve observations. Secondly, the potential influence of volcanic aerosol on circulation or continental surface temperatures--in the single realization of reality--cannot be assessed by focusing on a single eruption. The observational basis for the winter warming theory has established that the signal is within the range of natural variability but is detectable because of its consistency across eruptions: the observational studies identified winter warming or positive NAO anomalies by compositing observations after more than 10 eruptions (e.g., Robock and Mao, 1992; Christiansen, 2008). It is only because of the statistical significance of the observed winter warming across many eruptions that one may interpret the modest observed Eurasian warming after Krakatau as being linked to the eruption. By focusing on a single eruption, and by neglecting the observational basis, the study fails to mount a valid challenge to the volcanic winter warming theory.

We are grateful to Dr. Toohey for a careful reading of our manuscript. We are delighted to read that he sees nothing wrong with the methodology, or with the results of our study: he simply questions our interpretation. Specifically, he notes "two main problems", which we here address in turn.

(1) We do not, in the least, disregard the "observational evidence": we consider it unreliable. We refer to it twice in the paper: in the introduction section, and again in the conclusion section. In our previous paper (Polvani et al 2019, hereafter PBS19) we have discussed in great detail why the claims made by the handful of papers which have proposed the winter warming theory are, in our opinion, not robust . We saw no reason to repeat that material here. However, for the sake of completeness, in the revised manuscript we have now explicitly pointed the reader to the discussion in PBS19. It can be found on line 319 of the revised manuscript.

(2) Secondly, the referee claims that "the potential influence of volcanic aerosol... cannot be assessed by focusing on a single eruption." We could not agree more. Having analyzed the 1992 Pinatubo eruption in our earlier paper (PBS19), in this manuscript we study in detail the next big eruption for which we have good observations: the 1883 Krakatau eruption. In fact, in upcoming papers, we will report on earlier eruptions as well. However, we firmly believe that averaging over many eruptions -- especially over eruptions of different MAGNITUDES (as done in nearly all papers in this subject) -- is fundamentally incorrect. In seeking a forced response, the AMPLITUDE of the forcing matters. One would never think of averaging the responses in an RCP4.5 scenario with those of an RCP8.5 scenario. So, why do some many papers in this field blithely average responses from volcanoes of greatly different magnitudes (e.g. Tambora and El Chichon, which differ by more than a factor of 4)?

For the moment we have carefully analyzed -- separately -- Pinatubo (in PBS19) and Krakatau (in this paper), and the results are very consistent. Therefore, in the final section of the paper, we draw the appropriate conclusions from these. We do not believe there are any "fallacies" in our methodology or our reasoning.

Specific comments:

L6: The first main finding is that "observed post-Krakatau winter warming over Eurasia was unremarkable (only between 1- and 2-sigma of the distribution from 1850 to present)." However, prior studies do not suggest that warming over Eurasia after any single eruption is necessarily remarkable. Robock and Mao (1992) show temperature anomalies from 12 eruptions since 1883: Eurasian anomalies rarely exceed +3C, are in some cases negligible, and on average suggest a mean warming of ~1-2C. As another example, Christiansen (2008) showed that in the winters after 13 eruptions from 1880-2000, 11 winters showed a positive NAO. The NAO magnitude in each of those years is unremarkable--and the mean NAO anomaly is a very pedestrian ~0.6--but what is remarkable is the consistency of the post-eruption anomaly. The repeated description of the Krakatau winter warming as "unremarkable" is in no way evidence against the winter warming theory, in fact, that amount of warming is quite consistent with what one would expect based on observational studies.

The reason we employ the word "unremarkable" is that previous studies to date have not placed the post-eruption warming in the context of natural variability, while at the same time making big claims about the importance of volcanic eruptions in affecting winter surface temperatures at high latitudes. Need we remind the referee that Alan Robock had a paper in Science (2002) touting the Pinatubo eruption as a prime example of the warming theory, when observations clearly show the stratospheric polar vortex was not even anomalously strong that winter (as noted in later studies)?

Now, the referee argues that "what is remarkable is the consistency of the post-eruption anomalies". But is that really so? As already noted, we do not believe those claims are robust. Let us just deal with Robock and Mao (1992), the first to propose the warming theory. Did the referee notice that 6 of the 12 eruptions in that paper are not even in the tropics? Why did those authors mix in so many high-latitude eruptions for which the stratospheric pathway does not apply? And why did they pick the first winter for some eruptions and the second for some other eruptions? One could go on...

In any case: the point we wish to stress in our paper is that, when a post-eruption surface warming is seen (as in the case of Krakatau) the amplitude of that warming is NOT LARGE compared to the internal variability. This is what we are adding to the discussion in the first section of our paper: we show here that the post-Krakatau anomalies are largely INDISTINGUISHABLE from natural variability, and thus they are unremarkable. In simpler terms: for those living in Eurasia, the post-Krakatau winter would look little different from many other warmer-that-average winters which were not preceded an eruption.

In any case, the referee agrees with us that the anomalies are unremarkable, and requests no correction. So we have made no changes to the manuscript.

L7: The second finding is that "reanalyses indicate the existence of very large uncertainties, so much so that a Eurasian cooling is not incompatible with observations". This finding does not follow from the results shown. First, the phrase "not incompatible with observations" obscures the fact that only 3 out of 56 ensemble members in the reanalysis produce negative Eurasian temperature anomalies. Based on the ensemble, one should conclude that Eurasian cooling was very unlikely. Secondly, the statement refers vaguely to "observations" that "a European cooling is not incompatible with", without specifying that these observations are only the surface pressure observations that are assimilated into the reanalysis. Without more careful language, a reader might easily understand that a European cooling is not incompatible with all observations. But given the surface temperature reconstructions based on temperature measurements described in the study, this is clearly wrong. Overall, the conclusion seems to be an attempt to decrease confidence in the observation of a Eurasian winter warming after Krakatau, but there just isn't any reasonable way that results from 3/56 ensemble members from a reanalysis assimilating surface pressure measurements can have any influence on our understanding of Eurasian temperatures which are based primarily on actual temperature measurements. In contrast, the fact that the reanalysis ensemble mean "temperature anomalies are in good agreement with the observations" can only increase our confidence that Eurasian temperatures in winter 1883/84 were indeed warmer than normal.

We appreciate the correction: we have rephrased this sentence, and similar sentences occurring later in the paper, to make it more precise. The point here is not "to decrease confidence in the observation of a Eurasian winter warming after Krakatau", but to "to decrease confidence in the fact the warming was caused by circulation changes, and thus by the NAO, and thus by the polar vortex, and thus by the volcanic aerosols". This is the point we are trying to demonstrate, and the 20CR reanalyses definitely add evidence to support our claim. We apologize for not stating this accurately.

L8: The crux of the author's argument then comes down to the third finding, which is that "models robustly show the complete absence of a volcanically forced Eurasian winter warming". Based on this finding, the authors later conclude that "low-latitude eruptions as large as Pinatubo or Krakatau are unable to cause a forced surface temperature anomaly over Eurasia that can be distinguished from unforced variability". While the absence of winter warming in present-day model simulations is perplexing, model results cannot be used as a basis to discount a theory based on observations. The authors barely mention the observational basis of the theory, focusing more on describing the "stratospheric pathway" mechanism proposed by the early studies. This represents a "straw man" fallacy: the author's attack on the "stratospheric pathway" mechanism is justified, but this is not a valid argument against the observation-based winter warming theory itself.

First, we disagree with the statement that "the author's argument then comes down to the third finding". The first and second findings add much evidence which has not, to date, been presented. Second, the modeling evidence for a lack of surface warming in state-of-the-art models is OVERWHELMING, and it is NOT PERPLEXING at all. It makes a clear case for the fact that the stratospheric pathway is not operative in the models. The referee again brings up "the observational basis of the theory" which, as we have already discussed is highly questionable. But neither models nor observations indicate the presence of that pathway. So there is no "straw man" here: there is simply an emperor with no clothes!

L17: No justification needs to be given for a summary of background literature. It may be true that extant literature is "confusing and often contradictory", but this is hardly unique to this scientific topic, and to label prior work so might come across to some readers as a rhetorical tactic to undermine confidence in prior work.

The referee is correct: there are many scientific topics with a literature full of claims and counterclaims. But that very fact speaks for itself. All papers agree that increasing CO_2 warms the earth, and that makes it a well established fact. Conversely, the huge "confusion and contradiction" that plagues the volcanic warming theory literature clearly indicates that it is NOT well established at all. We think it is important to emphasize this, to encourage the colleagues to critically read the published literature.

L24: This "in a nutshell" description of the stratospheric pathway cites only papers from the 1990s, and neglects recent work that has both challenged the simple "meridional temperature gradient" mechanism and investigated other mechanisms, including planetary scale waves and tropospheric eddies (e.g., Toohey et al., 2014, Bittner et al., 2016, DallaSanta et al., 2019).

We have cited the seminal papers that propounded the "stratospheric pathway theory". Several subsequent papers, realizing that the original theory did not work, invoked progressively more complicated and unlikely mechanisms, none of which has proven robust. For instance, the tropospheric "meridional temperature gradient" theory proposed by Stenchikov et al (2002) has been invalidated by DallaSanta et al (2019), who found that "a naive argument that the stratospheric warming increases the equator-to-pole temperature gradient (and so strengthens the polar vortex) cannot qualitatively predict the simulated response". In the introduction to our paper, we do not think it is helpful to confuse the reader with these later claims and counterclaims.

