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Climate of the Past Discussions

Interactive comment on "Climate warming and vegetation response at the end of Heinrich event 1 (16 700–16 000 cal yr BP) in Europe south of the Alps" *by* S. Samartin et al.

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Michel Magny Guest Editor

To

Stéphanie Samartin Corresponding author For Manuscript cp-2012-45

Dear Stéphanie,

Manuscript cp-2012-45 entitled 'Climate warming and vegetation response at the end

of Heinrich event 1 (16,700-16,000 cal yr BP) in Europe south of the Alps' (special issue 'Holocene changes in environment and climate in the central Mediterranean as reflected by lake and marine records') has been reviewed by two external reviewers and by me. The comments of the referees are included at the bottom of this letter.

Both the referees have observed that your manuscript is an important contribution and they have recommended publication. But, they also suggest some moderate revisions to your manuscript. Therefore, as a Guest Editor, I invite you to respond to the referees' comments and revise your manuscript.

I also suggest additional minor revisions as follows.

In the section Introduction, you should made the text more precise by adding (1) the radiocarbon age of the onset of the Bölling warming (see the beginning of the first paragraph : '. . .about 1.5 millennia prior to the onset of the Bölling warming radiocarbon-dated to XXXX yr BP (Vescovi et al. 2007)'), and (2) the age of the onset of the Lateglacial Interstadial in the Greenland ice core (see second paragraph : '. . . in ice-core records from Greenland where the onset of the Interglacial Interstadial is dated to 14650 yr BP (Svensson et al. 2008)'). Thus, precise indications about the general context of your paper are presented to the reader as early as the beginning of the paper.

In section 3.5 'Evolution of the inferred temperatures' : in the first paragraph, the Younger Dryas and the early Holocene correspond to zones ORE-4 and ORE-5 respectively (instead of ORE-2 and ORE-1 in your text). Please, correct the text accordingly.

Figure 2 : please, add a sediment profile along the depth scale.

Figure 4 : in the chronological scale, the strokes to mark millennia should be longer. The same for Figures 5 and 7.

Figure 5 : the indications of units in c) and d) are too small. Please, homogenize with

units of a) and b).

Figure 6 : you should add thin vertical lines to mark millennia and made the comparisons between records easier.

Once again, thank you for submitting your manuscript to Climate of the Past. I look forward to receiving your revision.

With best wishes, Michel

Comments of Referee 1

General comments This is a very challanging paper which illustrate the great potential of Chironomid studies and explore a key period of transition between glacial and interglacial.Its structure is good.

Remark 1 is on the age model. I know how it is difficult, almost impossible to acquire 14C dates from sediments covering the last glaciation and the transition to LGI.the authors admit that the chronology of their sequence is not that robust but they claim that this chronology is sopported by similar evidences from other sites (continental, marine, speleothems). According to the available data, this assumtion and the correlation with HE1 remains still putative. The lack of a 14C at the limit between zones 1 and 2 is thus quite frustrative.

Remark 2 on the transition between zones 1 and 2 : the Chironomids diagram in fig. 3 is in agrement with an "abrupt" change at the beginning ofr a local expression of the "Pre-Bolling", but this evidence (expressed in the text) is less convincing on fig.4 and 5 which suggest a progressive warming. How do the authors explain this discrepancy?

Remark 3 on the general understandinf of the 18 000-14 500 interval. For me this transition is still difficult to understand and the authors could better underline the complexity of the question. For sure they insist on an altitudinal limit in the Alps : below 1000 m asl pollen data show faint evidences of reafforestation and nothing is observed above. But when looking at several pollen sites recording the period before the "Bolling", dated

or not, they show different evidences. For instance, at Monticchio, a never glaciated site, in a region well known as a refugial zone for trees, a "progressive warming" since 16.000 BP could have allowed an early forest expansion. It is not at all the case, and this period is marked by amaximum in Juniperus and Artemisia suggestig a dry phase. Still in Italy, at Lago dell'Accesa, a zone centered on 15 500 BP by tephrostratigraphy marked by a decline of Juniperus and a maximum of Artemisia "suggests more continental" climate. Thus the discussion is still open and fascinating and this paper constitutes an important contribution and an incentive for a deeper exploration of this period.

Technical correstions: On fig.4 and 6 the legends identify image b as a and a as b!

