**Author comment (response to reviewers comments) on: 'Abrupt shifts of the Sahara-Sahel boundary during Heinrich Stadials' by J. A. Collins, A. Govin, S. Mulitza, D. Heslop, M. Zabel, J. Hartmann, U. Röhl, and G. Wefer.**

**We would like to thank both reviewers for their thorough and constructive comments. Below, we address each of the comments in full. The original comments are given in italic, our responses are highlighted in bold and changes to the manuscript are underlined.**

## Anonymous Referee #1

Received and published: 24 January 2013

*This is an interesting paper that provides new paleoceanographic data from the eastern Atlantic concerning the development of the boundary between the Sahara desert and the Sahel since around 60,000 years ago. I think this is a useful contribution which will be of wide interest to the community trying to understand the development of climate around the North Atlantic, as well as controls on desertification and its links to other climate events most notably variations in solar insolation and in the degree of glaciation at high latitudes. The authors attempt to quantify the dust component in cores taken along the West African margin and attempt to show that times of enhanced dust sedimentation correlate with Heinrich stadials. They also indicate that there is a latitudinal shift with time that suggests a major equatorward motion of the boundary again mostly during Heinrich stadials. The authors do a relatively convincing job in demonstrating links between modern desertification and the marine records and there is clearly something coherent in the history that they present which requires the community to think carefully about the patterns. If I had any significant concerns about the study it might be that the effects of bottom currents are not fully understood. Although they have attempted to account for the longshore currents the effect of transport across the margin is less well constrained. It is interesting to note the relatively high proportion of river sediment versus dust sediment in these cores is partly inexplicable as a result of current activity. However such a solution does leave the reader wondering that if this is such an important process could this not also be governing the large-scale excursions that paper focuses on. Nonetheless, on balance I believe that the story is probably essentially correct and so I support publication.*

*1) Page 125 line 4 - late Holocene section of the sediment cores - Can you tell us exactly how old the late Holocene samples would be? How did you select and from what depth? What is the purpose of the late Holocene samples compared the surface samples? Can you explain the rationale?*

**The late Holocene section of the sediment cores represents the mean value between 3 ka and the core top (see section 2). The age of the late Holocene section varies slightly between cores because the core top of our gravity cores does not always correspond to the present day (the age of the core top can be up to 2200 years; core GeoB9528-3), mostly due to loss of surface sediments during coring. Because of different sedimentation rates,**

**the late Holocene represents the top 4cm in core GeoB9528-3 and the top 31cm for core GeoB7920-2. As can be seen from the data (Fig. 2), the calibrated scanner data are subject to some scatter. As such, to make a robust estimate of dust% for the late Holocene (i.e. an estimate of the modern day), we took a mean of several measurements from the top of the core rather than one sample at the very top of the core.**

**The surface samples are from a published dataset of surface sediments spanning the Atlantic Ocean basin (Govin et al., 2012). Although, the age of the surface sediments samples from Govin et al. (2012) is not known, these samples are based on the uppermost 1 or 2 cm of the sediment and should hence be of comparable age to the late Holocene. We use these surface samples as additional samples to reinforce the message that West African ocean margin sediments reflect the input of dust and river material from the adjacent continent. These samples are also used to give us an estimate of the uncertainty associated with the sediment composition, arising from possible differences age, differences in water depths or differences in sediment partitioning between the samples. We plot them along with our cores to show that our late Holocene core data fit with the regional pattern of Govin et al. (2012).**

**Although there are slight age differences between each of the late Holocene sections and probably also between the late Holocene and the surface samples, records from this region suggest that climate and variations in dust input during the last 3 ka were relatively minor compared to changes over the last 60 ka (e.g. during the Last Glacial Maximum and Heinrich Stadials). In addition, the uncertainty on the regression between dust and latitude (9% dust), which is partly attributable to differences in age, is propagated into the final uncertainty on the position of the Sahara-Sahel boundary.**

**In section 3 (paragraph 4), we re-iterate that the late Holocene represents sediment between an age of 3 ka and the core top. In the methods section we now explain our selection of samples for which we calculate the late Holocene values. We also explain that we calculate dust% for the dataset of Govin et al. (2012) and state that this data is included to show that our late Holocene dust% values are robust and to provide us with an estimate of the compositional variation along the margin. We state that th3 modern day compositional variation is incorporated into the final uncertainty on the position of the Sahara-Sahel boundary (section 3, paragraph 5).**

*2) Page 124 line 10 - uncertainty is represented as non-parametric 68 % confidence intervals - Can you explain why this particular interval was used? Is this a common convention?*

**Confidence intervals are used to estimate the reliability of an estimate, in this case the reliability of the median of 500 bootstrap iterations or the reliability of the linear regression. We use 68% confidence intervals because they are analogous to 1 sigma uncertainty – hence this approach follows common convention.**

