Reviewer #2 Comments and Responses:

Comment #1: I am not convinced by this paper, and so recommend rejection or major revisions. Perhaps the timing of the eruption is close to indicators of cooling (not my specialty), but the hand-waving arguments about why this eruption caused cooling and larger ones did not are not convincing.

Response #1: We are sorry to read that Reviewer #2 feels that our arguments are overly speculative. We feel that perhaps the reviewer has missed some key points, which probably reflects that these were poorly communicated on our part. We hope that the revised submission as well as our responses will help better communicate our hypothesis, and we welcome the opportunity to improve the manuscript.

Regarding the last part of Reviewer #2's comment, we are unclear as to which eruptions the reviewer is referring to when Reviewer #2 mentions '…larger ones did not…'? We state:

'…it is in fact the only known sulphur-rich high latitude eruption coinciding with the most sensitive ice volume conditions during the last deglaciation. '

and devote an entire section to other eruptions (Section 3.6), so the other large eruptions that the reviewer is referring to is not clear. We have published previous research indicating that there is a strong statistical significance between large Northern Hemisphere eruptions and long-lasting climate change (Baldini et al., 2015, Scientific Reports) and there is now abundant evidence that Toba also triggered long-lasting climate change (discussed below, in our responses to Comment #7). So there is strong evidence that other larger eruptions did cause cooling, and we argue that the Laacher See eruption occurred during a particularly sensitive climatological transition, so we wonder if the reviewer could clarify which eruptions they are referring to. We now also include a lengthy section discussing volcanic impacts on climate over the last millennium, and an enhanced discussion regarding the amount of sulphur in the eruption, which was substantial. Part of that enhanced discussion is pasted below:

'Both Pinatubo and the LSE were Magnitude 6 (M6) eruptions, where 'magnitude' is a measure of eruption size referring to the amount of material erupted (Deligne et al., 2010 JGR) on a logarithmic scale. However, the cooling effect of a volcanic eruption is controlled by the amount of sulphur released, and not necessarily the eruption size (Rampino and Self, 1982). In general, magnitude and

erupted sulphur amounts are well correlated (Oppenheimer, 2003), and therefore magnitude is often used as a surrogate for sulphur yield. All the available evidence suggests that, if the LSE deviates from this trend, it was anomalously enriched in sulphur relative to its magnitude (Baales et al., 2002; Scaillet et al., 2003), and almost certainly released considerably more SO_2 into the stratosphere than the climatologically significant 1991 AD Pinatubo eruption.'

Comment #2: Indeed more work is required, including climate model simulations that include all the relevant processes.

Response #2: We agree that climate model simulations would benefit the research, but we have decided not to include them for three reasons. i) Neither the meltwater forcing nor the bolide impact hypotheses used climate model simulations to support the initial hypotheses initially, yet both hypotheses led to fruitful discussions and future elaborations, including climate model research. We therefore feel that climate model simulations would make for interesting future work, and indeed we are looking into this ourselves. But we do not feel that they are required for this current submission. ii) There is a good chance that different climate models would return different answers, in which case modelling is better left for future researchers who can devote considerable attention towards developing robust and replicable model outputs. iii) We may not know the details of all the relevant feedbacks, and therefore any model might be incomplete. This issue is actually brought up by the reviewer in their own phraseology: "…climate model simulations that include *all the relevant processes*." The issue is that we may not know all the relevant processes. For example, if we knew that there was in fact a pronounced positive feedback following Northern Hemisphere eruptions during intermediate ice volume conditions, we would know that this feedback was responsible for the Younger Dryas Event as well as other Greenland stadial events. Possibly the reason that we do not know what forced these events is that we do not know about the relevant process, and if that is the case, modelling would not help. This is point is further illustrated by a recent paper by Diallo et al., 2017 GRL, that states *"Thus, climate model simulations need to realistically take into account the effect of volcanic eruptions, including the minor eruptions after 2008, for a reliable reproduction of observed stratospheric circulation changes."* If ambiguous modelling results, and the incorporation of unrealistic eruptions effects into models, are demonstrably an issue in 2008, they will be even more of an issue during the less-well understood YD interval where a key feedback may remain unquantified. For these reasons, we feel strongly that modelling is best left for future work where intercomparison between different models is possible. We hope that this paper, if published, would encourage modellers to consider the mechanisms and feedbacks proposed, and include these in future models.