Comments of Referee 2

Samartin et al. present a good and well written paper. The main question (clearly) addressed in this paper is the link between the early afforestation south of the Alps, and a climate warming that possibly occurred 1500 years before the onset of the Bolling. Samartin et al. used a powerful approach: the reconstruction of summer temperature independently of pollen from the Lake Origlio subfossil chironomid record. Temperatures were inferred using an expanded transfer function recently developed by Heiri et al. (2012). The temperature reconstruction inferred from the chironomid record unambiguously showed a climate warming of nearly 2-2.5_C at ca 16 000 cal BP. As concluded by Samartin et al., this would suggest that the early afforestation south ofthe Alps is climatically driven. From these results obtained at Origlio, Sammartin et al. finally discussed the pattern of the Late Glacial climate change over Europe and theMediterranean realm. I am confident with the results produced in this study. I agree with what is said in the discussion and conclusion. That why I recommend this paper for publication in CPD. Nevertheless, I would recommend moderate changes before definitive publication, especially in the Discussion section. As stated by the authors, this study provides the first evidence for an early warming occurring at ca 16 000 cal BP south of the Alps. A similar warming was also found from continental archives in

South-West France, Turkey and Israel. Therefore, the results presented by Sammartin et al. are of great importance for the understanding of climate and vegetation history in Europe. Given this importance, I think the reliability of the temperature reconstruction is not sufficiently assessed in the present form of the paper. The author should add a sub-section dealing with this issue (the reliability of the temperature reconstruction) in the Discussion section. Several items should be discussed: 1-The early warming corresponded to the Biozone ORE-2. This key biozone is composed of only 6 samples (with one sample with counts less than 50 Head-capsules). Furthermore, samples were not taken contiguously along the core (?). According to what is said in the "coring and sediments " sub-section, I suppose that samples were 1 or 2 cm thick (see specific comments). ORE-2 is 33 cm thick. In other words, only ca 40 % of the sediment were analyzed ((6samples*2cm)/33cm). Is this sampling design well suited to provide representative material (and data) for the whole unit? 2-In the sub-section "Interpretation of faunal trend", the authors discussed the main changes in chironomid assemblages and their possible forcing factors. In their discussion (biozone by biozone), the authors identified several possible forcing factors according to the ecology of taxa: lake-level (profundal versus littoral taxa), température (cold versus warm-adapted taxa) and trophic level (oligotrophic versus mesotrophic taxa). The remaining questions which are not addressed in the discussion are: Is temperature the main forcing factor of changes in chironomid assemblages ? This is the pre-requesite for a relevant temperature reconstruction. What about the possible influence of possible confounding factors? Is there a risk for biases induced when inferring temperature from Origlio chironomid ? Could the disappearance of profundal taxa (with cold optima) and the appearance of littoral taxa (with warm optima) characterizing ORE-2 be explained at least in part by a lake lowering? Please note that in a previous study of record from Lake Accesa (Tuscany, Italy, Magny et al. 2006) evidenced a strong lake lowering before the onset of the Bolling. It was the main driving factor of the chironomid assemblages (Millet et al. 2007). 3-In their presentation of the vegetation history (p1619), author stated "This vegetational change stabilized the soils and the shift from sandy silt to silty gyttja shows that the

erosional input into the lake significantly reduced". The change in vegetation cover at ca 16000 cal BP induced a change in lake sediment (erosional input and probably OM type and amount). Can these changes in the watershed and induced changes in the lake sediment be a cause for chironomid changes independently of climate? OM analysis would be helpful: Are these data available ? 4-In the "results" section, Samartin et al. presented in a sub-section "Evaluation of the inferred temperatures" a detailed description of the reconstruction diagnostics. What do these results tell us about the reconstruction? Are there some specific parts of the record where we must be less confident with the reconstruction? This issue is briefly discussed (p1630, line 3: "However, a disagreement of ca 3_C is within the method inherent reconstruction error (+ 1.5-1.6_C SSPE)"). 2.5_C was also the amplitude of the shift in temperature between ORE-1 and ORE-2 which is the key result of the paper and also stays "within the method inherent reconstruction error". Is the change in reconstructed temperature between ORE-1 and ORE-2 really significant? 5-The reliability of the temperature reconstruction derived from the Origlio chironomid record is supported by the good concordance with d13C record from South-west of France (and Turkey). Nevertheless, a temperature reconstruction derived from chironomid record of a lake in the Western Pyrenees (Ech paleolake) has been recently published (Millet et al. 2012). This record covered the last ca 18 000 years. There is no trace of such an early warning in this temperature reconstruction. This is in apparent discrepancy with the speleothem record from south-west France (Genty et al 2006). Genty et al. favour the biogenic control for the observed calcite d13C variations (changes in the soil (microbial activity) and vegetation (plant root respiration) above the cave). The d13C variations are indirectly related to changes in climate. Other climate parameters than summer temperature may be involved in the d13C variation in speleothem (saisonnality, moisture . . .).

Minor comments: p1620: It would be useful to have more basic information about the sediment sampling: what was the thickness of the samples? Were they taken contiguously? p1622: about chronology: It should be better to estimate the average 95% confidence interval of the calibrated ages taking into account the density of radiocar-

bon dates and the distribution of dated levels (e.g. by using CLAM). p1626, line 8: I suppose the right sentence is “In many of the YD (ORE-4) and early Holocene (ORE-5) samples.

Interactive comment on Clim. Past Discuss., 8, 1615, 2012.