**In section 3 (paragraph 3) we state that 68% refers to 1 sigma uncertainty.**

*3) Page 126 line 6 - The low dust% values are hence thought to be due to lateral ocean advection - if the concentration of dust in the sediments can be changed very strongly as a result of ocean currents then is it not possible that the variations we see in the cores are driven by changes in ocean currents rather than in the intensity of aridity in the adjacent continent?*

**This response also applies to comment 27. Surface currents operate in both directions off the coast of West Africa. The Canary Current and African Coastal Current flow southwards, while the Mauritania Current and North Equatorial Countercurrent flow northward (e.g. Mittelstaedt, 1983). Central Waters are dominated by the Mediterranean Intermediate Water and bottom currents by the North Atlantic Deep Water.**

**Ocean currents appear to spread out the finer (i.e. river-derived) fraction of the sediment in both a north and south direction: for example the Senegal mudbelt extends both north and south of the Senegal river mouth (Domain, 1977). Nonetheless, as we state in the manuscript, the gradient between dust-dominance off the coast of the Sahara, versus river dominance towards the Guinea (Fig. 1c) coast is still preserved in the transect of late Holocene/surface samples, suggesting that currents are not the dominant control on sediment distribution (and dust% values) for the modern day. It seems, however, that the distinction between the two sources is perhaps 'blurred' by the effect of currents. The coarser fraction of the sediment (i.e. dust; Stuut et al., 2005, Mulitza et al., 2008) should not be affected by ocean currents because it sinks too rapidly to be transported any great distance (Grousset et al., 1998, Wefer and Fischer, 1993).**

**We agree that one possible explanation for the increased dust% values during Heinrich Stadials could be winnowing or advection of fine material out of the region by stronger currents. On the Mauritanian Shelf, for example, reworking processes redistribute sediment today (Michel et al., 2009), although this is on a smaller scale and in much shallower water than our cores (which are at 2300-3200m water depth). One possible ocean current change during Heinrich Stadials could be an enhancement of the northward flowing Mauritania Current. However, we suggest this to have been unlikely because it would have brought more clay into the area from the south of the region (e.g. Govin et al., 2012) and thus would not explain the increased dust% during Heinrich Stadials. Therefore, during Heinrich Stadials a more likely scenario would be that the southward flowing Canary Current and African Coastal Current were enhanced or shifted southwards as a response to stronger trade winds. The influx of mid-latitude waters associated with a stronger Canary Current might in turn explain cooler sea surface temperatures during Heinrich Stadials (Niedermeyer et al., 2009). In the case of a much stronger Canary Current and African Coastal Current, fine/river material that is normally carried north from the Senegal today would be more likely to be carried south. Similarly, any material that is normally carried south would likely be carried further south. As such, we would expect a southwards transport of material and hence a relative increase in river material (i.e. a decrease in dust%) in cores 3 and 4, which are south of the Senegal River. However, in our data we see an increase in dust% in all of our cores and thus we suggest that an increase in strength of the Canary Current / African Coastal Current cannot explain our data.**

**Another scenario could be complete removal of the fine fraction from the entire West African margin between 21°N and 9°N. However, this would have required extreme current speeds across the entire margin. It seems more likely that currents would result in redistribution of material at the scale of our transect or smaller (as is seen on the shelf; Michel et al, 2009).**
**Another possibility would be winnowing of the fine fraction by bottom currents. It has been shown that the core site in this region was likely bathed in the Antarctic Bottom Water during Heinrich Stadials, rather than North Atlantic Deep Water (Niedermeyer et al., 2009). However, it is unlikely that the Antarctic Bottom Water was fast enough to cause winnowing. Moreover, if this was the case, we would expect a northward transport of fine material during Heinrich Stadials, which we do not see. It seems more likely that if the northward flow of the Antarctic Bottom Water was significant, it likely acted to counteract any increase in the Canary Current. Moreover, another core, that is not thought to be affected by bottom currents (due to its location on a seamount) also displays an increase in dust input during Heinrich Stadials (Jullien et al., 2007), suggesting that the increase in dust in our cores cannot be attributed to bottom currents.**
**Finally, sedimentation rate holds some clues as to the likelihood of advection or winnowing of fine material removal by surface and bottom currents. As mentioned in the manuscript, sedimentation rate of Heinrich Stadials is rather difficult to calculate, since it is rare that the radiocarbon age control points are positioned at the start and end of the each Stadial. Nonetheless, for core 2, there are age control points within Heinrich Stadial 1 and they do not suggest a decrease in sedimentation rate, which would be expected if advection of fine river material were the cause of the increase in dust%.**
**In summary, ocean currents may exert some control on our dust% records, but they probably only explain differences between the records rather than the coherent patterns that we see.**
**<u>In the manuscript, we have expanded our discussion of the effect of surface and bottom currents on the dust% signal including the above points (section 5.1, paragraphs 2-3). In addition, as requested in comment 27 we have included a bathymetric map in Fig. 1a and a description of surface and deep currents in section 2.</u>**