L32: It is not true that "very little aerosol is left in the stratosphere in the second post-eruption winter". Satellite-based retrievals of stratospheric aerosol optical depth (AOD) after Pinatubo show that the peak in global mean AOD was around winter 1991/92, with a value of ~0.1. One year later, the AOD is around 0.68, still elevated by an order of magnitude above the background value of ~0.06. A similar result can be found if one looks particularly at the AOD in the tropical region. There may be valid pragmatic reasons to limit focus to the first post-eruption winter, but the statement that there is "very little aerosol is left in the stratosphere in the second post-eruption winter" is not true, and this claim should not be used to invalidate prior studies which averaged 2 post eruption winters.

"Very small" may not be the correct expression, so the referee has a point. However, the fact remains that the amount of volcanic aerosols left in the second winter is a small fraction of the one present in the first winter. We have rephrased this sentence to make that clear. We are grateful for the correction.

L34: Stating that these papers all failed to replicate the results of earlier studies is very much oversimplifying their work. For example, Bittner et al. write: "For eruptions of the size of Krakatau and Pinatubo, the multi-model ensemble shows a strengthening of the polar vortex in the first post-eruption mid-winter, which challenges the assumption of a general failure of coupled climate models to simulate the dynamical response to volcanic eruptions." Also, Wunderlich and Mitchell (2017) do not present any results from CMIP5 models regarding the winter warming: they explore winter warming in reanalyses and present from some CMIP5 models simulated tropical temperature anomalies.

We beg to differ. The early papers reported A CLEAR AND ROBUST WINTER WARMING at the surface following volcanic eruption. Bittner et al (2016), in contrast, do not show or even discuss the surface warming in their model (their entire paper is narrowly confined to the stratosphere). Why? If one looks at the Ph.D. thesis of Dr. Bittner, one discovers the fact -- surely deeply embarrassing to the authors, and therefore hidden away on Figure 6.4 on page 85 of that dissertation -- that the their model shows NO FORCED SURFACE WARMING IN WINTER over Eurasia after the Pinatubo eruption, even after averaging 100 simulations. That work, just to mention one example cited by the referee, TOTALLY FAILED to replicate the early observational studies and modeling studies. So, with all due respect, we are not oversimplifying: we are calling a spade a spade.

L41: While some of the earliest model studies used very small ensemble sizes, most past studies used ensembles of some reasonable number and quantified the statistical significance of the ensemble mean response, taking into account natural variability. It is therefore unjustified to claim that "much of the earlier literature had failed to properly account for the large internal variability associated with the stratospheric polar vortex and with the NAO".

Again, we politely disagree with the referee. Yes, many recent studies have analyzed ensembles of model runs. But most used them incorrectly. Nearly all papers we are aware of compared to the ENSEMBLE MEAN of their model (or models) to the observations. As they found no warming in the model MEAN, they concluded the models were unable to reproduce the observations. The key point that was missed is that, for each eruption, the observations are just one realization of the system, and thus should not be compared to the mean of many model runs. We invite the referee to reread Driscoll et al (2012), who analyzed the CMIP5 models, just to cite one example: in their conclusion they state

> **"None of the models simulate a sufficiently strong reduction in the geopotential height at high latitudes, and correspondingly the MSLP pressure fields and temperature fields show major differences with respect to the observed anomalies. This is despite some models having 10 ensemble members, giving a potentially strong signal-to-noise ratio."**

See what they're saying? "We have averaged as many as 10 members, and yet the surface temperature in the models does not look like the observations: ergo, the models must be wrong." We believe this is a methodological flaw that afflicts most of the extant literature on this subject, in addition to an uncritical acceptance of the observational claims.

L164: The number of deaths caused the Krakatau eruption has absolutely no bearing on the expected relative magnitude of the winter warming signal, as the number of deaths depends strongly on the population living in proximity to the volcano.

Of course, we agree with the referee. But the reason for citing that number here is simply to remind the reader how destructive that eruption was. Krakatau is universally described as a truly cataclysmic event. And yet, as we show in our paper, the OBSERVED warming anomalies over Eurasia in the following winter were TOTALLY UNREMARKABLE, because the internal variability of the extratropical atmospheric circulation is very large, and an events as large as the 1883 Krakatau eruption is unable to overcome it. This is the key point of our work: that internal variability is MUCH LARGER than the impact eruptions as big as Krakatau (and Pinatubo).

L224: The response of the two most extreme ensemble members illustrates that there is natural variability, and that the anomaly in any one post-volcanic year may vary from case-to-case, but it does not negate the possibility of a non-zero mean response, i.e., a higher probability of either negative or positive anomaly.

We are agreed. The large spread between the two extreme members "does not negate the possibility of a non-zero mean response". However, all the studies based on CMIP3 and CMIP5 models report PRECISELY a zero mean response, including the 100-member ensemble analyzed in our paper for Krakatau. Showing the two extremes is the best way to illustrate just how LARGE that variability is, and thus how tiny the forced response is, a fact that had not been appreciated until very recently. Hence our emphasis.

L231: "acceleration"

Thanks for flagging this typo. We have corrected it.

L257: Wunderlich and Mitchell didn't look at winter warming in the CMIP5 models.

Yes, they looked only the NAO, which they found NOT to be anomalous after large volcanic eruptions, in agreement with our results and many other papers: BUT the whole reason for their study was to understand the lack of winter warming in those models. So, that citation is entirely appropriate in context.

L258: This misrepresents the results of Stenchikov et al. (2006) who state in their abstract "The IPCC models tend to simulate a positive phase of the Arctic Oscillation in response to volcanic forcing similar to that typically observed. However, the associated dynamic perturbations and winter surface warming over Northern Europe and Asia in the post-volcano winters is much weaker in the models than in observations."

Well... the claims in the abstract of Stenchikov et al (2006) regarding the CMIP3 models are, we fear, not quite representative of what one reads in the paper itself.

First, they only analyze 7 models, most of which are low-top, and hence with very poor stratospheric resolution. Second: two of the nine eruptions averaged in that study (Tarawera and Bandai) are not in the tropics which, again, confuses things. Third, they average both the the first and second winters, which confuses things even more; also, for 2 eruptions the winters were shifted by 1 year as the eruptions occurred in October (we find this indefensible, and adds even more confusion). Fourth: only 3 of the 7 models (i.e. less than half!) show any statistically significant warming over Eurasia at all (see their Figure 2), and in all cases it is much smaller than the observed one. So, this is pretty thin evidence for in support of the winter warming theory via the stratospheric pathway, using models which don't even simulate the stratosphere.

But there's more. The follow-up study with the CMIP5 models (Driscoll et al, 2012), explicitly says that the models in Stenchikov et al (2006) did a poor job in simulating the winter warming response to volcanic eruptions, and that the CMIP5 models, alas, are no better! We quote from the Driscoll et al study (S06 is Stenchikov et al, 2006):

> **"With substantially different dynamics between the models it was hoped to find at least one model simulation that was dynamically consistent with observations, showing improvement since S06. Disappointingly, we found that again, as in S06 , despite relatively consistent post volcanic radiative changes, NONE OF THE MODELS MANAGE TO SIMULATE A SUFFICIENTLY STRONG DYNAMICAL RESPONSE." (emphasis ours)**

So there you have it: the authors are "disappointed" that NONE of the CMIP5 models agrees with observations, and state that these models are no better than the CMIP3 models analyzed by Stenchikov et al (2006). We agree with them.

Footnote 4: For the sake of balanced consideration of prior work, reference to Zambri and Robock (2016) should come in the introduction rather than here near the end of the paper. Also, editorial commentary characterizing the work as "a single dissenting voice" or "not, to date, ... independently reproduced" is clearly rhetoric meant to undermine confidence in this study, and would benefit from being recast in more objective and quantitative terms.

Why should we mislead the readers early on in our paper, by giving the false impression that there are two equally-weighted sides to the story, when in fact ALL the studies which analyzed the CMIP5 models are unanimous in reporting the lack of a winter warming, EXCEPT for that one paper? The Zambri and Robock (2016) paper is an outlier, and no other study to date has independently backed their claims. This is an objective statement, is it not?

A FINAL NOTE OF REASSURANCE: We sincerely hope Dr Toohey will not be taken aback by the strong tone of our response to his comments. We hold him in great respect, and are very grateful for his time and consideration. While we strongly disagree on the validity of the winter warming theory, we look forward to his reply and will carefully consider it.

Scant evidence for a volcanically forced winter warming over Eurasia following the Krakatau eruption of August 1883

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Abstract. A recent study has presented compelling new evidence suggesting that the observed Eurasian warming in the winter following the 1992 Pinatubo eruption was, in all likelihood, unrelated to the presence of volcanic aerosols in the stratosphere. Building on that study, we here turn our attention to the only other low-latitude eruption in the instrumental period with a comparably large ~~Volcanic Explosivity Index (VEI)~~magnitude: the Krakatau eruption of August 1883. We study in detail the temperature anomalies in the first winter following that eruption, analyzing (1) observations, (2) reanalyses, and (3) models. Three findings emerge from our analysis. First, the observed post-Krakatau winter warming over Eurasia was unremarkable (only between 1- and 2-σ of the distribution from 1850 to present). Second, reanalyses based on assimilating surface pressure alone indicate the existence of very large uncertainties, so much so that a Eurasian cooling is not incompatible with ~~observations~~those reanalyses. Third, models robustly show the complete absence of a volcanically forced Eurasian winter warming: we here analyze both a 100-member initial-condition ensemble, and 140 simulations from the Phase 5 of Coupled Model Intercomparison Project. This wealth of evidence strongly suggests that, as in the case of Pinatubo, the observed warming over Eurasia in the winter of 1883/84 was, in all likelihood, unrelated to the Krakatau eruption. ~~Together with the results~~ This, taken together with a similar result for Pinatubo, ~~we are led~~ leads us to conclude that if volcanically forced Eurasian winter warming exists at all ~~,~~ an eruption with a magnitude far exceeding these two ~~(VEI=6) events is needed~~ events would be needed to produce a detectable surface warming.