Comment #3: Even if the timing was close, there is no proof that this was not just a coincidence. The authors claim that the climate system was particularly sensitive to volcanic forcing at the time, but this is just speculation. Where are the model results to show this?

Response #3: Both of the other leading hypotheses for a Younger Dryas trigger also rely on the coincidence of a trigger with the advent of cooling. For example, the meltwater pulse hypothesis relies on the timing of a hypothetical meltwater pulse with the start of the Younger Dryas. However, unlike the Laacher See hypothesis, there is little agreement within the community regarding the source of that meltwater or in fact whether it even occurred simultaneously as the start of GS-1. The bolide impact hypothesis also relies on the coincidence of evidence for a meteor airburst or impact with the Younger Dryas initiation, but whether or not this even occurred is extremely controversial. The Laacher See hypothesis does rely on coincidence, but the event itself is much clearly expressed than the other leading hypotheses. In fact, it is the only hypothesis that features a trigger that is universally accepted as actually having happened. We have now included a lengthy (almost 1,000 words) new section ('3.7 Compatibility with other hypotheses') to better discuss how the Laacher See hypothesis is competitive relative to the other leading proposed triggers.

Please see our response to why we choose not to include models above (Response #2).

Finally, we are not the first to suggest that the climate system is particularly sensitive to volcanic forcing during climatological transitions, and this is not 'speculation'. It is well established that millennial-scale climate change was most sensitive to a forcing during intermediate ice volume conditions, and we simply propose that that forcing was volcanism. Zielinski et al. (1996) noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self 1992 (Nature) stated that 'Volcanic aerosols may also contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval.' Our research confirms and builds on this earlier work, and identifies a volcanic eruption potentially responsible for the YD. We have included the following text:

"This perspective is consistent with previous observations, including those of Zielinski et al. (1996) who noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self (1992) stated 'Volcanic aerosols may also contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval.'. Our results are entirely consistent with this perspective, and here we identify the volcanic eruption responsible for the YD."

Comment #4: In fact, in Fig. 4 there are two larger eruptions during the same period. Why did only Laacher See produce cooling? They claim in the Fig. 4 caption that the Hekla eruption was more proximal, and therefore should be discounted, but the way it works is that Icelandic eruptions into the westerlies have to go around the world before the acid snow deposits on Greenland, and so there is no reason to think that it would have a smaller climate impact than Laacher See.

Response #4: The volcanological information regarding the size of the Hekla eruption is from published sources (Muschiatello et al., 2017, Nature Communications; Mortensen et al 2005, Journal of Quaternary Science), so we are simply referring to previously published research when we refer to the fact that Hekla was substantially smaller than the Laacher See eruption, and that it appears in the GISP2 ice core. Additionally, the reviewer is incorrect about Icelandic eruptions, and it is well-established that sulphate from even small Icelandic eruptions appears in Greenland ice cores (Muschiatello et al., 2017, Nature Communications; Abbott and Davies, 2012, Earth Science Reviews; Abbott et al., 2012, QSR). Muschiatello et al. (2017) state: "Icelandic volcanoes remain the dominant source of volcanogenic aerosols in Greenland ice cores due to their relative proximity and high eruptive frequency". The other eruption we mention in the Figure 4 caption is the Nevado de Toluca eruption; we refer the reviewer to the text already in our manuscript that explains why its climate expression may not be as clear:

'The eruption was approximately the same size as the LSE, so the lack of climate cooling may reflect a different climate response due to the eruption's latitude, which caused a more even distribution of aerosols across both hemispheres, or a lower sulphur load. The 12.6 ka BP sulphate spike is associated with a short but dramatic cooling; therefore the lack of long-term cooling may simply

reflect the fact that temperatures had already reached the lowest values possible under the insolation and carbon dioxide baseline conditions characteristic of that time.'