*4) Page 126 line 23 - Finally, increased turbidite activity during HS - I have already forgotten what HS means, highstand? Heinrich stadial? In any case perhaps some of the variation we see in the sediments is caused by preferential transport not along the coast but perpendicular and into the deep water. Could the downslope transport be in the form of more gradual currents rather than as more catastrophic turbidites?*
**HS means Heinrich Stadial. As also suggested in comment 13, we have removed the abbreviations throughout the paper.**
**In this region, the strongest currents are parallel to the shore (see comment 3). Bottom currents perpendicular to the shelf are not able to transport coarse material to the depth of the core sites, as is suggested by**

**modern-day grain-size distribution (Koopman, 1981). Most sedimentological studies from this region suggest that turbidity currents are required to transport coarse material to the core sites at the lower slope and do not invoke gradual currents perpendicular to the coast (e.g. Henrich et al., 2010, Hanebuth and Henrich, 2009). Finally, increases in dust in a core taken from a seamount off West Africa (Jullien et al., 2007) would also argue against the gradual supply of coarse material originating from the shelf as the cause of increased dust during Heinrich Stadials.**

**We now state that turbidity currents are the main method by which relatively coarse material is transported down the slope (section 5.1, 5th paragraph).**

*5) Page 128 line 5 - because of the latitudinal position of the continental dunes - If the location of the continental dunes is so well known then why is it important to have the marine record at all? Can't we constrain the SSB just using these as indicators of the boundary?*

**This point is linked to comment 25 on the novelty of the study. Relict sand dunes in Africa have been well documented (e.g. Grove 1958) and represent a good indicator of the position of the Sahara-Sahel boundary. They are indeed positioned on the continent themselves and thus circumvent any additional controls such as ocean currents or wind strength which could potentially modify the position of the sedimentary Sahara-Sahel boundary in marine records. However, sand dunes are commonly remobilized and thus destroyed during subsequent periods of aridity (Kocurek, 1991; Chase, 2009). The dune record is hence quite fragmented and may be biased towards the most recent phases. In addition, dunes are difficult to date. The most extensive period of dune formation in the Sahara and Sahel was the Ogolian phase (12-24 ka), a relatively long period which is commonly attributed to aridity/wind strength during the Last Glacial Maximum (e.g. Talbot, 1980). However, short millennial-scale dusty events such as Heinrich Stadial 1 (15-18.5 ka) took place during the Ogolian period. Because of the fragmented nature of dune records, these short events are very likely missing /overprinted by later dune phases or have simply been grouped together with and not distinguished from variations attributed to the Last Glacial Maximum. In contrast, marine records are continuous and relatively easy to date and as such act to 'fill in' the gaps in the dune record. Therefore by comparing the marine and continental records, we are able to build up a fuller and more robust picture of the evolution of the Sahara-Sahel Boundary.**

**We have now expanded on these points in the introduction (section 1, paragraphs 2, 4 and 5). We also include an additional reference to Chase (2009) which highlights the difficulties surrounding the climatic interpretation of sand dunes.**

*6) Page 129 line 15 - the coarser fraction is normally only be transported by wind – But sand can also be transported in rivers and then reworked across the shelf by current activity. The sand could even originally have been eroded from dunes on shore via the rivers.*

**Our statement regarding the transport mechanism of coarser versus fine particles is based on measurements of river and dust material which suggest that dust, although it has a wider size range, is generally coarser than river material (e.g. Stuut et al., 2005, Gac and Kane, 1986). As such, because we have excluded ocean currents and turbidity currents, the coarse-grain material must be transported by wind. Although it is feasible that a stronger river flow could transport larger grains in suspension, the grain size of the Senegal suspended loads remains remarkably stable between the wet season and dry season (Gac and Kane, 1986), suggesting that the size and composition of the riverine suspension load does not change dramatically. Even if stronger river flow (wetter conditions) did increase the size of the river bedload in the past, once this coarse material enters the ocean, it would sink relatively rapidly and be deposited on the shelf because coarse material is not thought to be transported any great distance (Grousset et al., 1998, Wefer and Fischer, 1993). In any case, there is no reason to think that the increased dust% during Heinrich Stadials is due to stronger river flow, because hydrological proxies (Niedermeyer et al., 2010) and climate models (Mulitza et al., 2008) suggest drier conditions in the Sahel during Heinrich Stadials.**
**We have included the reference to Stuut et al.. (2005) regarding the grain size of dust and river material in the setting (section 2, 2nd paragraph). We quote the data of Gac and Kane (1986) which shows that the size of Senegal River material is relatively constant (section 2, 1st paragraph). We discuss the possibility of sand erosion/transport by rivers (section 5.2, 1st paragraph). We suggest that this is unlikely to be transported to core site depth by gradual currents due to rapid deposition (section 5.1, paragraph 2). Also, we point out that increased dust% during Heinrich Stadials is anyway unlikely to be due to increased river strength because of the evidence for aridity during Heinrich Stadials (section 5.2, 1st paragraph).**