1 Introduction

The vexed question of whether large, low-latitude volcanic eruptions are able to cause a winter warming of the continents in the Northern Hemisphere is receiving renewed attention. As the extant literature is quite confusing and often contradictory, a brief summary of how this topic has evolved will help set the stage for the present study.

The surprising idea of a post-eruption *warming*, obviously at odds with the naive expectation of a surface cooling from additional reflection of incoming short-wave radiation by the volcanic aerosols, was championed by a series of early observational studies (Groisman, 1992; Robock and Mao, 1992, 1995). These were accompanied by modeling studies (Graf et al., 1993; Kirchner et al., 1999) which reported a causal relationship between strong eruptions and a positive phase of the North Atlantic Oscillation (NAO) in winter. These early studies were highly influential because they proposed a cogent physical mechanism linking low-latitude eruptions to high-latitude surface temperatures anomalies. In a nutshell: the lower stratospheric tropical warming associated with the absorption of long-wave radiation by the volcanic aerosols increases the equator-to-pole stratospheric temperature gradients, resulting in an anomalously strong polar vortex leading, in turn, to a positive NAO phase, and eventually a surface warming over Eurasia in the winter months (Kodera, 1994; Perlwitz and Graf, 1995). We will refer to this mechanism as the “stratospheric pathway”.

While such a mechanism appears plausible, these early observational and modeling claims have not stood the test of time. For instance, analyzing 15 eruptions in one temperature reconstruction extending back to 1586, Fischer et al. (2007) found that the largest winter warming over Europe seems to occur in the second post-eruption winter, not the first. If confirmed by other reconstructions, this result would obviously negate the stratospheric pathway mechanism, as ~~very little aerosol is~~ the amount of aerosol left in stratosphere in the second post-eruption winter is a small fraction of the amount present in the first winter, and no physical process has been proposed that would provide ~~such a~~ a multi-year long memory in the polar stratosphere and the NAO. Even more perplexing are the numerous studies (Thomas et al., 2009a; Driscoll et al., 2012; Toohey et al., 2014; Bittner, 2015; Wunderlich and Mitchell, 2017, just to cite the most recent ones), which have analyzed, literally, dozens of state-of-the art climate remodels and repeatedly failed to replicate the early modeling results. Averaging over many different low-latitude eruptions, over many models, and over many runs, these more recent studies have consistently reported the lack of a statistically significant post-eruption surface winter warming over Eurasia. This begs the question: how could the later, better models fail to simulate the causal connection reported with the earlier, simpler models?

A resolution to this conundrum was recently proposed by Polvani et al. (2019), hereafter PBS19, who showed that much of the earlier literature had failed to properly account for the large internal variability associated with the stratospheric polar vortex and with the NAO. Focusing primarily on the ~~199~~ 1991 Pinatubo eruption, PBS19 analyzed three “large ensembles” of model runs, and showed how tiny initial perturbations of the *same* model forced with the *same* volcanic aerosols result vastly different winter temperature anomalies over Eurasia. They also showed that, averaging over many runs, i.e. computing the “forced” response, yields insignificant post-eruption anomalies, in agreement with most recent studies. The early modeling studies, it so happens, simply lacked the vertical and horizontal resolution to properly simulate the internal variability of the Northern Hemisphere circulation, and thus yielded spurious results. PBS19, therefore, concluded that the observed Eurasian surface warming in the winter of 1991/1992 was, very likely, *not* caused by the preceding eruption of Mt Pinatubo.

That conclusion appears unassailable, since PBS19 merely corroborated the lack of a forced post-eruption winter warming already reported by many previous studies with many other models. Nonetheless, one might argue that the 1991 Pinatubo eruption was somehow peculiar, and thus unrepresentative of most, large, low-latitude eruptions. To determine if this is so different eruptions need to be examined. PBS19 did, in fact, examine the ~~1983~~ 1982 El Chichón eruption (Robock, 1983) with the same three large ensembles, and again concluded that the winter warming over Eurasia in the winter 1982/83 was, in all likelihood, *unrelated* to El Chichón. But this is perhaps unsurprising, as the El Chichón eruption was of smaller magnitude

~~what~~ than the Pinatubo eruption. What is really needed, it should be clear, is a low-latitude eruption at least comparable in magnitude, if not larger, than the last Pinatubo eruption.

Hence this study: building on PBS19, we here examine the eruption of Mt. Krakatau in August of 1883. There are several reasons for focusing on that eruption. First, it is the only low-latitude eruption that occurred during the instrumental period with a magnitude comparable to the 1991 Pinatubo eruption (Robock, 2000), in both volcanic explosivity index (VEI) and dust veil index (DVI=1000): this means that we can establish, with some confidence, whether Eurasia was anomalously warm or cold in the first winter after the eruption. Second, the Twentieth Century Reanalysis (Compo et al., 2006) starts in 1851, and thus includes that eruption~~: in~~. In fact, not ~~only~~ a single renalaysis, but a 56-member ensemble of that reanalysis is now available, so that the observational uncertainties can be quantified. Third, the historical integrations of dozens of models participating in Phase 5 of the Coupled Model Intercomparison Project (hereafter CMIP5; Taylor et al., 2012) also start at 1850: there is, therefore, a wealth of model output that covers that eruption, and we will examine both a large initial condition ensemble (100 members), and the multi-model CMIP5 ensemble.

As we will show in detail below, this wealth of data allows us to establish the three facts: 1) that Eurasia was indeed warmer than average in the winter following the ~~1993~~ 1883 eruption of Mt. Krakatau, but 2) that the anomalous warming that winter was in no way exceptional, and 3) that it is unlikely that volcanic aerosols were the *cause* of, or even substantially contributed to, that warming. After a brief section on methods, each of these facts will be discussed in detail in Sections 3-5. The paper will close with a summary and discussion.

2 Methods

2.1 Definitions

For each of the datasets detailed in the following subsection, we quantify the potential impact of the 1883 Krakatau eruption by computing a *winter temperature anomaly*, defined as the difference between the surface temperature in the winter 1883/84 (i.e the average of December 1883, January 1884, and February 1884) and the mean over a reference period, chosen to be five previous winters (i.e. from ~~1978~~1878/89 to 1882/1883). For brevity we will sometime refer to this quantity as "the post-Krakatau anomaly", or simply "the anomaly." Unless otherwise stated, the length of five seasons is chosen for the reference period throughout this paper; this is done to remain consistent with PBS19, so that the post-Krakatau anomalies may be quantitatively[1] compared with the post-Pinatubo anomalies. In the figures, we will use ΔT_s to refer to these temperature anomalies.

[1]Stenchikov et al. (2006), Driscoll et al. (2012), Bittner et al. (2016a) and most other studies, opted to use reference periods of different lengths for different eruptions. We understand their motivation for doing so: compositing the longest possible number of unperturbed years prior to each eruption. Our goal, however, is a little different: we are trying to validate the Pinatubo findings of PBS19 with a different eruption. Hence, we prefer to use the same reference period as ~~theirs~~PBS19, to avoid introducing a source of potential confusion. In any case; we shown below that the results are largely insensitive to the length of the reference period.

Furthermore, as explained in ~~details~~ detail in PBS19, and also suggested by Zambri and Robock (2016) and Bittner et al. (2016a), there is no plausible reason to consider the second winter after the eruption, as no study to date has proposed a credible mechanism that would allow anomalies caused by the volcanic aerosols to affect the stratospheric polar vortex and the NAO beyond the first winter. Finally, to compute time series and probability distribution functions, the average Eurasian anomaly is computed as the mean over the region (40-70°N, ~~0-15~~0-150°~~0W~~W), as in PBS19. We will refer to this as the “Eurasian box.” Our results are insensitive to this choice, as one can see from the actual anomaly maps, shown throughout the paper.

2.2 Datasets

To construct a comprehensive and convincing picture of potential impact of the 1883 Krakatau eruption on wintertime Eurasian temperatures, we here analyze three different types of surface temperature data: observational reconstructions, reanalyses and model output. We describe each one in detail in the following subsections.

2.2.1 Observations

Three distinct datasets which have constructed observational estimates of monthly-averaged temperatures are here analyzed:

1. The NOAA merged Global Surface Temperature (NOAAGlobalTemp) dataset, version 4.00, spanning the period January 1880 to present (Smith et al., 2008; Vose et al., 2012). We will refer to this as the NOAA anomalies.

2. The GISS Surface temperature analysis, version 4, spanning the period January 1880 to present (Lenssen et al., 2019). We will refer to this as the GISTEMP anomalies.

3. The Climate Research Unit (CRU) air temperature anomalies, version 4, spanning the period January 1850 to present (Jones et al., 2012). We will refer to this as the CRUTEM anomalies.

While all three cover the 1883 Krakatau eruption, both the NOAA and GISTEMP products start at 1880, so only three winters are available to define a pre-eruption reference period. We test the robustness of the estimated anomalies as a function of the reference period in Section 3 below.

2.2.2 Reanalyses

At the time of this writing, the majority of available reanalyses reach back to 1979, the start of the satellite era, or a little earlier (e.g. JRA-55, Kobayashi et al., 2015, which starts at 1958), with only a few extending back to the beginning of the 20th century (e.g. ERA-20C, Poli et al., 2016): unfortunately, none of these serve our purposes. However, the NOAA-CIRES Twenty Century Reanalysis, Version 2c (hereafter 20CR, Compo et al., 2011) extends back to January 1851, and can thus be used to study the 1883 Krakatau eruption. Recall that 20CR only assimilates sea level pressure measurements (Compo et al., 2006), while the underlying atmospheric model is forced with ~~SST~~ sea surface temperature reconstructions (Rayner et al., 2003) and time varying CO_2 concentrations, incoming solar radiation and volcanic aerosols. More importantly, we here analyze a 56-

member ensemble of 20CR: this allows us to quantify the uncertainties associated with the post-eruption Eurasian temperature anomalies.