Comment #5: Even the size of the eruption is speculation, and the authors mix mass of SO2 with that of elemental Sulphur with that of stratospheric aerosol. What do they claim actually was the stratospheric loading for this eruption? And each time you talk about mass, please convert it to the same chemical so it can be compared.

Response #5: We thank the reviewer for picking up on this. This partially stems from an ambiguity in a previously published paper that we used as a reference. We have now converted everything to the same chemical, except where the context requires us to discuss a different one.

Comment #6: The title is confusing. Why is it "re-evaluating?" There is no initial evaluation that is addressed in the abstract or in the paper.

Response #6: This issue was also brought up by another comment by another reader of the manuscript. The Laacher See eruption was proposed very briefly in the late 1980s as a trigger for the YDE, before it was discarded due to evidence (incorrectly) showing that it occurred too early. However, the most recent lake core and ice core data indicate that the beginning of YD cooling (the start of GS-1) occurred synchronously with the LSE, so we feel that 'Re-evaluating' is the correct word to use here. We stated in the introduction of our originally submitted manuscript:

"Instead, we re-introduce and provide new support for the hypothesis that the YD was triggered by the ~12.9 ka BP eruption of the Laacher See volcano, located in the East Eifel Volcanic Field (Germany). Early research considered the eruption as a possible causative mechanism for the YD (Berger, 1990). However, the concept was dismissed because lacustrine evidence across central Europe appeared to indicate that the YD's clearest expression appeared ~200 years after the Laacher See Tephra within the same sediments (e.g., Brauer et al., 2008; Brauer et al., 1999; Hajdas et al., 1995)."

We were not the first to suggest the eruption as a trigger, although we are 're-evaluating' the eruption's climatological consequences in a modern context. Still, because two reviewers raise this same issue, we have now included the following text to better explain why we chose to use this term in the title:

"Therefore, we are 're-evaluating' the Laacher See eruption's role in triggering the YD, building on early research that first suggested the eruption as a causative mechanism for the YD, and later research that dismissed the original version of the Laacher See hypothesis"

Comment #7: Since the Laacher See eruption was high latitude, we would expect that for the same stratospheric loading, it would have much less of a climate impact than an equivalent tropical eruption, since the atmospheric lifetime would be much shorter, and there is less insolation at high latitudes. When you compare to Toba, this must be addressed.

Response #7: We have added the following text to discuss this:

'The residence time of aerosols within the atmosphere is not critical within the context of this model provided the positive feedback is activated. A sufficiently high aerosol-related cooling even over only one summer and one hemisphere could suffice. The long-term climate response will depend on the climate background conditions; if a NH eruption occurs during an orbitally-induced cooling trend (as was apparently the case for Toba), the eruption will catalyse cooling towards the insolation-mediated baseline. If the eruption occurs during rising insolation and intermediate ice volume (e.g., Laacher See), the eruption will trigger the feedback which will continue until it is overcome by a sufficiently high positive insolation forcing. A useful way of visualising this is to consider two extreme scenarios: i) very high CO_2 concentrations and no ice (e.g., the Cretaceous) and ii) very low CO_2, low insolation, and very high ice volume (e.g., the Last Glacial Maximum). An eruption the size of the LSE would probably not significantly affect climate beyond the atmospheric residence time of the sulphate aerosols during either scenario. Under the background conditions characteristic of the first scenario, insufficient aerosol forcing would occur to trigger ice growth, and consequently no positive feedback would result. In the case of the second scenario, the eruption would cause ice growth and cooling for the lifetime of aerosols in atmosphere before conditions returned to the insolation-mediated baseline. In contrast to these extreme scenarios, an injection of volcanogenic sulphate aerosols into the NH atmosphere would most effectively trigger a feedback during intermediate CO_2

