Reviewer #1 Comments and Responses:

Comment #1: Baldini and colleagues present a previously published volcanogenic sulphate record from GISP2 ice cores and compile age estimates for the Laacher See tephra to argue that the LS eruption is recorded in Greenland ice cores as a large sulphate spike positioned approximately at the onset of Greenland Stadial 1 (i.e. Younger Dryas). The authors finally suggest that the LS eruption triggered the YD through a chain of feedbacks resulting from the initial volcanic-induced cooling in the Northern Hemisphere. Even though their hypothesis is tantalizing I find the manuscript excessively speculative and the conclusions very much stretch what can be observed in the reconstructions.

Response #1: We appreciate that this topic is controversial, and the worst-case scenario would be if the manuscript were published without undergoing a rigorous review process. We also thank the reviewer for suggesting that the hypothesis is 'tantalising'; we agree and we hope to convince Reviewer #1 that the conclusions are not overly speculative. We would like to note upfront that we did not 'cherry-pick' the records that were used in the manuscript, rather these were chosen as the records with the most robust chronologies both in absolute terms and with respect to the timing of GS-1 versus the LSE.

Comment #2: I should also point out that the putative link between the LST and a volcanic sulphate spike in GISP2 records was originally suggested by Brauer et al. (1999). In my opinion, the present manuscript doesn't offer anything new but I brief review of the climatic implications of volcanic cooling and far-reaching speculations on the triggers for the YD.

Response #2: Should this manuscript be published, it would open the door to the consideration of a new trigger for the Younger Dryas Event. Of the most recent reviews of the Younger Dryas and its possible causes (e.g., Carlson 2010, *Geology*; Fiedel 2011 *Quaternary International*; Broecker et al., 2010 *Quaternary Science Reviews*), none even mentions volcanic forcing; we therefore disagree wholeheartedly with the reviewer's statement that this '…manuscript doesn't offer anything new…'. This manuscript presents a very novel and provocative hypothesis, and does not represent 'business-as-usual'. Although there are certainly elements of a review, we are the first paper to outline in detail the positive ice-AMOC feedback following the LSE within the context of the YDE. We also identify the sulphate spike associated with the Laacher See eruption, using the most recent chronology for the GISP2 ice core. Reviewer #1 is correct that Brauer et al 1999 did consider this same spike as being potentially linked to the LSE, but ultimately they decided that it occurred too close to the YD boundary and concluded that an earlier spike represented the LSE (this is now discussed); so I think it is correct to say that we are the first to attribute the LSE to this particular spike, though we will now discuss the fact that Brauer et al (1999) considered the spike before us.

This manuscript uses previously published data to reach new conclusions, and we therefore feel that this goes considerably above and beyond a 'brief review'.

Comment #3: Assigning the LS eruption to a nearly synchronous large sulphate peak in GISP2 is a flawed assumption until proven otherwise by tephochronological analyses. The authors should consider alternative hypotheses. For instance, volcanic records from Greenland are particularly sensitive to Icelandic eruptions due to the eruptive frequency and proximity (e.g. Abbott and Davies, 2012). Hence, even small-size Icelandic eruptions characterised by moderate sulphate emissions can result in disproportionally large sulphate anomalies in ice core stratigraphies.

Response #3: We agree that tephochronological analyses would confirm our identification, and we have now included this statement in the text. We note that although this is certainly an excellent idea, the lack of tephochronological data did not prevent other researchers from attributing sulphate spikes to individual eruptions (for example, Brauer et al., 1999, mentioned by the reviewer in Comment #2, Svensson et al., 2013 Climate of the Past, and many others). We have also provided ideas for confirming our hypothesis in general in the conclusion.

Comment #4: 2. I think the paper would greatly benefit from exploring some of the mechanisms the authors discuss (e.g., southward wind shifts, AMOC decline, sea ice expansion, etc.) by looking into climate model output and/or historical reanalyses data sets. The authors present output data from MAECHAM4 simulations but they don't provide any additional analysis of the atmospheric parameters in the model. CMIP historical climate simulations and PMIP last-millennium simulations offer valuable model data to examine the role of volcanic eruptions on the coupled atmosphere-ocean system. Model output based on volcanically-forced transient simulations with earth System Model (MPI-ESM) (Jungclaus et al., 2010) (available online) may also be useful.

