Response to interactive comments received on "Orbital and millennial-scale environmental changes between 64 and 25 ka BP recorded in Black Sea sediments" by Shumilovskikh et al.

The authors are thankful to both reviewers and editor for constructive critics, comments and useful additional references, all of which helped us to improve the manuscript significantly.

General comments

The major critics of M.F. Sanchez Goñi and U. Müller are related to our interpretation of the long-term trends throughout of MIS 4 to 2 in the vegetation dynamics in Northern Anatolia. Following the recommendations of the referees, we improved our interpretation and provide a major revision of the manuscript. First of all we extended palynological analysis by 5 ka in order to cover the LGM and discuss these new results. As result we revised the abstract, 1 Introduction, 3.2 Palynological investigations, 4 Results, 5 Discussion and 6 Conclusion sections. Points discussed in previous section 5.3 "Land-sea correlation and regional comparison" were moved to other discussion sections 5.1 and 5.2.

We agree with M.F. Sanchez Goñi that the mountainous Northern Anatolia presents a very complicated system, where climatic conditions and, as a result, vegetation depends not only from latitude but also from orography. This aspect is clearly addressed in the section 2. We also state that the main vegetation type in Northern Anatolia is temperate euxinian forest, including warm- and cool-temperate trees with evergreen shrubs and lianas. However, presence of typical Mediterranean vegetation such as *Olea, Phillyrea, Pistacia, Quercus coccifera, Myrthus communis, Arbutus andrachne* around the Black Sea (Pontic Mountains, Caucasus, Crimean, Bulgarian coasts) is related to climatic conditions that are favorable for growing Mediterranean vegetation and thus allow to consider this areas as part of the Mediterranean region s. l.

In order to evaluate the role of summer insolation for the development of forests in Northern Anatolia, we extended our pollen analysis of the core interval, covering the period from 25 to 20 ka. To our surprise, pollen assemblages reveal increasing percentages of AP as well as temperate biomes during the LGM and summer insolation minimum. These new data confirm our conclusion about direct response of the Northern Anatolian environment to summer insolation changes.

Highly discussed question about the driving factor(s) of vegetation changes during the MIS 4 to 2 we left open. In section 5.1.2, we discuss the possible role of orbital parameters such as obliquity and precession, as requested by M.F. Sanchez Goñi and U. Müller. Moreover, we suggest possible scenario explaining forest spread during the LGM as local feature of the Black Sea/Northern Anatolia. Additional comparisons to the carbon isotope speleothem record from Sofular Cave (Northern Anatolia) (Fleitmann et al., 2009), LGM pollen records from Marmara Sea (Mudie et al., 2002) and glacier expansion in Northern Anatolia (Akçar et al., 2007, 2008; Zahno et al., 2010) confirm our reconstructions of relatively wet LGM in Northern Anatolia, which however, was much drier than today.

In section 5.1.2, we also added an additional explanation about the role of atmospheric circulation for delayed extension of forests during GIs as suggested by M.F. Sanchez Goñi.

U. Müller pointed out the contrast between the millennial-scale glacial pollen record 25-GC1 (MIS 4-2) and the long-term record (MIS 5-1). We agree that glacial-interglacial cycles show a different dynamics compared to sub-orbital changes. However, particularities of these differences and their reasons are not in the focus of this manuscript and should be discussed elsewhere.

Section 5.2.1 was changed following recommendations of U. Müller, who requested to consider different causes for the freshening of the Black Sea surface. In accordance, we suggest several scenarios such as humid phases in the Black Sea region, glacier retreatment at the beginning of the MIS 3, possible Caspian Sea – Black Sea connection, seasonal freshening of the sea-surface through floating ice or closer position of the river mouth to the core location due to lake level changes. The extension of the Fennoscandian ice sheet into the catchment area of the Dnieper River indeed had an impact on the Black Sea hydrology, as suggested by U. Müller. However, this event, known as the "red layers" (e.g. Bahr et al., 2006; Soulet et al. 2013), took place during the termination 1 (~18-15.5 ka BP) and is therefore not considered in our manuscript.

Common specific comments

Both referees note the inconsistencies in the terminology for D-O cycles and suggest different ways for nomenclature: D-O interstadials/D-O stadials or warm phases of D-O cycles/cold phases of D-O cycles or GI/GS. We agree with U. Müller suggesting to avoid pre-assumptions in the characteristics of phases for the Black Sea region and choose GI/GS as suggested by M.F. Sanchez Goñi. Terminology was changed throughout the text.