(and consequently ice volume) conditions; in other words, during a transition from one ice volume state to another. Under these conditions, we suggest that activation of the feedback would occur even if the sulphate aerosols settled out of the atmosphere after just one year. Therefore, although the Toba and Laacher See eruptions were of very different magnitudes, the nature of the positive feedback would depend largely on the background conditions present during the individual eruptions. We argue that both eruptions were large enough, and contained enough sulphur, to activate the positive feedback under their respective background conditions. The strength of the positive feedback was then controlled by background conditions rather than the size of the eruptions. Furthermore, we predict that the more asymmetric the hemispheric distribution of the aerosols, the stronger the feedback. For these reasons, the long-term climate response following the extremely large but low latitude Toba eruption would approximate those of the far smaller but high latitude Laacher See eruption. It is also worth noting that the long-term climate repercussions to a very large volcanic eruption would probably not consist of long term radiative cooling (i.e., 'volcanic winter') but rather of geographically disparate dynamical shifts. '

Comment #8: And if the eruption was in the fall or winter, most of the aerosol would have fallen out of the stratosphere before the Sun comes up the next summer and there would be minimal impact on climate.

Response #8: For historical eruptions similar in size to the Laacher See eruption, aerosols have remained in the atmosphere for much longer than one year. For example, aerosols remained in the atmosphere for ~3 years after the Pinatubo eruption (15°N, 120°E) (Diallo et al., 2017), which is estimated to have released approximately eight times less sulphur than the LSE (20 Mt versus 150 Mt) (Sheng et al., 2015; Baales et al., 2002). Aerosols remained in the atmosphere for more than one year even after the 1980 Mount St. Helen's eruption (46°N, 122°W) (Pitari et al., 2016), which injected up to almost 100x less sulphur into the atmosphere than the LSE (2.1 Mt versus 150 Mt) (Pitari et al., 2016; Baales et al., 2002) and erupted laterally (Eychenne et al., 2015, JGR-Solid Earth). Furthermore the eruption almost certainly occurred in the late spring. We have added the following text to discuss this in the manuscript:

'Initiation of the positive feedback requires volcanic aerosols to remain in the atmosphere for at least one summer season. Evidence based on the seasonal development of vegetation covered by the LST suggests that the LSE occurred during late spring or early summer (Schmincke et al., 1999), and varve

studies similarly suggest a late spring/early summer eruption (Merkt and Muller, 1999). Available evidence therefore suggests that the eruption occurred just prior to maximum summer insolation values, maximising the potential scattering effects of the volcanogenic sulphate aerosols. Even if it were a winter eruption, for historical eruptions similar in size to the Laacher See eruption, aerosols have remained in the atmosphere far longer than one year, regardless of the eruption's latitude. For example, aerosols remained in the atmosphere for ~three years after the Pinatubo eruption (15°N, 120°E) (Diallo et al., 2017), which is estimated to have released approximately nine times less SO2 than the LSE (~15-20 Mt versus 150 Mt) (Sheng et al., 2015; Baales et al., 2002). Aerosols remained in the atmosphere for approximately three years even after the 1980 Mount St. Helen's eruption (46°N, 122°W) (Pitari et al., 2016), which injected almost 100x less SO2 into the atmosphere than the LSE (2.1 Mt versus 150 Mt) (Baales et al., 2002; Pitari et al., 2016) and erupted laterally (Eychenne et al., 2015). In short, the LSE eruption probably occurred during the late spring or early summer, but even if the eruption were a winter eruption, the LSE's aerosols would have certainly persisted over at least the following summer, with the potential to catalyse the positive feedback we invoke.'

Comment #9: The paper is replete with undefined acronyms, making it very confusing. All acronyms have to be defined the first time they are used. For example, what is LST? It is never defined. Is it LSE and a typo? What are TOMS, NGRIP, GISP2, GICC05modelext, ITCZ, GS-20, GI-19 (in Fig. 6), ...? Please keep in mind that there will be readers not from your specific discipline, and so jargon needs to be defined. GS-1 is finally defined long after it is used, but the authors still never say what Greenland Stadial 1 is. What is a stadial? Why does Greenland have one? How many does it have?