Response #4: We now include a revised and extended discussion of relevant, previously published model outputs over the last two millennia. The climate and AMOC response following volcanic eruptions is different in each of the models; this is now discussed.

Comment #5: I think some of the arguments proposed here (as well as the records presented in Figure 3) have conveniently been picked to craft a story where the LS eruption stands out as the INITIAL cooling that triggered the YD. This does not faithfully reflect the state of the knowledge around the dynamics that took place prior to and at the onset of the YD. Several reconstructions show that a gradual but substantial cooling across Northern Europe and in the Nordic Seas preceded the start of GS-1. Pollen records from the British Isles indicate cooling as early as 13,200 years BP (Walker et al., 2012). A drop in air temperatures a few centuries before GS-1 has also been recorded in chironomid-based temperature records from Norway (Bakke et al., 2009; see supplementary information), Sweden (Muschitiello et al., 2015), and the Netherlands (Heiri et al., 2007). Similarly, sea surface temperature records from the Norwegian Sea indicate a rapid cooling approaching YD values as early as 13,500 years BP (Bakke et al., 2009) with temperature values dropping by ca. Well-dated paleoceanographic records from the coast of Norway also show a progressive aging of surface waters starting at ca. 13,200 years BP (Bondevik et al., 2006), which is suggestive of a slowdown of surface-water circulation and reduced advection of warm subtropical waters prior to the start of GS-1. I therefore believe that the authors should do a better job in contextualizing the LS eruption in the regional climate picture since proxy reconstructions from Central Europe (e.g. Grafenstein, 1999; Rach et al., 2014) are evidently not fully representative of large-scale North Atlantic climate. In particular, the records mentioned above clearly suggest that cooling and climate deterioration was long underway before the start of the YD, which implies that the cooling associated with the LS eruption cannot be the trigger for the YD.

Response #5: We appreciate the very thorough suggestions for other records to consider, and here we provide reasons why the records are, or are not, relevant to the Laacher See hypothesis. Again, we assure the reviewer that we have not knowingly excluded records that contradict our hypothesis. Rather, the records chosen are those with the most robust chronologies both in absolute terms and with respect to the timing of GS-1 versus the LSE. All of the records used contain both a high resolution regional temperature proxy record and either the Laacher See Tephra directly, or an excellent layer-counted chronology.

Muschitiello et al., 2015 Nature Communications: We do not doubt that meltwater pulses did occur and undoubtedly affected climate during the last deglaciation. In fact, we suggest that the YD was terminated by a Southern Hemisphere meltwater pulse that triggered long-term warming. Muschitiello et al. argue that meltwater forcing affected climate from 13.1 to 12.880 ka BP. We do not argue against this, but nothing in this paper contradicts our manuscript. In fact we note that, once again, a pronounced climate shift occurs coincident with the Laacher See eruption at 12.880 ka BP. The large inflection point in Figure 4 (Panel d and elsewhere) is indistinguishable from the date of the eruption. It is currently difficult to disentangle whether the forcing was a meltwater pulse from the Fennoscandian Ice Sheet (or elsewhere), a bolide impact, or the Laacher See eruption, and we hope that our research will promote future debate assessing the pros and cons of each hypothesis.

We also note that it is conceivable that the Fennoscandian Ice Sheet was melting from 13.1 to 12.88 ka BP due to rising insolation, releasing meltwater. The end of this meltwater pulse coincides perfectly with the cooling (GS-1) that we argue was triggered by the LSE; it is therefore reasonable that post-eruptive cooling, combined with a positive feedback, could temporarily reverse Fennoscandian Ice Sheet melting. We also note that Muschitiello et al. state that summer temperature dropped by 2 degrees at 12.883±0.035 ka BP, “suggesting substantially drier and colder summer conditions.” It is conceivable that this is reflecting the direct aerosol effects of the LSE.

Heiri et al. 2007: The paper presents an interesting chironomid-based temperature record of the Younger Dryas from the Netherlands. Unfortunately, it is too low resolution to be particularly useful to this manuscript. The dating is also not as high precision as the records that we have chosen, though we note that the decrease in July temperature starts just after the eruption based on their chronology. Still, we choose not to include this record and others like it because of the low resolution and more uncertain chronology.