*7) Page 130 line 10 - Our data thus suggest a gradual response to insolation forcing – Is it possible that any of this increased dust since the middle Holocene is generated by human settlement and agricultural development in the Sahel?*
**This is unlikely because the main increase in human settlement and agricultural development in the Sahel was during the last 200 years (Mulitza et al., 2010). Since this question has already been directly addressed in Mulitza et al., (2010), and particularly since our cores are not of high-enough temporal resolution (and also our core tops are not always recent enough to cover this time period), we prefer to refrain from commenting on this in our manuscript.**

*Technical corrections*

*8) Page 120 Line 13 - during Heinrich Stadials - I think it would be helpful if the authors provided numerical ages as well as just saying “stadials”. Likewise I think for the “Last Glacial Maximum”.*
**The ages of Heinrich Stadials have been provided (section 4, paragraph 2), based on the NGRIP record. Moreover, in section 1 paragraph 4, we point**

**out that the term Heinrich Stadials has been used in Mulitza et al. (2008). Last Glacial Maximum is based on the MARGO (2009) definition (19-23 ka).**

*9) Page 120 Line 14 - SSB position – I recommend against using abbreviations but if you really must do this then you need to define them before using them.*
**For the sake of clarity, we have removed the abbreviations throughout the manuscript.**

*10) Page 121 line 2 - Sahara desert during was extended - you need to delete the word "during".*
**We have removed the extra word.**

*11) Page 121 line 5 - 14 N - I don't think there should be a space between ˚ and the number. Please try and fix this throughout the whole paper. Likewise for < and >*
**Unfortunately this is out of our control (the typesetters have control of this).**

*12) Page 121 line 13 - cal ka BP - ka typically means thousands of years ago. I think that the BP is redundant here and inconsistent with what you were doing earlier in the paper.*
**This late phase of dune formation is based on calibrated radiocarbon ages (from Swezey, 2001). For calibrated radiocarbon ages, the convention states the BP should normally be included. For the other ages in the paper, however, we use ka because the methods are a mixture of OSL or, in the case of our cores, also include oxygen isotope stratigraphy.**
**For consistency with the rest of the paper, we have changed the units of the late phase from cal ka BP to ka.**

*13) Page 121 line 27 - Heinrich Stadials; HS - this paper has far too many abbreviations in it. They make the whole paper unreadable for anyone who is not already an expert.*
**The abbreviations have been removed.**

*14) Page 122 line 16 - Mali-Mauritania region (Fig. 1b) - this region is not marked on the map.*
**We have included countries in Fig. 1b to make the source area clear.**

*15) Page 122 line16 - North-East - I think this should be northeast.*
**We have changed to northeast.**

*16) Page 123 line 6 - We use 4 sediment cores - whole numbers up to 10 should be spelt out. Is there some way that we can find out the precise location of these cores?*
**The precise locations of the cores were given in Table 1.**
**We have spelt out the numbers in the text.**

*17) Page 123 line 24 - analysis using - I think you need to delete the word "using".*
**We have deleted the extra word.**

*18) Page 125 line 13 - during the Bølling-Allerød - this is not shown on figure 2. You need to label that.*
**This is now labeled on Fig. 2.**

*19) Page 127 line 20 - Senegal and Chad - if you're going to mention countries by name I think you need to mark them on the map.*
**The countries have now been marked on the map in Fig. 1b.**

*20) Page 128 line 22 - the Early phase - I think you need to call out the figure at this point.*
**We now refer to the Figure 3 at this point.**

*21) Page 128 line 26 - formed during HS4 and HS5 - can you provide numerical ages as well?*
**Numerical ages have now been provided, as with comment 8.**

*22) Page 129 line 20 - reduction in JAS cross-equatorial insolation - what does JAS mean?*
**JAS means July-August-September.**
**We have replaced the abbreviation with the full meaning, in line with comment 9.**

*23) Page 130 line 16 - slowdown of the AMOC – What does AMOC mean?*
**AMOC is the Atlantic Meridional Overturning Cirulation.**
**We have replaced with the full meaning, in line with comment 9.**

*24) Page 131 line 8 - greater by up to 4âU¿eC during HS – is this because the water is hot °in the Gulf of Guinea or because the water is colder offshore Iberia?*
**This is mostly because water is colder offshore Iberia.**
**We have clarified this point in the text and have included the Gulf of Guinea SST in Fig 3.**

