

Interactive comment on “Orbital and millennial-scale environmental changes between 64 and 25 ka BP recorded in Black Sea sediments” by L. S. Shumilovskikh et al.

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General comments:

The paper of Shumilovskikh et al. presents continuous pollen and algal records from the SE Black Sea for the interval 64 to 25 kyr BP with an average resolution of 260 yrs. The data are of great value to reconstruct vegetation dynamics in the region and sea-surface conditions of the Black Sea. The interpretation of the data is focused on long-term trends on orbital time scales and on short-term fluctuations on centennial to millennial timescales.

I need to point out a major and serious concern regarding the interpretation of long term

C2517

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trends. The authors argue that maxima of xerophytic steppe biomes, characteristic for more arid conditions coincide with summer insolation maxima; whereas maxima of temperate forest biomes reveal more humid conditions at a time of minimum summer insolation.

This interpretation is in sharp contrast to what is documented in long pollen records from the Mediterranean region (e.g. from Allen et al., Tzedakis et al., Sanchez Goñi et al., Roucoux et al., Müller et al.) which clearly show the link between summer insolation and vegetation: Summer insolation minima (such as in MIS 2 and 4) result in the spread of steppe or open vegetation, whereas summer insolation maxima (such as in MIS 5e, 5c, 5e, 1) result in the spread of temperate tree populations. See also the paper of Milner et al. (2012, Geology) which shows that the most prominent summer insolation maximum of the last 140 kyr provided most enhanced precipitation.

Shumilovskikh et al. try to argue in a totally contrary way, i.e. maximum steppe/aridity correlates with maximum summer insolation and maximum tree population correlates with summer insolation minima. Indeed, the first glance of Fig. 5 may support this view. However, this is because the interval in consideration captures only subdued variations of summer insolation during MIS 3, but not the prominent minima of summer insolation in MIS 2 and 4, and likewise not the prominent maxima of summer insolation in MIS 1 and 5. If the authors would extend the length of the record and consider the situation during the prominent insolation maxima and minima – and not only look on subdued insolation variations within MIS 3 – than they would not proceed with this doubtful interpretation.

In addition, if the authors would properly use the summer insolation record for the latitude of the site they consider, i.e. 42°N, than the subdued insolation minima at 45 kyr BP would be even weaker because the more north we go the stronger is the influence of the obliquity and the weaker is the influence of precession on summer insolation.

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The argument that the algal record supports the pollen record interpretation of Shumilovskikh et al. is also doubtful. The authors argue that maxima of summer insolation coincide with a reduced freshening of the Black Sea. However, I can not understand why the thick black line (moving average) in Fig. 5h indicates a freshening centered at 48 kyr BP. During the critical 45-to-51-kyr-BP interval data for freshwater algae concentration are either missing or zero.

Still, the freshening trend from 33 to 25 kyr BP (Fig. 5h) is clearly supported by data. However, the coeval decline of summer insolation does not imply that summer insolation minima are associated with more humid summers (as the authors argue on p. 5456). The freshening may have other triggers (such as the Fennoscandian ice sheet that may have extended at that time into the catchment area of the Dnieper River flowing into the Black Sea). Therefore, the argument that summer insolation minima support more humid summers is not convincing.

Back to the interpretation of the pollen record: The trend towards increasing percentages of arboreal taxa during the interval 36 to 25 kyr BP (Fig. 5d) is hard to interpret. The authors argue that this increase of arboreal taxa indicates a trend toward increased humidity (which would fit to the freshening of the Black Sea). However, we need to consider that *Quercus* percentages and pollen concentrations do not increase during that interval. Such an increase would be expected if humidity would have increased. Furthermore, the long Mediterranean pollen records indicate that the MIS 3/2 transition was associated with a general and strong change towards colder and drier climates. This holds also true for the Roucoux et al. 2005 record off NW Iberia which the authors quote (p. 5458) to support their interpretation. Roucoux et al. 2005, p. 1646 state: 31-23 ka appears to be the coldest, most arid part of the period under consideration (they consider the last 65 kyr). The eastern Mediterranean record Tenaghi Philippon shows the same picture: the MIS 3/2 transition provides the coldest and driest interval of the last 70 kyr (Müller et al. 2011).

In summary, I recommend publication provided the authors address the concerns

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raised above. The authors should keep in mind that a link between summer insolation minima and maximum spread of temperate forest taxa (as they propose) would be in sharp contrast to what is known from long Mediterranean pollen records and that their data do not provide comprehensive evidence for such an interpretation. Since the record they present does not cover the situation during the prominent insolation maxima and minima in MIS 1, 2, 4, and 5, but only subdued insolation variations in MIS 3, the paper provides no good evidence for correlating summer insolation with long term trends in the records.

Specific comments

p. 5442, line 22: from Greece instead Balkans

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

p.5444, 5: unit?

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

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

p. 5448: terminology: “D-O event” is the abrupt warming at the onset of D-O Interstadial
5448, 19: it is not a maximum but maxima of temperate biomes at 52 and 46

5449, 3: Terminology: “Cold phases of D-O cycles” pre-assumes that it was cold in the region but the characteristic of the phase could be also aridity. I suggest to use the term “D-O stadial” consistently in the paper as this term does not imply a pre-assumption.

5449, 11: the same: the term “warm phase” implies a pre-assumption. Better would be to use the term “D-O interstadial” consequently in the entire paper as the characteristic of the phase might be rather humidity than warmth.

5450, 10: how do we know that the signal is from the coast; better: in the region

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

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

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

5451, 18ff: See general comments

5451, 23: The general increase of arboreal pollen is mainly due to an increase of Pinus but not of Quercus. Pollen concentration are also low. I do not see that the data clearly support an increase of moisture availability (see general comments).

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.

5452, 16: why “partly”

5455, 8ff. these are rather single cysts

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?

5456, 13: Increase of arboreal pollen suggests rather wet conditions at the end of MIS 3? See general comments: the low values of Quercus and pollen concentration do not support this. Furthermore, this interpretation would be in sharp contrast to other pollen records in the Mediterranean region.

5458, 3: See general comments: I do not see clear evidence in the pollen and algae record that would reveal rather wet conditions towards MIS 2.

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.

5459, 8ff. See general comments

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

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.

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9, C2517–C2522, 2013

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