2.2.3 Models

It so happens that a huge amount of model output is available to study the ~~1882~~ 1883 Krakatau eruption. To a large degree, this is due to the fact that the CMIP5 project recommended that all so-called "historical" integrations should be initialized at the year 1850. Over that period, the CMIP5 also specified how models ought to be forced, with both natural (i.e., solar and volcanic) and anthropogenic (e.g., CO_2) forcings, in order be directly comparable with observations. Specifically, we here analyze two distinct datasets.

1. First, we consider the 100-member of historical simulations of the so-called "MPI Grand Ensemble" produced by the Max Planck Institute for Meteorology (hereafter MPI, Maher et al., 2019). The atmospheric component of that model (MPI-ESM-LR) has a horizontal resolution of 1.9° (approximately, at the equator), and 47 vertical levels with a model top at 0.01 hPa. As such it is a so-called "high-top" model, with a good representation of the stratospheric circulation and, more importantly, of its variability. We note, in passing, that the other widely ~~used~~ analyzed large ensemble (Kay et al., 2015) was initialized at the year 1920, and therefore cannot be used for the purposes of this study. All simulations in this ensemble are performed with an identical model configuration and with identical forcings, following the CMIP5 protocol: they only differ in their initial conditions. This ensemble of runs allows us to unambiguously quantify the importance of internal variability, and contrast it with the forced response.

2. Next, we consider the CMIP5 historical (1850-2005) integrations: the output of 40 distinct models is available, with many groups contributing small ensembles of runs (typically 3 to 10 members), for a total of 140 historical runs. The specific models analyzed here, and the number of integrations available, are listed in Table 1. We examining all available simulations from these models, sorting the models into two categories: high-top and low-top, following the classification in Charlton-Perez et al. (2013) and Rea et al. (2018). If, as argued in the early studies, the stratospheric pathway is indeed operative, one should be able to see a clear difference between the models with a good representation of the stratosphere and those with a poorer representation.

3 Observations

We start by determining whether, in the first winter after the Krakatau eruption of August ~~1993~~1883, Eurasia was anomalously warm or anomalously cold. It is important to stress that for most eruptions occurring before the 19th century, not only is the strength of the eruption difficult to estimate, but even the surface temperature anomalies over Eurasia are only known with considerable uncertainties, as those eruptions predate the instrumental period. Hence the 1883 Krakatau eruption offers the only opportunity – other than the 1991 Pinatubo eruption – to examine a large, low-latitude event for which the surface temperature anomalies can be determined robustly.

The post-Krakatau wintertime anomalies, for the CRUTEM dataset are illustrated in the top row of Fig. 1. Focusing first on the middle column, where the December to February (DJF) mean is shown, we see a clear warming of the Eurasian continent. This independently confirms the result shown in Fig. 1 of Robock and Mao (1992), and again in Fig. 1 of Shindell et al. (2004), obtained with an earlier ~~version of the temperature reconstruction of the CRU (Jones and Moberg, 2003), with a slightly different methodology~~versions of the CRU temperature reconstruction, and with slightly different methodologies. The post-Krakatau warming in DJF is further corroborated by the other two reconstructions we have analyzed, the NOAA and GISTEMP datasets, shown in the middle column of the bottom two rows of Fig. 1, respectively. Note, however, that the reference period for those two datasets is shorter (only 3 winters) than the standard 5-winter period used throughout this paper, as those reconstructions do not extend far enough into the past.

One might then legitimately wonder to what degree these anomalies might depend on the length of the reference period used. To address that concern, in Fig. 2 we show the post-Krakatau anomalies computed with respect to a 3-, 5- and 10-winter reference period from the CRUTEM product, the only one for which this calculation can be performed. As one can see, the warming in DJF is very robust: averaged over the Eurasian box, we obtain a mean warming of 1.75°C, 1.89°C and 1.91°C for, respectively 3-, 5- and 10-winter reference periods. We conclude, therefore, that the post-Krakatau DJF temperature anomaly is robust in the observations, irrespective of the dataset and reference period one uses.

Having established that a relative warming over Eurasia did, in fact, occur in the first winter following the ~~1993~~ 1883 Krakatau eruption, we now ask two important questions: (1) how persistent was that anomaly, and (2) how unusual was it? To shed some light on the persistence, we have plotted in left and right columns of Fig. 1, respectively, the November to January (NDJ) and January to March (JFM) anomalies. While Eurasian warming is present in NDJ, one can see that it turns into a cooling in JFM, notably to the east of the Ural Mountains. Examination of individual months (not shown) reveal a clear cooling over the Urals starting in February, and becoming large enough in March to dominate the JFM average. This suggests that, at least for this event, the warming anomalies over Eurasia did not persist into the late winter. Note that the seasonal polar vortex breakdown typically occurs around April 15 (as estimated from reanalyses, over the period 1981-2016, by Butler et al., 2019): hence the presumed stratospheric pathway would be operative in the month of March, but it does not appear to be persistent.

Finally, focusing again on the DJF months, we address the question of whether the post-Krakatau winter warming anomalies over Eurasia were, in some way, exceptional. Recall that the 1883 eruption of Mt. Krakatau was a cataclysmic event, causing over 36,000 deaths and producing a global cooling that lasted for several years, with temperatures not returning to normal until 1888 ~~(Judd et al., 1888; Simkin and Fiske, 1983)~~(Simkin and Fiske, 1983). And two modeling studies have even claimed that Krakatau may have so strongly impacted Southern Ocean temperatures as to alter ocean heat uptake for many decades (Fyfe, 2006; Gleckler et al., 2006). In such a context, one ~~would~~ might expect the 1883/84 Eurasian temperature anomalies to clearly stand out.

It is perhaps disappointing, therefore, to discover that the post-Krakatau anomalies are far from exceptional, when compared to all the winters available in the instrumental record, as shown in Fig. 3a. In that figure we plot the probability distribution function (PDF) of wintertime (DJF) anomalies averaged over the Eurasian box, computed using all the available winters in the CRUTEM datasets (from 1850 to present), with the post-Krakatau winter highlighted by the blue vertical line. Notice how that line falls almost evenly between the one-σ and two-σ (dashed) lines: with a mean value at 1.89°C, the winter 1883/84 falls within the 85th percentile of the distribution. This is confirmed by the PDFs computed from the NOAA and GISTEMP dataset, using the shorter 3-winter reference period, shown in Fig. 3b and c, respectively. In those datasets too, the anomalies fall between one and two σ of the PDF. So, whereas the months (and years) following the 1883 Krakatau eruption may have been memorable in many respects, that adjective does not apply to the Eurasian wintertime anomalies. We emphasize that this result does ~~not rely on using~~ not rely on the use of any climate models: it is a ~~purely observational~~ *purely observational* result, and is robust across all three temperature reconstructions analyzed here.

4 Reanalyses

Next we turn to examining the 20CR reanalysis, for which a 56-member ensemble is available, starting from the year 1850. The top row of Fig. 4 shows the *ensemble mean* post-Krakatau Eurasian anomalies in 20CR, for NDJ, DJF and JFM, and can be directly compared with the top row in Fig. 1. Although only sea level pressure measurements are assimilated into 20CR, its temperature anomalies are in good agreement with the observations. Note, in particular, how warming in NDJ and DJF gives way to a cooling in JFM over a large portion of Eurasia. This good agreement is perhaps not surprising, since an accurate reanalysis is expected to capture the observations.

Surprising, perhaps, is what can be seen by examining individual members of the ensemble. The anomalies for member #42 are shown in the middle row of Fig. 3: this is the member with the minimum temperature anomalies in DJF across the ensemble. Note the strong cooling in NDJ and DJF over most of Eurasia, which is diametrically opposite to the ensemble mean. It is important to stress that member #42 is driven by the *same* natural and anthropogenic forcings (including volcanic aerosols and CO_2), and constrained by the *same* sea level pressure measurements, as all the other members of the ensemble. Its Eurasian cooling anomalies, therefore, are entirely consistent with those forcings and observations.

For completeness, in the bottom row of Fig. 3 we show the member with the maximum Eurasian temperature anomalies in DJF (member #7). The contrast with member #42 is remarkable. The key point here is that, driven with identical forcings and observations, these two members give diametrically opposite results. The conclusion, therefore, is that the observational uncertainties are large and that, while ~~unlikely (~~its occurrence is unlikely (only 3 of 56 members), a post-Krakatau cooling over Eurasian is not incompatible with ~~observations~~the 20CR reanalysis.

5 Models

Finally, to quantify the *volcanically forced* post-Krakatau Eurasian wintertime surface temperature anomaly (if any), i.e. to separate it from the large internal variability, and to establish whether the stratospheric pathway mechanism may have been operative in the first winter following that eruption, we turn to climate models. We start by examining the 100-member ensemble of historical simulations made available by the Max Plank Institute for Meteorology (hereafter MPI, Maher et al., 2019). We are aware that ~~a few, large, initial condition~~ several large, initial-condition ensembles of historical simulations are now available (Deser et al., 2020)~~: however, most of them do not cover the 1883 Krakatau eruption. Of the few that do, we have decided here~~. We have hear chosen to focus on the MPI ensemble ~~because the underlying model~~ for two reasons. First, with 100 members, it is the largest ensembles currently available. Second, and more importantly: the atmospheric component is a so-called "high-top" model, with a good representation of the stratospheric circulation and, more importantly, of its variability. This is important if ~~we one~~ ne is trying to evaluate whether the stratospheric pathway mechanism is indeed affecting the post-eruption Eurasian winter surface temperatures.