## Anonymous Referee #2

*Received and published: 20 February 2013*

*This paper presents an exercise to analyse the movement of the Sahel-Sahara boundary during last 60 ka based on quantifying the amount of dust in a latitudinal transect of marine cores. Although the exercise is well resolved and statistically sound, I consider this study lacks the effort to fully understand other mechanisms that are influencing the sedimentation of particles at the study sites. In addition, the authors do not attempt to go further on analysing the composition of dust in marine cores and they rely on the XRF elemental data without exploring the mineralogy or even the isotopic composition (Sr and Nd), both probably more sensitive analyses to track the Saharan dust particles in marine sediments that are available for other marine cores. Nevertheless, in spite of these concerns that are* *further developed below, I believe that the paper merits publication in Climate of the Past, once the following remarks are considered.*

*25) Novelty of the study. Last paragraph of the introduction justify why this is an interesting study and what the novelty of the presented results is. To me, the originality or the novelty of this study is not well explained (neither in the introduction nor later in the manuscript). The authors should better justify the reasons to carry out this work in the context of many other publications that have reconstructed dust variability during last glacial cycle on marine sediments from NW Africa. Some of those previous studies from NW African margin sediments (Jullien et al. 2007; Mulitza et al. 2008;Tjallingii et al. 2008; Itambi et al. 2009; Zarriess et al. 2011) and, previously, from the Western Mediterranean (Moreno et al. 2002) already recognized an increase of dust input during HE, due to an increase of aridity in Africa, similar conclusions reached by this new study. Besides that, all the data used in this study were already presented in other papers where XRF core scanner data were also employed to discriminate the eolian fraction in marine sediments (eg. GeoB9526-5, Zarriess et al. 2011). Thus, since similar conclusions were already attained by previous studies and almost all the presented data were already published (including the data to construct their model, see S1 in Supplementary material), I recommend the authors to be more specific in the introduction when stating the novelty of this study. Particularly, they should highlight the importance and novelty of the statistical methodology followed to construct a curve of dust amount for every marine core that is later transformed in a curve of SSB position.*

**The advantage of our study over the previous ones is twofold. Firstly, the unmixing analysis (statistical methodology) allows us to quantify the elemental data in terms of dust versus river content and also quantify the associated uncertainty. This is based on dust and river sediment compositions that have been measured on terrestrial samples. As such our record represents a more meaningful parameter than elemental ratios, for example, and thus in itself provides us with new information. Secondly, by quantifying the same parameter across four cores, we are able to directly compare the response recorded in each core i.e. compare changes in West African climate at different latitudes. Furthermore this allows us to interpolate between the cores and form a continuous series of maps over the last 60 ka. As such, we are able to reconstruct the spatio-temporal evolution of dust input to the West African margin, which has not been performed before. As such, although previous studies were able to assess that Heinrich Stadials were dusty (supporting our methodology), we go one step further by assessing the position of the Sahara-Sahel boundary and comparing with continental records of sand dune mobilisation. Although some of the raw data that we use for the unmixing analysis have been published in raw form, either as test data for a numerical model (GeoB7920-2, GeoB9508-5; Bloemsma et al., 2012) or as scanner elemental data (GeoB9526-5; Zarriess et al., 2011), they have not been used in the same form as we do (i.e quantified as dust). We are the first to publish the full dataset of every core together and use it to interpolate between cores. Finally, many of the data are actually new. The full dataset (scanner and**

**powder data) for core GeoB9528-3 is new, as are the powder data for GeoB9526-5.**
**<u>We further clarify the novelty of the study by emphasising the lack of spatial reconstruction on millennial timescale (section 1 paragraphs 1 and 2, and abstract). We further highlight that we quantify the elemental data in terms of dust% (section 1, paragraph 4 section 3, paragraph 3) and are able to calculate the associated uncertainty on these values. In addition, we emphasise that our approach allows us compare and interpolate between several cores, which allows us to reconstruct the spatio-temporal evolution of dust input (section 1, paragraph 4). Finally, we point out that this is useful for comparison with (and is complementary to) other lower resolution studies such as dunes records (section 1, paragraphs 2, 4, 5).</u>**