Response #9: The reviewer is quite correct here, and we apologise for not defining these terms. We have gone through the manuscript and defined all acronyms and terms which might be confusing for a non-specialist. We have not mentioned the total number of stadials because it is not relevant to the manuscript and would add confusion. The cause of other stadials is not known for sure, though we do highlight that our previous research implicates volcanism.

Comment #10: The paper talks about magnitudes for volcanic eruptions, but never says what the scale is. Magnitude of what? If not of sulphur injection, then what is the point? And where do the data come from? There are no references to that.

Response #10: 'Magnitude' is a common term in volcanology that refers to the amount of tephra and lava erupted. It is very difficult to know for sure how much sulphur was in eruptions recorded in the geological past due to the fact that much of the sulphur existed in a volatile phase and is not preserved in the rock record. In general magnitude and sulphur concentrations are well correlated (Oppenheimer et al., 2003), and therefore magnitude is therefore often used as a surrogate for sulphur concentrations. All the available evidence suggests that if the LSE deviates from this trend, it is anomalously enriched in sulphur than expected. We have added the following text to the manuscript:

'Both Pinatubo and the LSE were Magnitude 6 (M6) eruptions, where 'magnitude' is a measure of eruption size referring to the amount of material erupted (Deligne et al., 2010 JGR) on a logarithmic scale. However, the cooling effect of a volcanic eruption is controlled by the amount of sulphur released, and not necessarily the eruption size (Rampino and Self, 1982). In general, magnitude and erupted sulphur amounts are well correlated (Oppenheimer, 2003), and therefore magnitude is often used as a surrogate for sulphur yield. All the available evidence suggests that, if the LSE deviates from this trend, it was anomalously enriched in sulphur relative to its magnitude (Baales et al., 2002; Scaillet et al., 2003), and almost certainly released considerably more SO2 into the stratosphere than the climatologically significant 1991 AD Pinatubo eruption.'

Comment #11: As for the Toba eruption, the paper is missing key references on the climate impact. Robock et al. (2009) found a larger short-term impact, but no long-term effect. Timmreck et al. (2010) claim that it would have had a small impact, as the particles would have grown and had a smaller impact per unit mass.

Robock, A., C. M. Ammann, L. Oman, D. Shindell, S. Levis, and G. Stenchikov, 2009: Did the Toba volcanic eruption of _74 ka B.P. produce widespread glaciation? J. Geophys. Res., 114, D10107, doi:10.1029/2008JD011652. Timmreck, C., et al., 2010: Aerosol size confines climate response to volcanic supereruptions. Geophys. Res. Lett., 37, L24705, doi:10.1029/2010GL045464.

Response #11: We thank the reviewer for these suggestions, and we have now included these references as well as an enhanced discussion.

Comment #12: In any case, I find the Haslam and Petraglia (2010) Figure 1 very convincing that it got cold before the eruption. By the way, that reference is missing from the reference list. Why does the timing of the Toba eruption in Fig. 6 here differ from that in Fig. 1 of Haslam and Petraglia (2010)? Which is correct, and why?

Response #12: As we note in our previous submission's text, the timing of the Toba eruption S spike within the ice cores is based on Svensson et al., 2013, Climate of the Past, which uses a very thorough analysis of both Arctic and Antarctic ice core records. This represents the most up-to-date assessment of the timing of the Toba eruption relative to Greenland climate change, so we use this, in agreement with other recent publications (e.g., Polyak et al, 2017, Geology). The Haslam and Petraglia 2010 paper precedes the Svensson et al. 2013 analysis, and uses an older chronology. So our assessment is correct, and Haslam and Petraglia 2010 is incorrect, at least in terms of reflecting the most recently accepted chronology. Because the Haslam and Petraglia 2010 paper is demonstrably out-of-date (through no fault of their own – the paper preceded the chronological revisions we used), it is not worth discussing at length in this manuscript.

Comment #13: The paper ignores all the work that has shown that the 1257 Samalas eruption caused the Little Ice Age (Zhong et al., 2011; Miller et al., 2012; Slawinska and Robock, 2017). What does this tell us about the claim that a much smaller eruption of Laacher See caused a much larger climate response?

