

Interactive comment on “The 4.2 ka BP event in the vegetation record of the central Mediterranean” by Federico Di Rita and Donatella Magri

Federico Di Rita and Donatella Magri

federico.dirita@uniroma1.it

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Reply to reviewer #2:

Dear Anna Maria,

Many thanks for your comments that certainly improve our text and, in some cases, provide us the occasion for a scientific debate.

OVERALL COMMENT : This is a very interesting and well written paper pointing to a key event that has many intriguing ambiguities in the Mediterranean, made of local contexts with plant/niche diversity and repeated impressive development of (western) civilizations. The synthesis is based on palynological data obtained from 36 well-dated off-site records depicting slight to strong changes in Arboreal Pollen percentages. The

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AP curves are taken as exemplar cases showing tree/shrub reactions to climate and human-driven forces. The distinction of four main groups/regions matching a good correspondence between the AP/forest response and the latitude can be shared as useful to start with a classification of 4.2 event responses in the Mediterranean. The idea of latitude shifts is in the daily experience of who works in coeval sites from northern Africa to S up to N Italy, and deserves to be further developed. The theme of this paper is of stringent actuality and very interesting for Climate of the Past. The sites cover a large range of regions from the Apennine to the coasts, from Italian peninsula to islands to North Africa, an appreciable consideration of the southern part of the Mediterranean basin. Moreover, it is laudable the effort made by the authors to sort as more as possible the ‘climate side of the moon’ because the debate risks sometimes to become too much ‘human-centred’, especially now that archaeology (including demography) has fully entered this debate as an unavoidable element of the discussion. However, we must admit that middle-late Holocene rise and fall of trees cannot be objectively considered an effect of climate change alone: not in the Mediterranean, not at the beginning of one of the most explosive cultural movement as the Bronze Age has been. This is the first culture dramatically impacting on woods because the wood (where woods were naturally present) was used to build large villages and huts besides palisades, fuel and metallurgy, possibly made ships etc. The idea of a true ‘wood management’ and ‘exploitation adapted to environmental features’ during the Bronze Age emerges from recent literature (e.g. Mercuri et al. 2015; Fall et al. 2015; Seijo and Rellà 2017).

REPLY: thank you for your appreciation of our work and also for the general framing of a scientific problem that is very complex and far from being well studied.

COMMENT: My major remark is that some weaknesses in the conceptual method proposed in this paper should be addressed (in the Introduction/Aim and Conclusions?) before it is definitively accepted. Despite the complex and composite situation of the Mediterranean landscape is clear from interdisciplinary research, the paper wants to be ‘climate-centred’ and (only?) in some points moves towards a ‘cultural explanation’.

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If I have well understood, and if the authors want to convince better who is especially 'human impact-oriented', I invite them to clearly declare this vision with something like 'we will discuss only/mainly of climate to check for the non-anthropic' signals' (possibly at the opposite of Mercuri 2014 having a title similarly centred on the central Mediterranean).

REPLY: Our intention is not to provide a climate-centered explanation of the vegetation change during the 4.2 ka BP event, but to detect possible geographical patterns in the vegetation changes of the central Mediterranean during the 4.2 ka BP event. As clearly stated in the introduction, our intention is to "highlight possible geographical patterns that may reflect the responses of vegetation to changes in precipitation and water availability". In the discussion we observe that "considering the geographical location of the sites and their water availability, it is possible to distinguish four main situations" and in the conclusion we argue that "the examination of pollen records from the central Mediterranean regions in the time interval around 4.2 ka BP has allowed the recognition of vegetation dynamics reflecting a geographical pattern, related to latitude and precipitation regime". It is not our intention to convince 'human impact-oriented' scientists, but only to show that despite an undeniable and strong human impact on vegetation, a geographical pattern in the forest decline process is clearly visible and is related to the precipitation gradient still existing in the central Mediterranean.

COMMENT: The 4.2 is a climate event, palynology can show its effects on vegetation. The possibility we have to see it in pollen diagrams is a matter of resilience of ecosystems rather than other – I believe your paper can support this ecologically-driven dynamics (and you mention this, not explicitly, in your conclusions, when you mention that there are "changes in vegetation composition and not in forest cover". Plants and humans, vegetation types (adapted to elevation and latitude) and cultural knowledge (depending on the way landscape is exploited) are part of these ecosystems. This is true both considering the central Mediterranean areas and the north Africa/Saharan areas (Mercuri et al. 2011), and I believe the same we can be inferred from your

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interesting study.