*26a) End-member modelling. To me, the end-member unmixing model lacks the adequate number of samples, especially for river material since it is constrained by just one river without specifying if the 9 selected samples represent a special section of the river. Since Senegal river is 1790 km long, I suspect the particles in suspension would probably have different geochemical composition from the headwaters to the mouth). Those data come from a relatively "old" paper (Gac and Kane, 1986). Dust end-member is not fully constrained either. It is said (Page 122, Line 23) that dust end-member, compared to river end-member, is enriched in Si and K. Checking carefully both tables in Supplementary material (soil and aerosol data versus river sediment data) it is clear to me that the amount of K is really similar in both end-members (3.1 +/ 0.2 vs 3.5 +/ 1.4) making uncertain the differentiation of dust and riverine material just based on elemental geochemistry.*
**The data for the river end-member from Gac and Kane (1986) were taken at St Louis, Senegal, very close to the river mouth. It thus integrates the composition of material delivered along the river and reflects the composition of the material which is delivered to the ocean. Moreover, the composition of the suspended material does not vary between the wet and dry season (Gac and Kane, 1986). Additional data on the composition of suspended river material from West African rivers is relatively sparse. However, the composition of suspended river material from major African rivers (Congo, Niger and Senegal rivers) remains relatively homogeneous, in comparison with the dust composition (see table below). This suggests that the data are valid, (even if old), and the assignment of this data as a 'river' end member is robust. Moreover, the Senegal River is from a similar source area (Fouta Djallon) to the other smaller rivers in this area, so there should not be a large influence of rock type on the elemental composition of the Senegal River compared to these other rivers. The dust end-member is constructed using 28 dust samples covering all dust source regions. By considering aeolian material from a range of geographical regions and constructing spectra of end-member compositions via bootstrapping (Mulitza et al., 2010), the variability due to the possibility of shifting source areas is incorporated into the uncertainty of the unmixing results.**
**<u>We now state that the river suspended data are taken from close to the river mouth (section 3, paragraph 2). We state that uncertainty on dust end</u>**

**member is included in the final uncertainty (section 3, paragraph 2). We have removed the reference to K in the text.**

| | | Al(%) | Si (%) | K (%) | Ca (%) | Ti (%) | Fe (%) |
|---|---|---|---|---|---|---|---|
| **Senegal River (Table S1)** | **Mean** | **28.3** | **51.8** | **3.1** | **0.4** | **1.2** | **15.1** |
| **Niger River (Martin and Meybeck, 1979** | | **26.1** | **53.5** | **3.8** | **1.2** | **1.6** | **13.8** |
| **Niger River (Gaillardet et al., 1999)** | | **29.7** | **43.9** | **3.6** | **0.6** | **1.6** | **20.6** |
| **Congo River (Sholkovitz et al., 1978)** | | **31.1** | **46.4** | **1.8** | **1.1** | **1.0** | **18.6** |
| **Congo River (Martin and Meybeck 1979)** | | **25.7** | **52.4** | **2.6** | **1.8** | **1.8** | **15.6** |
| **Dust (Table S1)** | **Mean** | **13.1** | **69.1** | **3.5** | **5.4** | **1.2** | **7.7** |

*26b) I would suggest including in this end-member modelling exercise other parameters such as grain-size, mineralogy or Nd isotopes that would be more sensitive to the origin of the material. At least, the reasons to not consider those indicators should be explained in the manuscript.*

**The main difference between dust and river material is Si and Al. This is evident in Supplementary Figure 2. Si/Al basically reflects the proportion of quartz grains versus clays i.e. the two main differences between the dust and river material. As such, dust% and Si/Al are also tightly linked to grain size (compare, for example grain size data of e.g. Tjallingii et al., 2008, Mulitza et al., 2008 with our dust% data). Therefore in this region, our end-member model is actually very sensitive to the origin of material. In this region, grain size and mineralogy would also reflect the proportion and size of quartz grains versus clay material and as such would bring little extra information to the manuscript.**

**The reviewer also suggests that Nd isotopes are a useful indicator of source. However, a number of studies using Nd isotopes conducted on sediments off West Africa have not identified a change in the source area of Saharan dust over time (Skonieczny et al., 2011, Cole et al., 2009, Meyer et al., 2011, Grousset et al., 1998). Nd isotopes also do not distinguish between Sahara and Sahel (Meyer et al., 2011). In our study, we took advantage of the speed of XRF analysis, that allowed us to produce almost continuous high-resolution records across four cores, which allowed us to reconstruct the spatio-temporal evolution of dust input. Performing such a high resolution spatio-temporal study with Nd measurement would be unfeasible due to the analytical time required and even if it was possible, it would bring little extra information. Finally, the spatial pattern of the core top samples gives us confidence that our method reflects changes in dust versus river material.**

**We now state that dust is dominated by quartz grains and river material by clay (section 2, paragraphs 1 and 2). We emphasise that dust% is mostly reflecting Si/Al, which is in turn linked to grain size (section 3, paragraph 2; Supplementary Figure 3). This implies that, in this region, grain size and mineralogy would be unlikely to bring additional information. We also**

**<u>comment on the relative speed of XRF analysis, which made it possible to analyse four cores at high-resolution (section 1, paragraph 5).</u>**

*26c) My last concern regarding this topic is related to my first comment on the novelty of this study. I would ask the authors to clarify what is new in their end-member modelling respect to the model presented in the Nature paper by Mulitza et al. (2010). If I am right, authors of this study used the same samples, same data, and applied same methodology (see supplementary material in Mulitza et al. (2010). Thus, it should not be presented in the Methods section (Page 123) but just citing the source.*