One of the referee's comments attends to salinity, suggesting to add units to the values. However, since nowadays practical salinity is a standard way for salinity descriptions and it has no units. Therefore, we left values without units.

Pollen source area for the core site 25-GC1 is the next question prompted by referees. This question is part of an ongoing study, of which preliminary results are published as a part of the PhD thesis by Shumilovskikh (2013). These results suggest the nearest coastal areas and northern slopes of the Pontic Mountains as the main pollen source area for the marine cores from Archangelsky Ridge (locations of cores 22-GC3/8 and 25-GC1). Our assumption is based on these preliminary results. One explaining paragraph was added to the section 3.2 Palynological investigations.

According to referee's suggestions, we changed the summer insolation curve from 30 to 41°N, where we assume the main source area for the counted pollen.

Particular specific comments

1) M. F. Sanchez Goñi

For consistency, in the abstract, in section 5.1.2 "Abrupt climate changes" and in the conclusions the authors should replace "D-O interstadials" and "D-O stadials" with "the warm phases of D-O cycles" or "GI" and "the cold phases of D-O cycles" or "GS", respectively.

Done. See response "common specific comments"

I suggest to the authors adding the units for the salinity values.

See response “common specific comments”

In the “Introduction section” and section 5.1.2 “Abrupt climate changes”, the authors state that Heinrich events do not strongly impact the vegetation of southwestern and southeastern Iberia (Sanchez Goñi et al., 2000 and 2002). This statement is a misinterpretation of what is written in both papers. Heinrich events produced the rapid replacement of the open Mediterranean forest with semi-desert landscapes. Note that in the 2002 paper quantitative estimation of temperatures and precipitations give strong anomalies in the mean temperature of the coldest month and annual rainfall with decreases by 6 to 13°C and 400 to 500 mm, respectively. The authors should modify the text accordingly.

We state that the impact of HE on the vegetation of southern Iberia was **not stronger than that of GS**. This thesis is based on papers Sanchez Goñi et al., 2000, 2002, citation from which we provide below.

Sanchez Goñi et al., 2000, Conclusions: “The steppic-dominant pollen assemblages, corresponding to both Heinrich and Dansgaard-Oeschger cold events, **are very similar**, making the **distinction of major and minor cold oscillations on the continent difficult**” and further: “However, the nucleus of each Heinrich event, as well as the other Dansgaard- Oeschger stadials events, is always related with **steppic conditions in southwestern Europe**.”

Sanchez Goñi et al., 2002, 4.3: “In contrast, **during the other stadials** the MTCO and the annual rainfall reductions **were larger on the Atlantic** than on the Mediterranean site, **with similar amplitudes** (5–12 °C and 300 mm) **to those of the HEs**”. Conclusions: “In contrast with the Mediterranean site, the **Atlantic site shows similar precipitation and temperature drops for all the D–O stadials, including those related to the HEs**”.

However, since our formulation obviously leads to a misunderstanding, we re-phrased these paragraphs in Introduction and in the section 5.1.2.

In the section “Material and methods” the authors should add a paragraph discussing the main vegetation source area from where the pollen is originated and recruited in the sediments of core 25-GC1.

About pollen source area please see response to specific comments.

Additionally, could the authors clearly state that the dinocyst species used as tracer of productivity are not damaged by acetolysis?

Autotrophic dinocysts, which were used as tracer of productivity, are moderately sensitive or resistant to acetolysis (Marret and Zonneveld, 2003). Additional test for differences in dinocyst concentrations during GI and GS was made for non-acetolysed samples. This test confirmed results gotten from acetolysed samples. These results were not published.

Could they also add the temporal resolution of the analysis?

Sentence about temporal resolution of the analysis was added at the beginning of the section 4. Results

In section 5.1.1 “Long-term dynamics”, the authors refer to the “euxinian” vegetation record shown in Figure 3. However, this record is missing in the figure. Could the authors add this record?

euxinian elements are added

In the same section, the pollen record is compared with the summer insolation curve at 30_N. Why do the authors chose 30_N if the core is located at 42_N, and represents the vegetation of the Pontic Mountains, a region located northern than 40_N?

We agree with referee to take insolation curve for more northern latitude. Because the assumed pollen source area includes coasts and northern slopes of Pontic Mountains, we have chosen 41°N for plotting.

In this section also, the authors should replace “general increase” with “long-term increase”.

replaced

In section 5.2.1 “Long-term patterns “, the authors present two hypothesis to explain the occurrence of subtropical and fully salinity species during the interval 64-25 ka. The authors may add the reference of Dorale et al. (2010) showing that the sea level during MIS 5a was particularly high, close to that of MIS 5e.

thank you very much for the reference!