Miller, G. H., Á. Geirsdóttir, Y. Zhong, D. J. Larsen, B. L. Otto-Bliesner, M. M. Holland, D. A. Bailey, K. A. Refsnider, S. J. Lehman, J. R. Southon, Ch. Anderson, H. Björnsson, and T. Thordarson, 2012: Abrupt onset of the Little Ice Age triggered by volcanism and sustained by sea-ice/ocean feedbacks. Geophys. Res. Lett., 39, L02708, doi:10.1029/2011GL050168. Slawinska, J., and A. Robock, 2017: Impact of volcanic eruptions on decadal to centennial fluctuations of Arctic sea ice extent during the last millennium and on initiation of the Little Ice Age. J. Climate, doi:10.1175/JCLI-D-16-0498. http://journals.ametsoc.org/doi/abs/10.1175/JCLI-D-16-0498.1

Zhong, Y., G. H. Miller, B. L. Otto-Bliesner, M. M. Holland, D. A. Bailey, D. P. Schneider, and A. Geirsdottir, 2011: Centennial-scale climate change from decadally-paced explosive volcanism: a coupled sea ice-ocean mechanism. Clim. Dyn., 37, 2373-2387.

Response #13: We thank the reviewer for flagging this up; we have now included these suggested references, as well as some others as well. As for the second part of the comment, we refer the reviewer to our response to their Comment #7 (i.e., the size of the feedback is dependent on the background conditions, not the size of the eruption, provided the eruption is sufficiently large to trigger ice growth and interrupt ocean circulation). We have included a substantial amount of new text in the revised submission:

'A similar mechanism may have also contributed to Little Ice Age cooling (Miller et al., 2012), with recent research suggesting that a coupled sea ice/AMOC mechanism could extend the cooling effects of volcanic aerosols by over 100 years during the Little Ice Age (Zhong et al., 2011). This perspective is supported by modelling results suggesting that a large volcanic forcing is required to explain Little Ice Age cooling (Slawinska and Robock, 2017). Lehner et al. (2013) identify a sea ice/AMOC/atmospheric feedback that amplified an initial negative radiative forcing to produce the temperature pattern characterising the Little Ice Age. Similarly, large volcanic eruptions in 536, 540, and 547 AD are hypothesised to have triggered a coupled sea ice/AMOC feedback that led to an extended cold period (Buntgen et al., 2016). Recent research also highlights the possibility that volcanism followed by a coupled sea ice/ocean circulation positive feedback triggered hemispheric-wide centennial to millennial-scale variability during the Holocene (Kobashi et al., 2017). If a sea ice/AMOC feedback was active following volcanic eruptions during the 6th Century, the Little Ice Age, and the Holocene, the intermediate ice volume and transitional climate characteristic of the last deglaciation should have amplified their effects. This perspective is consistent with previous observations, including those of Zielinski et al. (1996) who noted that when the climate system is in a state of flux it is more sensitive to external forcing, and that any post-volcanic cooling would be longer lived. Importantly, Rampino and Self (1992) stated "Volcanic aerosols may also contribute a negative feedback during glacial terminations, contributing to brief episodes of cooling and glacial readvance such as the Younger Dryas Interval". Our results are entirely consistent with this perspective, and here we highlight a candidate volcanic eruption whose timing coincided with the onset of YD-related cooling. However, despite increasingly tangible evidence that eruptions can affect AMOC strength and sea ice extent, the exact nature of any positive feedback is still unclear. Future research should prioritize the identification and characterisation of this elusive, but potentially commonplace, feedback that amplifies otherwise subtle NH temperature shifts.'

Comment #14: It would have been nice to have used hanging indents or additional spacing for the reference list to make it easier for the reader to find each paper in the list. In addition, there are another 35 comments in the attached annotated manuscript that need to be addressed.

Response #14: We agree with the reviewer, and have added a space between the references for increased leginility. We have addressed all of the comments contained in the annotated manuscript.