**The modeling procedure is similar to the study of Mulitza et al., (2010) and is described in full in Mulitza et al. (2010) as we stated in the Methods section. However, the Mulitza et al. (2010) paper focused on short timescales that are relative to human activities (last 2000 years). Here, we apply the same approach to several sediment cores to address different questions on longer timescales – i.e. to reconstruct the evolution of the Sahara-Sahel boundary. The reviewer suggests that we should exclude a description of the unmixing methodology in the methods section. However, we maintain that it is worthwhile to include a brief description of the end-member modeling approach in the methods section, to aid the reader's understanding of the approach. It is also necessary to include the list of modern river an dust samples used in our study to allow the reader to repeat or verify our results.**

*27) Controls on past sediment composition. Discussion on factors that control sediment composition is well written, concise and clear but particularly one of the factors may be further explained. I think that N-S advection by bottom currents may be more important than authors state in the manuscript (Page 126, line 10). Sediment reworking, partitioning and winnowing depends on local factors, it is true, but it is important to consider that all four studied cores are located under the same oceanographic setting, so under the same "local" factors. A bathymetric map and more information about surface and deep currents would be necessary to really discard the influence of those "local" processes on the final results.*

**The effect of sediment reworking, partitioning and winnowing owing to regional-scale surface and bottom currents has been addressed in comments 3 and 4.**

**<u>In the manuscript, we have added a bathymetric map (Fig. 1a) and more information about surface and deep currents in section 2, paragraph 3. We now discuss in detail the possible influence of these currents (section 5.1 paragraphs 2 and 3).</u>**

*28) Mechanisms causing equatorward shift of dunes during HE. This study shows a connection among the SSB position and HE, indicating an increase in aridity during those cold episodes. Although this idea is not new, it is in some way "quantified" and "mapped" in this work thus being of interest for the community. About the mechanisms, I am missing the classical idea of Rea (1994) that argued the necessity of arid climate (but not hyper-arid climate) to have the ideal conditions to* *generate and mobilize dust particles since some humidity is necessary for the weathering and dust particles generation. I am not sure if such a climate is coherent with dune formation or with Heinrich Events and may be further developed in the paper. Finally, some other studies that also marked an increase in dust input during HE but in the Mediterranean (eg. Si/Al ratio in Moreno et al. 2002) should be cited and maybe included in the final figure (Fig. 3).*

**Dune formation and mobilization requires relatively arid conditions in order to remove vegetation and mobilise dunes. However, as with dust mobilization (e.g. Rea 1994), dune formation is also limited by the supply of material (e.g. Chase 2009). Consequently, both dust export and dune formation are a function of aridity, wind strength and supply. Heinrich events are associated with a reduction in precipitation in the Sahel (Mulitza et al., 2008, Niedermeyer et al., 2010) i.e. a shift from semi-arid to arid climate in this region. As such we are not suggesting prevalence of hyper-arid conditions during Heinrich Stadials but rather a shift from semi-arid to arid conditions, in line with Rea (1994), which would permit continued supply of material. In addition, it has been suggested that oscillation between humid and arid conditions is an effective way to replenish dust sources and then deflate them (McTainsh 1987). Dunes themselves can be sources of dust (Pye, 1995), when re-activated after a semi-arid stable period (during which the dunes would have undergone weathering and formation of finer material). As such, for the case of West Africa, we propose that the rapid and relatively southward shift of the Sahara-Sahel boundary and aridification of the semi-arid Sahel during Heinrich Stadials would have likely resulted in deflation of these dust sources and re-activation and migration of sand dunes along with deflation of fine material. Following Heinrich Stadials, as the boundary retreated northwards, semi-arid conditions in the Sahel would have then replenished the dust sources.**

**In addition, as stressed in the manuscript, as well as aridity, wind strength was very likely increased during Heinrich Stadials, as suggested by the coarser grain size in the sediment (e.g. Mulitza et al., 2008). It seems likely that if wind strength increased, and coarser material could be deflated, this would have been able to deflate new source areas of material that were previously too coarse for deflation.**

**<u>We have expanded our discussion of the causes of dust deflation and dune formation (section 5.2, paragraph 1). We emphasise the need for supply of material and thus the need for either arid (rather than hyper-arid) conditions. We also emphasise that material would likely be supplied from newly exposed sources in the Sahel during Heinrich Stadials. We also describe how re-activated dunes may themselves be another source of dust. We include the references to Rea (1994), McTainsh (1987) and Pye (1995). We have now also included the reference to Moreno et al., 2002 in the manuscript (section 1, 3$^{rd}$ paragrpah). We also further emphasise that enhanced dust export and dune formation during Heinrich Stadials is likely also due to stronger trade winds.</u>**