We start by examining the tropical, lower-stratospheric, post-eruption, temperature anomalies in the historical MPI simulations, to ensure that the volcanic aerosols in that model do indeed produce a noticeable warming in that region after each large, low-latitude eruption. The time series of tropical 50 hPa temperature anomalies, from 1850 to 2015, are shown in Fig. 5. Comparison with Fig. 3 of Driscoll et al. (2012) should reassure the reader that these simulations are comparable[2] with those of the CMIP5 models. Note that the 1883/84 ensemble mean MPI anomaly in the tropical lower stratosphere is close to 8°C, with some members showing an anomaly as large[3] as 10°C. Note, in addition, that the post-Krakatau tropical warming is in this model is similar the one for the 1991 Pinatubo eruption.

With this in mind, we now examine the *forced* wintertime Eurasian temperature response to the 1883 Krakatau eruption, as simulated by the MPI ensemble, obtained by averaging all 100 members. It is shown in the top row of Fig. 6 and, as expected, it is basically zero. We emphasize that this result is consistent with the findings of PBS19 for the 1991 Pinatubo eruption, and with the above cited papers which have shown that there is *no statistically significant volcanically forced* surface winter warming in the CMIP3 and CMIP5 models. In the spirit of PBS19, and unlike most previous studies on this subject, we here also examine individual ensemble members. In the middle and bottom rows of Fig. 6, we show the wintertime anomalies for the coldest and warmest member, respectively, of the MPI ensemble. We emphasize that these two members are subjected to an identical volcanic aerosol forcing, which causes a very large warming in the tropical lower stratosphere in both cases (see Fig. 5). And yet, member #37 simulates a large winter cooling while member #38 simulates a large winter warming over Eurasia. Confirming the results of PBS19, this clearly indicates that the large internal variability of the midlatitude circulation completely overwhelms any potential impact from volcanic aerosols in the stratosphere for eruptions of this magnitude.

The inability of the proposed stratospheric pathway to affect wintertime Eurasian surface temperatures in a significant way, even after a very major event such as the 1883 Krakatau eruption, is further illustrated in Fig. 7, where the zonal mean zonal wind anomalies at 60N and 10 hPa (a standard measure of the strength of the stratospheric polar vortex; see e.g. Charlton and Polvani, 2007) are plotted against the surface temperature anomalies over the Eurasian box in the DJF months following the 1883 Krakatau eruption, one dot for each of the 100 members of the ensemble. ~~Notice first~~ Let us start by noting that, in the ensemble mean, the model simulates a stratospheric polar vortex ~~deceleration~~ acceleration of nearly 5 m/s. This independently

[2] Unlike Driscoll et al. (2012), we do not detrend the time series in Fig. 5, as the stratospheric cooling is *not linear* over the long 1850-2005 period. Note how the cooling rate increases substantially in the second half of the 20th century, as a consequence of ozone depletion.

[3] We are well aware that most current-generation models models are believed to overestimate the amplitude of volcanic forcing of the climate system, notably in the winter seasons. For ~~an~~ a recent appraisal of this issue, the reader may consult Chylek et al. (2020), and references therein. It should, however, be clear to the reader that this bias – if it exists – does not invalidate the main conclusion of our study: in fact, it makes it considerably *stronger*.

confirms the finding of Bittner et al. (2016a): analyzing the same ensemble using a slightly different metric, they reported that "12 ensemble members are necessary to identify the mean polar vortex anomaly of approximately 4.5 m/s", when considering the Krakatau eruption alone. In other words, given a sufficiently large ensemble, a small but statistically significant acceleration of the polar vortex *forced* by the Krakatau eruption can be established.

However, and this is the key point of our paper: a polar vortex acceleration of a few m/s does *not* translate into a statistically significant Eurasian surface temperature anomaly. Bittner et al. (2016a) did not address that important point, which we here make explicit. As one can see in Fig. 7, the ensemble mean (i.e. the volcanically forced) surface temperature anomaly is tiny and actually negative but, most importantly, it is *not* statistically significant in this ensemble (cf. the top row of Fig. 6) and it is uncorrelated ~~correlated~~ with the strength of the stratospheric polar vortex ($r \sim 0.3$, implying that 90% ot the surface temperature variance is not explained by the polar vortex strength) . This fact corroborates the finding of PBS19 for the 1991 Pinatubo eruption: for that event too models simulate a small polar vortex acceleration in the first post-eruption winter but, again, that does not translate into a significant surface warming.

Now, one might try argue that MPI model analyzed is flawed in some way, rendering it incapable of capturing the surface response to a small polar vortex acceleration. Our reply to that objection is threefold. First, the MPI model has been used extensively, for many years, to study stratosphere-troposphere dynamical coupling and the effect of volcanic eruptions on climate (e.g., Zanchettin et al., 2019; Illing et al., 2018; Timmreck et al., 2016; Bittner et al., 2016a, b, just to cite the most recent papers): to our knowledge, nobody has suggested a specific and demonstrable flaw that would render it inappropriate to simulate the 1883 Krakatau eruption. Second, we have separately analyzed the 1991 Pinatubo eruption in the MPI 100-member ensemble (not shown), and found its results nearly identical to those reported in PBS19 using different models: this suggests that there is every reason to believe that the MPI results are robust. Third: the lack of a post-Krakatau surface warming response over Eurasia, is completely consistent with the many recent studies cited above showing a lack of forced Eurasia winter warming after large volcanic eruptions. Can all these models be so uniformly wrong?

Finally, to complete the picture, we analyze 140 CMIP5 historical simulations (from 40 different models), in an attempt to determine the role (if any) of the stratospheric pathway mechanism. For purposes of understanding Eurasian wintertime post-volcanic warming, analysis of the CMIP5 models ~~for~~ has already been performed by several studies (e.g., Driscoll et al., 2012; Wunderlich and Mitchell, 2017) which, nearly[4] unanimously, have reported the absence of a volcanically forced winter Eurasian warming, in agreement with results from the CMIP3 (Stenchikov et al., 2006) and many other single-model studies.

Here, we take a different approach to examine the CMIP5 simulations: focusing exclusively on the 1883 Krakatau eruption, we separately analyze the so-called "high-top" models, which have a much better representation of the stratosphere and of its variability, and the remaining "low-top" models (see Methods). One might naively expect that if the stratospheric pathway matters at all, some difference might emerge between these two group of models. As it happens, no such difference exists.

[4] A single dissenting voice, Zambri and Robock (2016), concluded that the CMIP5 models show evidence for a Eurasian wintertime post-volcanic warming. However, that conclusion was reached by lowering the confidence level to 90%, and the areas of significance were found to be small and did not include central Europe. Also, that result was obtained by averaging together the Pinatubo and Krakatau eruptions, so it is not directly comparable with the present study. Finally, we simply note that the findings of that study have not, to date, been independently reproduced.

This is illustrated in Fig. 8, which compactly summarizes the findings of our study including, (1) the good agreement between observations and the 20CR reanalysis, (2) the large uncertainties in 20CR and (3) the non-existent forced response in the MPI ensemble. The CMIP5 models are shown in the two left-most box-and-whisker plots, where the anomalies for the high-top and low-top models have been separately averaged: note that for both types, the multi-model mean post-Krakatau anomaly is *statistically insignificant*, and no obvious difference can be seen between the two types. Thus, the CMIP5 models give no[5] evidence of a stratospheric pathway playing a role in affecting Eurasian surface temperatures in the first post-Krakatau winter.

6 Summary and Discussion

Building on a recent study of Eurasian winter temperature anomalies following the 1991 Pinatubo eruption (PBS19), we have here investigated those anomalies following the 1883 Krakatau eruption, examining observational reconstructions, reanalyses and model output (including one large ensemble and hundreds of CMIP5 simulations). Several key points have emerged from our analysis (see Fig. 8):

1. Observations indicate that from November 1883 to January 1884, Eurasian surface temperatures were warmer than in the preceding winters, with a DJF mean anomaly between 1.5 and 2°C when averaged over the Eurasian continent. Such seasonal anomalies, however, fall in the 1-σ to 2-σ range (of the 1850-present PDF), and are therefore unexceptional.

2. A 56-member ensemble of reanalyses (20CR) reveals a very large spread in post-Krakatau Eurasian wintertime surface temperatures anomalies, with a few members showing cooling. Given such large uncertainties, therefore, we conclude that colder than normal post-Krakatau winter conditions are not incompatible with observations.

3. Analysis of hundreds of state-of-the-art model simulations reveals the *complete absence of a volcanically forced response* over Eurasia in the first post-Krakatau winter. Furthermore, we find no evidence for a stratospheric pathway, which could (in theory) link tropical lower-stratosphere anomalies from volcanic aerosols to Eurasian surface temperature anomalies.