Walker et al 2012: Unfortunately the low resolution and ambiguous results make this paper a low priority for inclusion. There are hundreds of Younger Dryas climate reconstructions globally, and including all of them is simply not possible. Pollen reconstructions in particular are problematic due to the generally lower resolution of the datasets, the often less well-constrained chronologies, and the local nature of the proxy. We do not see how this paper provides a significant challenge to either a LSE, a bolide impact, or a meltwater trigger for GS-1 and the YD.

Bondevik et al., 2006: This is quite an interesting paper that we somehow had missed previously, and we thank the reviewer for bringing it to our attention. Their Figure 3 is particularly striking, and shows a pronounced inflection point in the radiocarbon concentrations precisely at the Laacher See eruption (Panels B, C, and D). They state that *‘A high reservoir age during the YD could be explained by a combination of increased sea-ice cover and reduced advection of surface water to the North Atlantic’*, both of which are entirely consistent with our proposed positive feedback mechanism. We have included this reference to the radiocarbon data within the context of a more substantial discussion of the positive feedback.

Bakke et al., 2009: The correlations between the data presented in this paper and the Laacher See eruption are remarkably strong, and we will now mention this in our revisions. In particular, Supplemental Figures S5 (noting of course that their data is in years b2k) and S7 show that the largest inflection point is coincident with the eruption. We thank the reviewer for bringing this supplemental material to our attention.

Specific comments:
L30: An alternative route has been proposed involving the drainage of the Baltic Ice Lake, the timing of which precisely coincide with the start of GS-1 (Muschitiello et al., 2016).
Response: This is now discussed.

L42-47: Without entering into the discussion on the credibility of the "impact" theory, I think that the evidence is still undermined by the poor chronological accuracy of the platinum/iridium anomaly.
Response: We now have an entire new section of almost 1,000 words discussing the other leading theories in more detail.

L54: What stratigraphic frameworks? Please specify and provide reference.
Response: We have now provided more context regarding this statement.

L57-58: ": : :consistent with the LS eruption: : :" Please add here "as recorded in varved and 14C dated lake records".
Response: We have reworded this sentence.

L148: Please provide reference here on the impact of volcanic forcing on AMOC (e.g. Otterå et al., 2010).
Response: We have now added this reference as well as several others in a more detailed discussion of the feedback.

L162: Please specify what model. This is not clear.
Response: This has been rephrased to be clearer.

L176-192: This section is all very speculative. Please see my comment on the possibility of examining climate model output to support these claims.
Response: We have added a substantially strengthened section regarding relevant published climate model results.

L181: None of the studies cited here present direct evidence of sea ice changes (only indirect and mainly based on terrestrial reconstructions). Please consider referring to marine reconstructions (e.g. Cabedo-Sanz et al., 2013).
Response: We have now added this reference.

L223: I'm not convinced that the data in Figure 5a and b follow a Gaussian distribution. Rather, the frequency distributions seem skewed. Also the authors cannot claim that "a Gaussian distribution exist" when they fit a Gaussian-best-fit model to their data. They should use a resampling/bootstrap method to draw from their empirical distribution and only then establish its shape.
Response: We have changed the wording in the main text. We have left the wording the same in the figure caption, because the grey-filled curve is a Gaussian distribution, and this describes the shape of the data.

L219-230: I suggest estimating the frequency of Greenland cooling relative to the number of volcanic events occurring at the end of the preceding interstadial. This should somewhat inform on the potential link between volcanisms and the onset of stadial cooling.
Response: Previously published research (e.g., Zielinski et al., 1997; Sternai et al., 2016) has already made the connection between increased frequency of volcanism and deglaciation, and this is mentioned a couple of times in the text. The number of volcanic events relative to the number of

Greenland cooling events is discussed in detail in one of our prior publications (Baldini et al., 2015) and already mentioned in this manuscript.