*Specific comments*

*29) Abstract, line 14: SSB is not explained before (it should appear in parenthesis in line 11)*

**<u>In line with comment 9 we have removed all abbreviations and now explain terms in full.</u>**

*30) Page 121, Line 14: "Continental pollen records. . .." It should be changed by "pollen records in marine cores" since both references are palynological studies on NW African margin marine cores.*

**<u>We have changed the sentence.</u>**

*31) Page 123, Bloemsma et al., 2012 and Mulitza et al., 2010 are not included in the reference list*

**<u>We now include these references.</u>**

*32) Page 123, Line 24. "by using an end-member unmixing analysis using".*

**<u>We have removed the extra word.</u>**

*33) Page 124, Line 16. It would be recommendable to plot dust % in Fig. 2 of Supplement to really see the comparison with Al/Si ratios.*

**<u>We have included dust% values in Supplementary Fig. 3, in line with comment 26b.</u>**

*34) Page 127, Lines 1 and 2. I wonder if there are SEM images or any other indication (mineralogy, Sr and Nd isotopes, etc) that can really confirm that the material delivered during HE is wind-blown dust and not transported by turbidity currents.*

**SEM images, mineralogy and Sr or Nd isotopes would not be able to differentiate whether the material was delivered as windblown dust and turbidity currents. However, a typical feature of turbidites is a fining upward sequence. Nonetheless, grain-size data from core 2 (Mulitza et al., 2008) do not show any such sequence during Heinrich Stadials. Finally, as was stated in the manuscript, foraminifera incorporated into the sediment of the Heinrich Stadials cannot have come from further up the slope (i.e. delivered by turbidity currents) due to their $\delta^{18}O$ composition.**

**A number of studies (Henrich et al., 2010, Hanebuth and Henrich, 2009) do identify relatively frequent turbidity current activity in this region although these cores are normally from underwater-canyon setting, whereas our cores are not. In addition, these studies do not display increased turbidity current activity during Heinrich Stadials, suggesting that they can have been the cause of increased dust% during Heinrich Stadials in our data. Instead, turbidity currents and downslope transport appear to be prominent during sea-level rise (Henrich et al., 2010), which is not coeval with Heinrich Stadials.**

**<u>We now include the absence of a fining-upwards sequence in the grain size in the discussion section (section 5.1, 3rd paragraph). We also include a larger description of studies documenting turbidites off West Africa and</u>**

**emphasise that they do not display consistent recurrence of turbidites during Heinrich Stadials.**

*35) Page 127, Line 8. Discard sea level as an important factor requires further arguments. Why the influence of sea level is not seen in the records? Please, explain.*

**A previous study (Just et al., 2012) suggested that reduced sea-level would have decreased the distance between the river mouth and the core site, and hence increased the contribution river material to the sediment core. Under certain settings, this may well be the case. However, in our cores, we see no such increase in river material during the glacial period, when sea level was lower. This may be partly to do with the depth of the core. For example, the core of Just et al, 2012 is located at 2400m, whereas our core from this region (GeoB9526-5) is located in deeper water (3200m). Perhaps at greater water depth, the reduction in the core to river mouth distance has less effect on sediment composition. Additionally, however, another core located on a seamount further away from the coast, which would be less susceptible to sea level changes, also displays increased dust input during the glacial relative to the Holocene (Jullien et al., 2007), rather than an increase in river material and is hence in line with our data. Finally, an exposed shelf during sea level lowstands would have been a source of dust (Sarnthein and Diester-Haas, 1977) and hence the dust source would have also shifted closer to the core site. As such, perhaps there is little influence of sea-level in our cores because the distance to both sources decreased (i.e. distance to dust source *and* distance to river mouth).**

**We include the above points in our discussion of the control of sea-level on dust% (section 5.1, 4th paragraph).**

*36) Page 129, Line 16. At the end of this assertion a "classical" reference would be recommended, such as Pye (1989)*

**We now include the reference to Pye, 1989 and also Stuut et al., 2005 and now make this assertion earlier in the manuscript (section 2, paragraph 2).**

*37) Page 130, Line 10. Why "our data suggest a gradual response to insolation forcing"? Please, explain and refer only to the Holocene. . . during HE is not exactly "gradual".*

**We have clarified the sentence and specify that we are referring to the period from the mid-Holocene to late Holocene.**

*38) Page 130, Lines 26-28. The three-phase evolution of HE is not clear. Please, remove. Fig. 1c I suggest marking a line with the present-day SSB position to see the interception with the regression line at 43% of dust.*

**We have removed the comment regarding the three phase evolution. We have included a dashed line on Fig. 1c marking the interception of 19°N with the 43% dust value.**

*39) Fig. 2. Add "core 1, core 2, core 3 and core 4" in the figure and caption since those records are referred with numbers in the text more than with their name (GeoB….).*

**The core numbers were already marked in the figure and the caption.**

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