These new findings regarding the 1883 Krakatau eruption strongly corroborate the findings regarding the 1991 Pinatubo eruption, reported in PBS19. This is perhaps not surprising, since these two eruptions are of comparable magnitude. Nonetheless, we submit that it is important to document each eruption individually, as each one offers an independent observational data point. Each eruption is unique, in some way: the state of El Niño-Southern Oscillation (ENSO) and the quasi-biennal oscillation (QBO) indices, the specific location and month of the eruption, the vertical penetration of the volcanic gas and dust into the stratosphere, the rate and extent of latitudinal spreading of the aerosol cloud are specific to each event, and could

[5]Charlton-Perez et al. (2013) also explored the possible existence of post-volcanic difference between the high-top and low-top models, although they examined a small subset of the CMIP5 models analyzed here (about half). Averaging together the first two winters following the 1991 Pinatubo and the 1982 El Chichón eruptions, they also found no difference in winter polar vortex anomalies between the mean of the high-top and of the low-top models.

affect how the eruption impacts surface temperatures[6] at higher latitudes. ~~It is~~We believe, therefore, ~~important to have~~ that it is important to examine each eruption individually. ~~For instance, the 1902 eruption of Santa Maria was also a powerful (VEI=6) low-latutide event, but the volcanic stratospheric sulfur injection (VSSI) from that eruption was only a fraction of the Pinatubo and Krakatau values (Toohey and Sigl, 2017), with the expectation of a comparably smaller impact at high latitudes. In contrast, the~~

Given these potential influences, one might wonder whether the strong similarity between the results reported here for the 1883 ~~Krakatau eruption stands together with the 1991 Pinatubo eruption as~~ Krakatau eruption and those reported in PBS19 for the 1992 Pinatubo eruption might stem from some environmental conditions common to both eruptions which would, in some sense, make them exceptional and thus non-representative of most large, low-latitude eruptions. In particular, the ~~only other recent, large (VEI=6), low-latitude, eruption with considerable stratospheric sulfur injection (for both, VSSI∼ 9 TgS)~~impacts of the QBO (see, e.g., Rao et al., 2020) and of ENSO (see, e.g., Calvo et al., 2008) on the polar stratosphere are well documented, and have been studied in some detail in the context of the Pinatubo eruption (Thomas et al., 2009b, a). As for the QBO: while it well known that the QBO was in an easterly phase in the first winter following the Pinatubo eruption (see, e.g., Toohey et al., 2014, and the discussion therein) no data is available as to the QBO state in the winter 1883/84. As for ENSO: the first post-Pinatubo winter occurred during an El Niño event, whereas ENSO was in a neutral state in the first post-Krakatau winter. Finally, to the best of our knowledge, no study has advocated that these two eruptions are peculiar in any particular, and should thus be discarded as non-representative.

We now ask, therefore: how do the findings reported above for Krakatau, taken together with the findings reported in PBS19 for Pinatubo, reshape our understanding of whether large, low-latitude eruptions can cause a winter warming over Eurasia? First, it interesting to note that an anomalous warming was indeed observed following both eruptions. However, as we have here shown, those anomalies (∼1-2°C in DJF) are far from exceptional. Second, analysis of a wealth of state-of-the-art model simulations leaves no doubt that for VEI=6 eruptions – such as Krakatau and Pinatubo – there is *no statistically significant forced* Eurasian surface winter warming in climate models. And this despite the fact that most models considerably overestimate[7] the lower stratospheric tropical warming that follows these eruptions. Third, we ~~emphasize~~ stress that the stratospheric pathway fails to produce a significant Eurasian warming not because it is physically implausible, but because the amplitude of the polar vortex acceleration that accompanies the warming of the lower-stratospheric by volcanic aerosols (a few meters per second, for both Pinatubo and Krakatau) is simply insufficient to overcome the very large internal variability of the midlatitude circulation.

We submit that, at this point, the evidence is overwhelming: low-latitude eruptions as large as Pinatubo or Krakatau are unable to cause a forced surface temperature anomaly over Eurasia that can be distinguished from unforced variability. As

[6]For instance, the 1902 eruption of Santa Maria was also a powerful (VEI=6) low-latutide event, but the volcanic stratospheric sulfur injection (VSSI) from that eruption was only a fraction of the Pinatubo and Krakatau values (Toohey and Sigl, 2017), with the expectation of a comparably smaller impact at high latitudes. In contrast, the 1883 Krakatau eruption stands together with the 1991 Pinatubo eruption as the only other recent, large (VEI=6), low-latitude, eruption with considerable stratospheric sulfur injection (for both, VSSI∼ 9 Tg[S]).

[7]In the case of Pinatubo, observations indicate a lower stratospheric warming of the order of 2.5°C (Labitzke and McCormick, 1992; Free and Lanzante, 2009), whereas models often show anomalies of the order of 6 to 8°C, as seen, e.g., in Fig. 5.

discussed at length explained in detail in PBS19 (we refer to reader to Section 5, specifically), the early claims of a causal connection were based on low resolution models with poor stratospheric resolution and little stratospheric variability, and on a few observational studies which often commingled high- and low-latitude eruptions as well as the first and second post-eruption winters (we note that none of these observational studies was independently validated). The situation, therefore, is now reversed: rather than taking for granted that large, low-latitude eruptions cause Eurasian winter warming (and seeking to explain the lack of causal evidence in models as, e.g., in Driscoll et al., 2012), the onus has now shifted to presenting evidence in support of the claim that a causal connection exists at all.

Where might one look for that evidence? If there is any hope left for the stratospheric pathway mechanism to communicate the impact of low latitude eruptions to Eurasia in winter, we need to look at larger eruptions. A few of those are well known, and the next best candidate might be the 1815 Tambora eruption. Note, however, that its VSSI index is only three times larger than Krakatau and Pinatubo (Toohey and Sigl, 2017), so even that event may not be large enough. One might then be tempted to look further back in time, e.g. at the 1257 Samalas eruption. Doing so, however, raises additional questions. Perhaps the most troublesome is the following: dendrochronological records constitute the bulk of proxy observations used to produce temperature reconstructions, and those are mostly found in summer (the growing season). As a consequence, the robustness of winter reconstructions remains unclear (Steiger et al., 2018, for instance, show that the DJF skill scores in their reconstruction are much weaker that for the other seasons). And yet, the stratospheric pathway is fundamentally a winter mechanism, as it requires the presence of the stratospheric polar vortex (which is disappears in the summer months). Hence, annual-mean temperatures, which are typical of many, perhaps most, reconstructions (e.g. the recent Last Millennium Climate Reanalysis Project, Hakim et al., 2016) are fundamentally inadequate for the task.

All things considered, establishing the existence of a volcanically forced winter warming over Eurasia, and of an accompanying stratospheric pathway (if any), appears to be a truly daunting challenge.

Code and data availability. All code and data are can be made available upon reasonable request.

Author contributions. LMP originally conceived of the study, and designed it together with SJC. SJC performed the analysis and produced the figures. LMP wrote the manuscript, in close consultation with SJC. Both authors contributed to the interpretation of the results.

Competing interests. The authors are not aware of competing interests.

Acknowledgements. LMP is grateful to the US National Science Foundation for its continued support (AGS-1914569), and wishes to thank ~~Alan Robock and~~ Alexandro Tejedor-Vargas for ~~many enlightening conversations~~several enlightening conversations, and Alan Robock for many friendly discussions and for unsolicited, yet very helpful, comments on an earlier version of the manuscript.

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Table 1. Acronym, number of simulations, and high-top/low-top classification (based on Rea et al., 2018) for the historical integrations of the CMIP5 models analyzed in this paper. Information on model forcings and experimental design can be found in Taylor et al. (2012).

model name	members	type	model name	members	type
ACCESS1.0	1	LT	GISS-E2-H-CC	1	HT
ACCESS1.3	1	LT	GISS-E2-R	16	HT
bcc-csm1-1	3	LT	GISS-E2-R-CC	1	HT
bcc-csm1-1-m	3	LT	HadCM3	10	LT
BNU-ESM	1	LT	HadGEM2-CC	1	HT
CCSM4	6	LT	HadGEM2-ES	4	LT
CESM1-BGC	1	LT	inmcm4	1	LT
CESM1-CAM5	3	LT	IPSL-CM5A-LR	5	HT
CESM1-FASTCHEM	3	LT	IPSL-CM5A-MR	3	HT
CESM1-WACCM	1	HT	IPSL-CM5B-LR	1	HT
CMCC-CESM	1	HT	MIROC-ESM	3	HT
CMCC-CM	1	LT	MIROC-ESM-CHEM	1	HT
CMCC-CMS	1	HT	MIROC5	5	LT
CNRM-CM5	10	LT	MPI-ESM-LR	3	HT
CSIRO-Mk3-6-0	10	LT	MPI-ESM-MR	3	HT
FGOALS-g2	4	LT	MPI-ESM-P	2	HT
FIO-ESM	3	LT	MRI-CGCM3	5	HT
GFDL-CM3	5	HT	MRI-ESM1	1	HT
GFDL-ESM2G	3	LT	NorESM1-M	3	LT
GISS-E2-H	10	HT	NorESM1-ME	1	LT