L246: The rate of cooling (as seen in the d18O ice-core stratigraphies) is similar among most of the stadials, not exclusively between GS-1 and GS-20.
Response: Agreed, and we would argue that the cause is similar. Baldini et al. 2015 showed that every well-dated, large volcanic eruption over the period 30-80 ka BP is within dating errors of a stadial. The post-eruptive positive feedback is likely to be identical between GS-1 (Laacher See), GS-20 (Toba), and many (most?) other stadials – GS-12 (Opala), GS-9 (Campi Flegrei), etc. Unfortunately most eruptions are still very poorly dated, but we would venture that every sulphur-rich magnitude 6 (or above) eruption occurring during intermediate ice volume conditions resulted in the positive feedback. We have added a substantial amount of text that will hopefully make this clearer.

L247-249: The initial cooling can be tested using ice-core (e.g. d15N) and other proxy based temperature reconstructions and compare the magnitude of the temperature change with that associated with large historical eruptions.
Response: We are not sure what extra information this would provide. There is no doubt that the eruption happened, and no doubt that it contained considerably more sulphur than the climatologically important Pinatubo eruption. So it almost certainly did result in cooling, but maximum cooling probably persisted for one year (more subdued cooling would have lasted for ~3 years). This would be very difficult to detect in an ice core, and even more difficult to attribute to the eruption. Even if detectable, the cooling could also be ascribed to a meltwater pulse or a bolide impact. Regardless, existing d15N reconstructions (Buizert et al 2014, Science) are fully consistent with the manuscript, and we have included these references in the discussion.

L253: Is this claim based on ice-core d18O profiles? I don't see any substantial warming in Greenland during the second half of GS-1. d18O of ice can be misleading due to changes in moisture source of precipitation. Before making this claim I would check the ice-core temperature records (δ^{18}O diffusion or d15N). As far as I can tell from the d18O data both GS-1 and GS-20 in Greenland were characterised by cold conditions throughout the stadial.
Response: There is abundant evidence that the maximum cooling during GS-1 occurred at around 12.65 ka BP, and that this was followed by moderate gradual warming. This is apparent not only in NGRIP and GISP2 δ^{18}O, La Garma Cave (Spain) δ^{18}O, Chauvet Cave δ^{18}O (France), Lake Ammersee δ^{18}O, etc., but also in ice core nitrogen isotope ratios and deuterium records. We have added add some references to this statement to clarify.

L258-260: Again, this is extremely speculative and I don't think there is any evidence supporting this claim.
Response: Please see our response to the point above. We do not see how this is 'extremely speculative' when there is abundant evidence supporting the claim. We have now added more references to further support the statement.

L275-284: The magnitude of the spike does not necessarily scale linearly with the magnitude of the eruption and could solely depend on the proximity of the volcanic source or the atmospheric circulation pattern.
Response: This is true, and we noted this in our previous submission when we state that the large spike preceding our candidate LSE spike is a small Icelandic eruption of Hekla. We have added some extra text to the manuscript to ensure that readers are clear on this point.

L290: Age uncertainties for the LST and the sulphate spike should be reported on their respective time scales (i.e. GICC05, IntCal13, MFM varve time scale, etc.). In addition, time scale offsets between the 14C time scale and GICC05 should be accounted when comparing radiocarbon-based and ice-core ages (e.g. Muscheler et al., 2014).
Response: The timing of the sulphate spike relative to the LSE is based on layer counting in the Meerfelder Maar core from the LST to the Vedde Ash, and then layer counting from the Vedde Ash to the position where the LSE should be (and where we find a sulphate spike). We will clarify this in the section where we discuss the sulphate spike. Muscheler et al., 2014, note that *"…there is no evidence for any significant difference between the GICC05 ice-core and 14C time scales at around 13,000 yr BP."* and we now mention this.

L291: Please see my previous comment. Cooling in the North Atlantic started a few centuries before the onset of GS-1.
Response: This comment is not correct. We refer the reviewer to our Figure 3, and the original records referred to in that figure, that all clearly show cooling beginning at around 12.9 ka BP, not a few hundred years previously. This comment seems to lean heavily on Muschitiello et al., 2015 Nature Communications, but does not seem to consider the many other excellent records that do not show this early cooling.

L305-306: As I mentioned above, it would be helpful to look into the frequency of stadials relative to the number of volcanic eruptions during the preceding interstadials.
Response: Please see our response to the reviewer's same comment above (comment on L219).