In section “Land-sea correlation and regional comparison” authors should replace “During MIS 3, the glaciers retreated and climate. . .” with “During MIS 3, the glaciers retreated from their MIS 4 position and climate. . .”. It is also more appropriate to replace “correlation” with “comparison” in the title of this section.

text is replaced, title is changed accordingly

Figure and legends

Figure 1: Contrary to author statement, this figure represents Europe and Northern Africa landmasses rather than the Mediterranean region. The authors should delimitate the real Mediterranean region, which excludes Northern Anatolia (Polunin and Walters, 1985; Bailey, 1998).

title was changed

Figure 2, the authors should explain the meaning of PJA.

explanation is added

*Figures 3, 4 and 6: there is a mistake in the concentration units. The authors should replace “specimens*cm-3 10-3” with “specimens*103*cm-3”.*

replaced

Figure 3 and 4: Replace “Horizontal grey bars. . . indicate D-O events. . .” with “Horizontal grey bands. . . indicate the warm phases of D-O cycles” or “GI”.

changed accordingly

Figure 5: Add the obliquity curve and replace "Marine isotope stadia" with "Marine isotope stages".

obliquity curve is added and words are replaced

Figure 6: Replace "The longest D-O events. . . " with "The longest warm phases of D-O cycles. . ." or "the longest GI". The term "event" only refers to the change, which only takes few decades in Greenland, and not to the interval produced by it.

changed accordingly

Typo mistakes

Page 5451 line 14: Remove "during D-O events".

done

Page 5454 line27: Add "the" before "dominance of P. psilata. . ."

done

2) Ulrich C. Müller

p. 5442, line 22: from Greece instead Balkans

done

p. 5443, lines 4-9: make two sentences

done

p.5444, 5: unit?

See response "common specific comments"

p. 5447: please indicate which taxa are included in the euxinian elements, xerophytic taxa etc.

all groups are presented in table 1

p. 5447: which taxa does the 100% reference include, which taxa are not included?

all identified taxa were included in 100%, except of reworked pollen

5448, 19: it is not a maximum but maxima of temperate biomes at 52 and 46

We agree that sum curve for temperate biomes show maxima at 52 and 46. However, at this place in the text we describe smooth curve of temperate biome, which shows a maximum at ~55-45 ka BP

5450, 14: why does euxinian vegetation indicates that (unclear as it is not indicated which taxa are included)

taxa included in euxinian vegetation are provided in table 1

5450, 21: in the region instead of in the Pontic Mountains

changed

5455, 8ff. these are rather single cysts

Yes, they are. However, their sums show changes through the time, which we discuss in this section.

5451, 15: decreasing temperature? Why not decreasing precipitation?

Text was deleted

5452, 1: to infer long-term trends from the short peaks of Hippophae associated with D-O interstadials 3 and 4 seems a bit risky.

We consider an increase of *Hippophae* in the record from 36 ka BP and not discuss short peaks associated with GI 3 and 4.

5452, 16: why "partly"

Because percentages of *Artemisia* do not show such clear variation correlating with GS like Chenopodiaceae

5456, 4 and 5: temperate tree populations show maxima at 52 and 46 kyr BP but not spreading between 55 and 45. Why should be the increase of humidity related to summer? Why not winter?

This part is re-written: decrease in marine indicators occurred at ~54 ka and coincide with spread of temperate forests at this time.

5458, 22: The Roucoux et al. 2005 record does not show rather wet conditions during MIS 2 Roucoux et al. 1646 state: 31-23 ka appears to be the coldest, most arid part of the period under consideration.

Roucoux et al. 2005 consider period 31-23 ka belonging to MIS 3 (1648: 6.2). As MIS 2 they consider the period 23-17.5 ka and state that climate of the LGM period "appears to have been rather warmer and moister in Iberia than during many stadial events of MIS 3".

5459, 12-17: Not sure the lines are logic: why should minima of available moisture enable forest spreading?

typo mistake, correct

5460, 5: D-O interstadial 14, the most prominent interstadial in MIS 3, started at 55 kyr, and the most prominent summer insolation maximum of MIS 3 was at 57 kyr BP (the 2kyr delay might be response time). Therefore, I can not follow the argument that strongest development of temperate forests occurred near summer insolation minima.

Additional pollen analyses for period 25-20 ka demonstrate increase in AP and temperate biomes with decreasing summer insolation, similar to the period 54-42 ka.