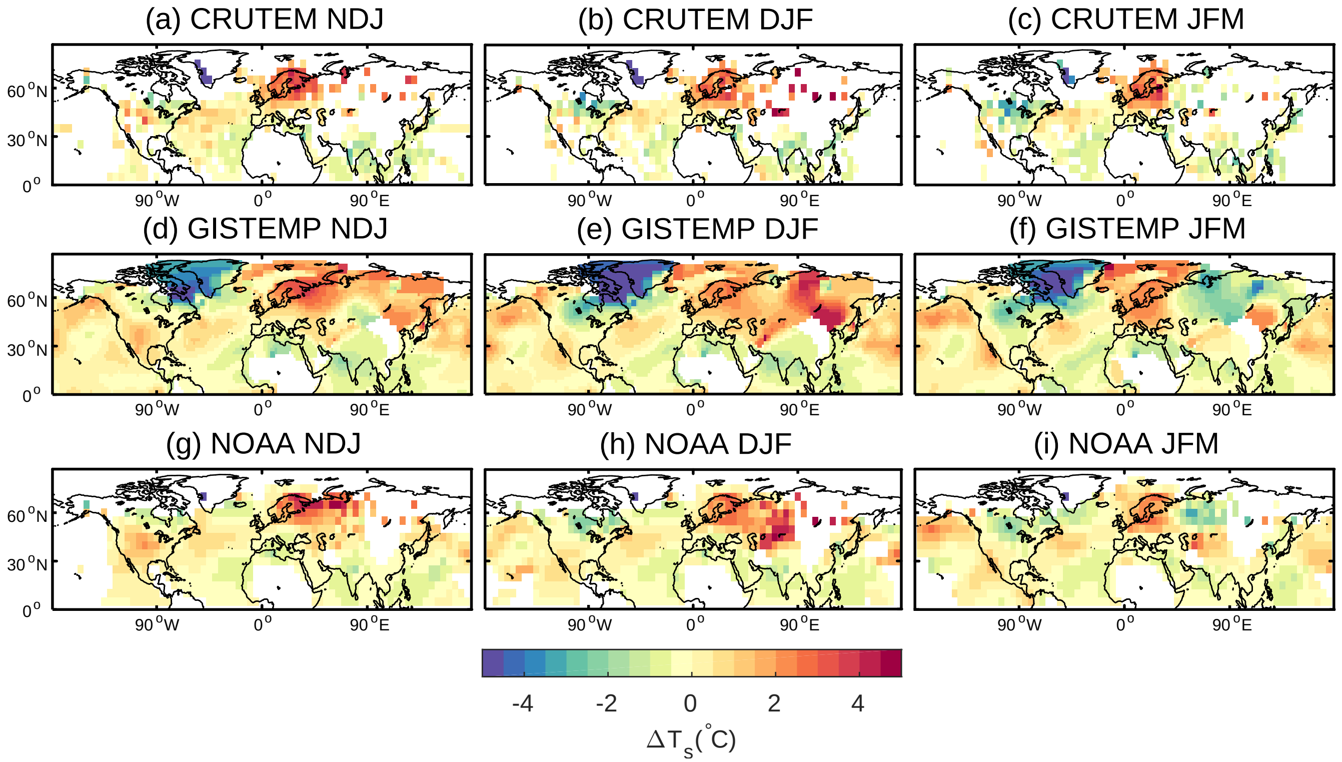

Figure 1. Top row: CRUTEM post-Krakatau surface temperature anomalies (ΔT_s) averaged over (a) November to January (b) December to February and (c) January to March. Middle and bottom row: as top row, but for GISTEMP and NOAA. Due to data availability, the anomalies in the middle and bottom rows are computed using a shorter 3-winter reference period.

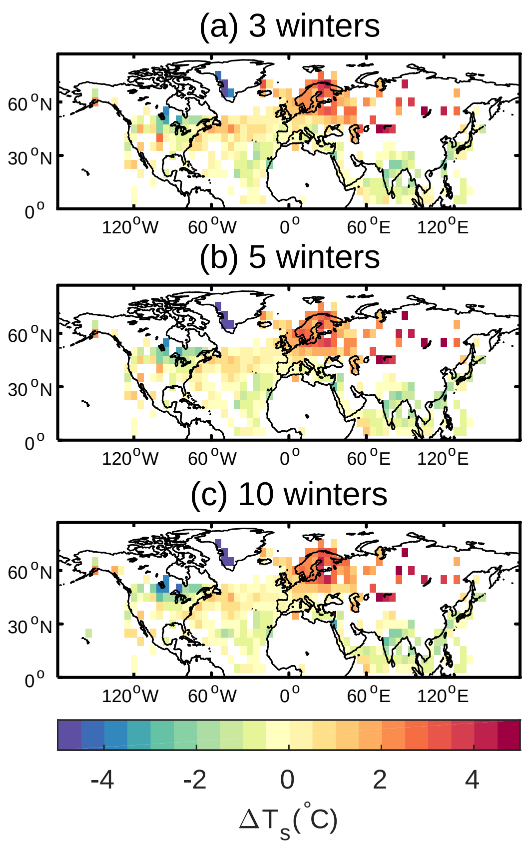

Figure 2. Post-Krakatau CRUTEM surface temperature anomalies (ΔT_s) in DJF, using (a) three (b) five and (c) ten winters for the reference period.

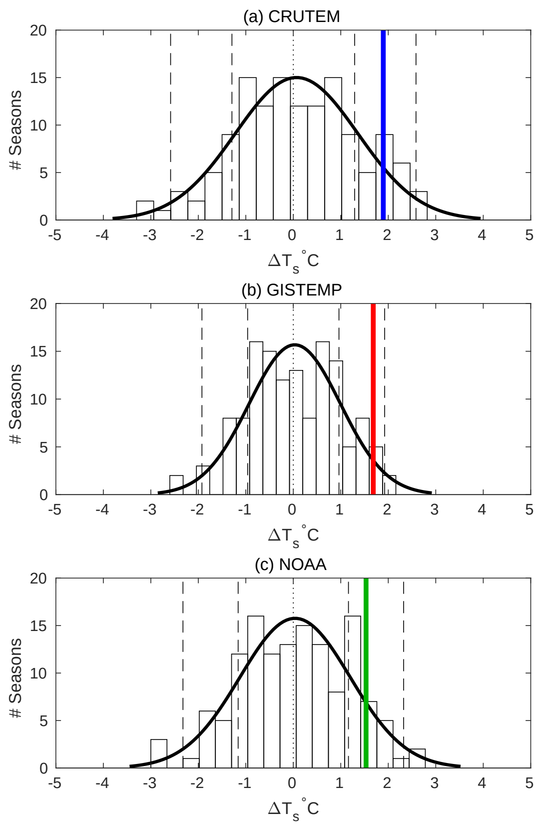

Figure 3. (a) Black boxes: histogram of CRUTEM post-Krakatau Eurasian temperature anomalies (ΔT_s) in DJF, over the period 1850 to present. Colored bar: the 1883/84 post-Krakatau anomalies. Solid black line: Gaussian fit; dashed black lines: the 1-σ and 2-σ intervals. (b) and (c): as in (a) but for GISTEMP and NOAA, respectively, using the shorter 3-winter reference period.

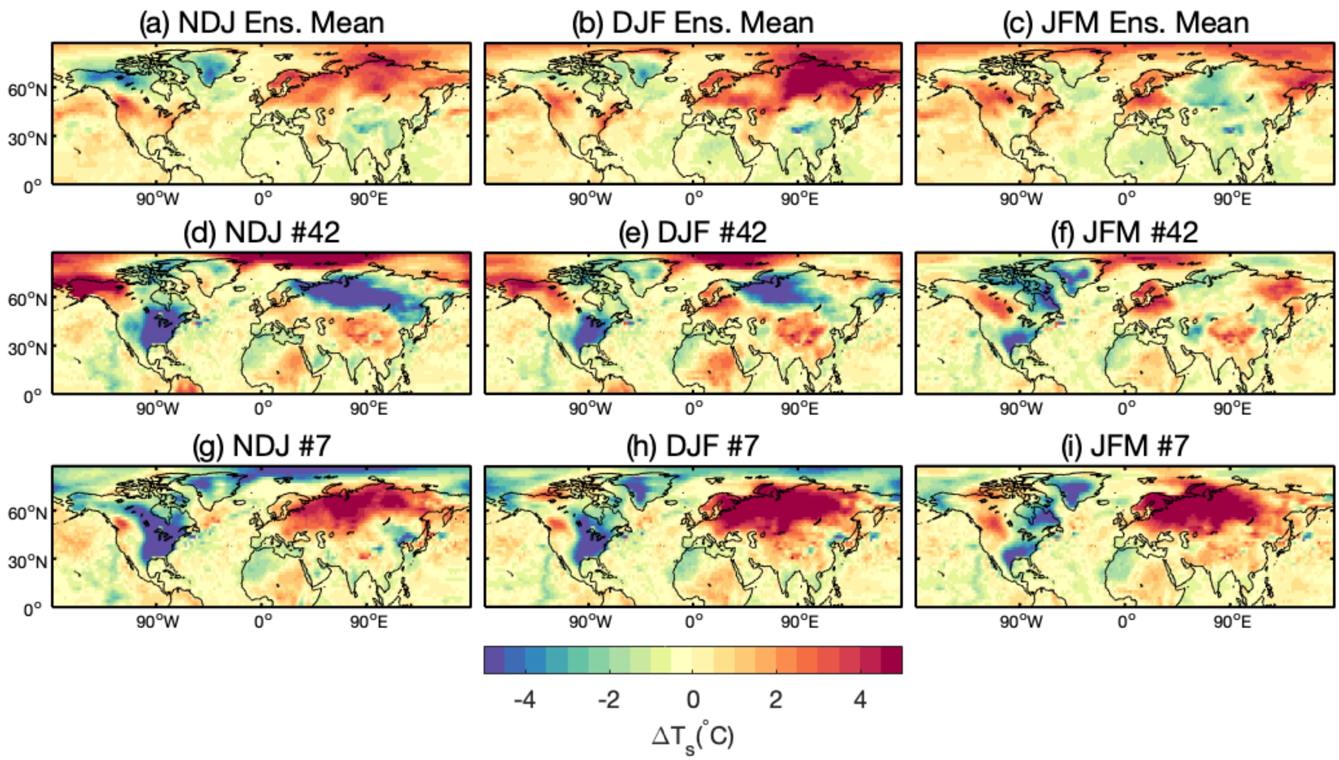

Figure 4. Top row: 20CR ensemble mean, post-Krakatau, surface temperature anomalies (ΔT_s) averaged over (a) November to January (b) December to February and (c) January to March. Middle row: as in the top row, but for ensemble member #42. Bottom row: as in the top row, but for ensemble member #7. Members 42 and 7 have, respectively, the minimum and maximum post-Krakatau Eurasian DJF anomalies across the 56-member 20CR ensemble.

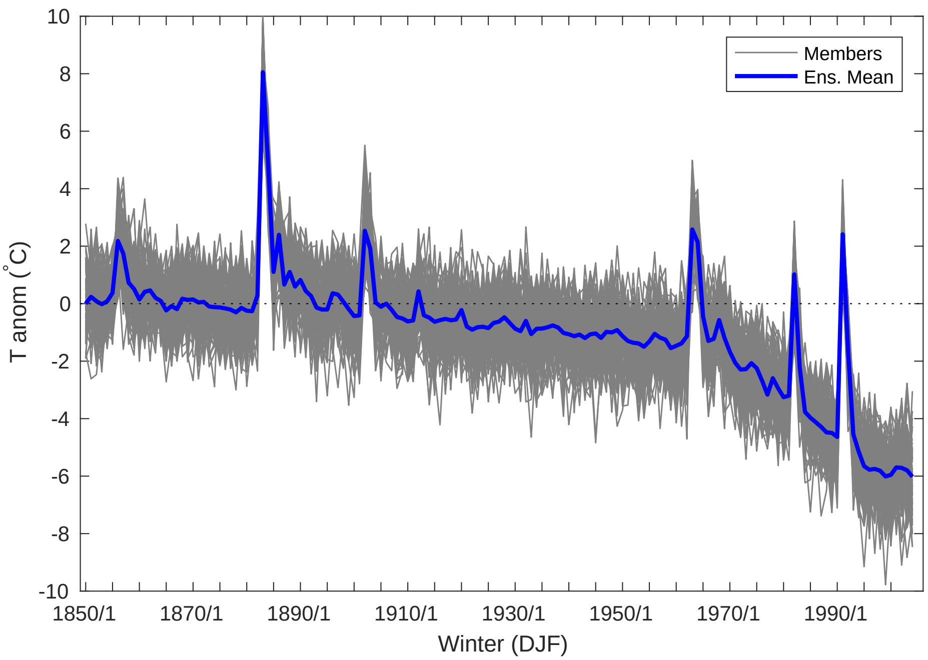

Figure 5. Tropical (30°S-30°N) temperature anomalies in DJF at 50hPa, in the MPI historical runs simulations, from 1850/51 to 2004/5. Grey line: individual members; blue line: ensemble mean. Anomalies are here defined based on the 1861-1880 period.

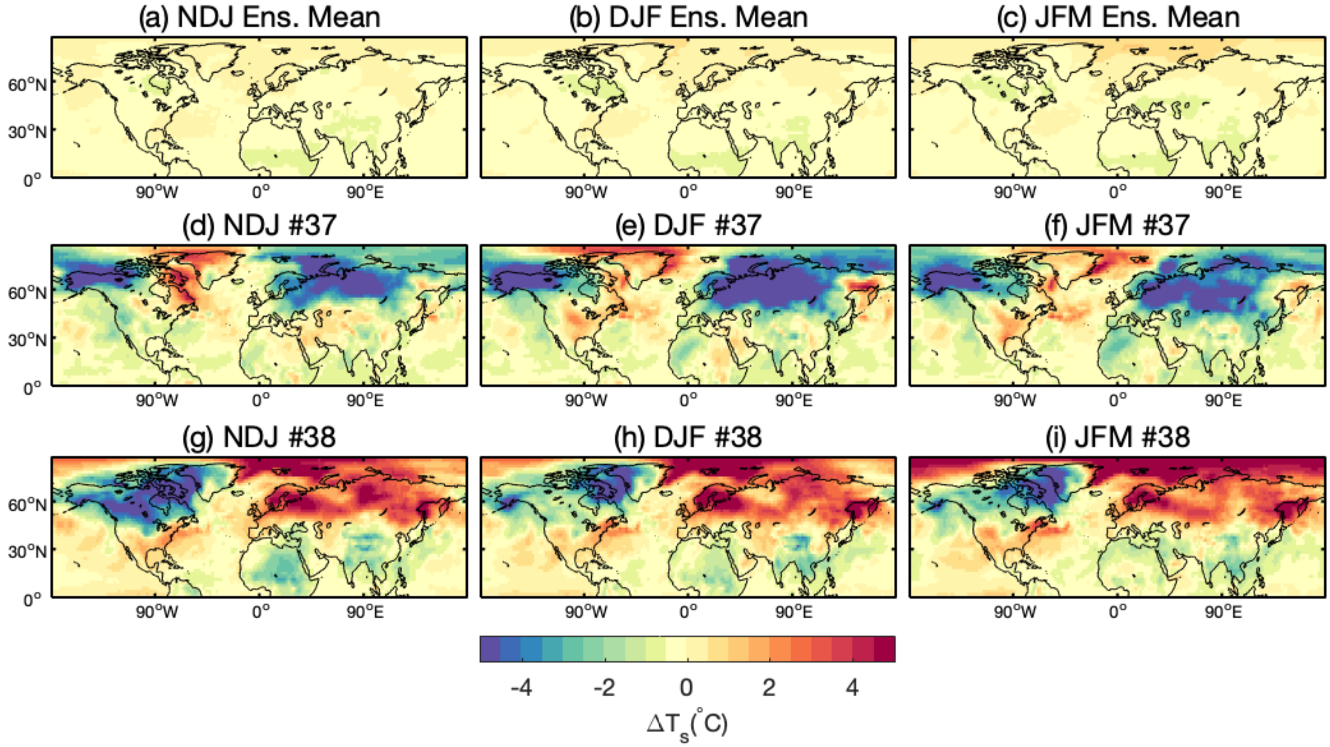

Figure 6. Top row: MPI ensemble mean, post-Krakatau, surface temperature anomalies (ΔT_s) averaged over (a) November to January (b) December to February and (c) January to March. Middle row: as in the top row, but for ensemble member #37. Bottom row: as in the top row, but for ensemble member #38. Members 37 and 28 simulated, respectively, the minimum and maximum post-Krakatau Eurasian DJF anomalies across the 100-member MPI ensemble.

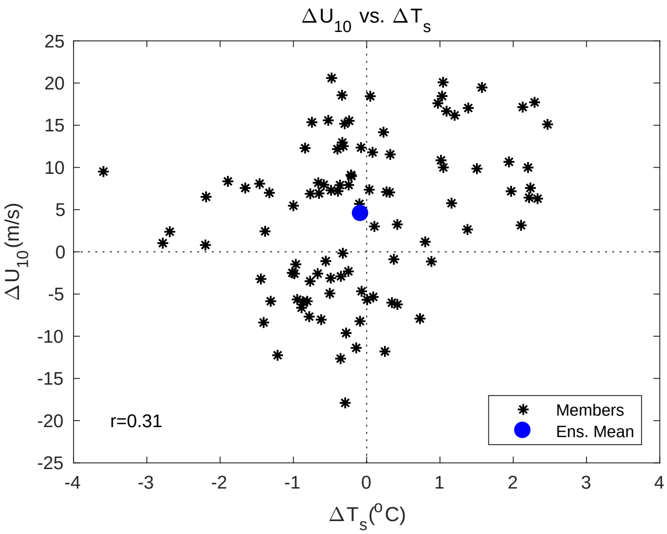

Figure 7. Post-Krakatau, DJF, zonal mean, zonal wind anomalies at 10 hPa and 60°N (ΔU_{10}) in the 100-member MPI ensemble vs the Eurasian surface temperature anomalies (ΔT_s). Blue dot: ensemble mean; black asterisks: individual members.

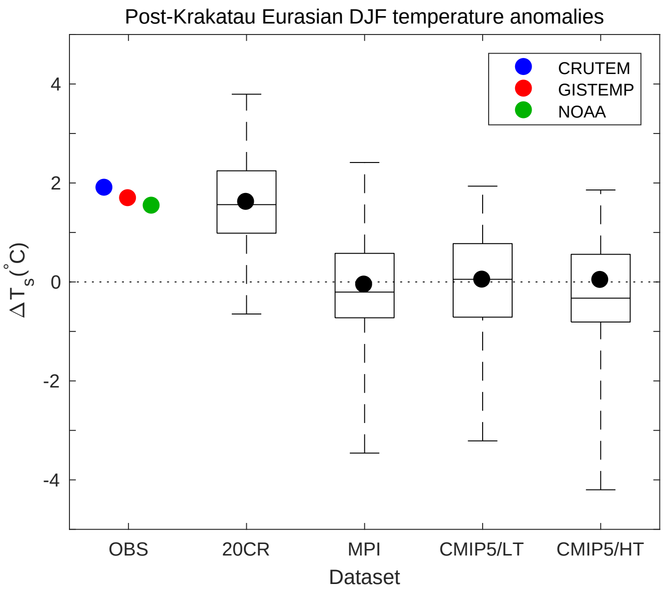

Figure 8. Eurasian surface temperature anomalies in the first post-Krakatau DJF season. Colored dots: observed values from CRUTEM, GISTEMP and NOAA anomalies (for the latter 2, a shorter only 3 reference period was used, owing data availability). The box plots show the median, the 25th and 75th percentiles in each dataset, while the whiskers show the spread of the ensembles for each dataset. For 20CR, a 56-member ensemble was analyzed; for the MPI model, a 100-member ensemble; for the CMIP5 low-top (LT) models 68 simulations from 21 LT models, and for the high-top (HT) models, 62 simulations from 19 HT models. See Table 1 for the list of CMIP5 HT and LT models, and the respective simulations.