REPLY: we definitely agree.

COMMENT: In the method section, some specific palynological issues should be reported: a) As pollen is the tool used in this review, it should be important to take into consideration that pollen image of human actions can be sensibly different under different cultures: "the Neolithic spread of agricultural systems would have only caused pollen signals producing fairly local effects (cereals) whilst the deforestation events of the subsequent cultural phases (often involving oaks) had regional echoes in pollen rain" (Mercuri 2014, p. 1806).

REPLY: Changed as suggested.

COMMENT: b) Based on my personal experience, pollen rain circulation can mask some altitudinal displacements of vegetation in mountains, and some changes only reflected as shift of the same species in the same area can result in a low signal/absence of oscillations in pollen curves. You have many mountain sites with airborne transport, and many coastal sites with significant water/river transport.

REPLY: Discussed as suggested.

COMMENT: c) Anthropogenic indicators: Which types? Which role? For example, in some records like Alimini Piccolo, the authors (you) interpret Cichorioideae as over-represented under a selective preservation (therefore, I believe that they are fenestrate pollen = Cichorieae: Florenzano et al. 2015). If you ignore the role of Cichorieae in marking the expansion of pasturelands you loss one of the main effects of pastoralism in central Mediterranean. However, the agro-pastoral economy/model of exploitation is mentioned since the Introduction of the text (p.2, line 23) suggesting you are aware of its role. In the abstract and elsewhere in the text, you use the 'Anthropogenic Pollen Indicators' as markers of 'influence of human activity on past vegetation' (line 10 of the Abstract): one can say that humans influence also a number of wild species and their

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impact is more pervasive than the cultivated/synanthropic evidence can show, however we assume that this is an useful methodological approach of palynology, and therefore what is included in this marker category is of key importance (for a discussion see also Mercuri et al. 2013: some anthropogenic indicators *sensu* Behre may be not so common or important in Mediterranean contexts; the idea of that paper was to check how are the true pollen grains we can recover from sites where humans were surely present).

REPLY: We agree that the type and role of anthropological indicators may be quite different in different environmental contexts. However, any interpretation that we might add in our synthesis paper could arouse criticism. In order to present an independent view of the pollen anthropological indicators, in the results we have carefully reported the interpretation provided in the original publication of each considered pollen record. This methodological approach avoids the introduction of systematic errors due to our personal interpretation of palynological data. We have specified this strategy in the methodological section.

Specific comments:

COMMENT: Line 24: “decline of Arboreal Pollen (AP) percentages, interpreted as an opening of the forest” = in this way the sentence is redundant: the decline of AP – if there are trees – is the reduction of number of trees determining the opening of the forest. Opening of the forest may be climatic or anthropogenic (and this may be matter of interpretation).

REPLY: The decline of Arboreal Pollen percentages may be also due to an increase of local herbaceous vegetation, for example an increase of reeds or sedges around the lake, that may affect the percentages of trees without corresponding to a loss of trees.

COMMENT: Pag. 2 Line 10: “In case of a climate shift towards aridity, the response of forest vegetation may be modest in areas with abundant water availability, while marked deforestation effects are expected in areas that suffer for water deficit” = are you think-

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ing to a natural loss of tree biomass or something different? The use of ‘deforestation’ suggests the cut of trees, which may be even independent by water availability. My advice is to check this term here and in the text, and prefer less ambiguous words when/if you want to remark a climate effect.

REPLY: We thank the reviewer for this comment: we have checked the meaning of “deforestation”, which is normally referred to cutting by humans rather than opening of the vegetation because of water deficit. We have changed the text accordingly.

COMMENT: Line 23: Demographic increase? Check Cardarelli 1997, and Palmisano et al. 2017

REPLY: Although the papers by Cardarelli (1997) and Palmisano et al. (2017) show fluctuating demographic trends in Italy, we think we can maintain “demographic increase” in its overall meaning.

COMMENT: Line 26: “documented distribution of archaeological sites”: here you have to take into consideration that the documented archaeological site is only a spot presence within an “area on influence” and activity of humans – since the Bronze age this is well attested (Cremaschi et al. 2016; Mercuri et al 2015): people lived in houses and cultivated in fields, out of the villages, but fields and pasturelands commonly give insignificant archaeological evidence.

REPLY: We agree with the reviewer, however it is important to infer human activity not only from pollen records, but also from the independent archaeological record. We have mitigated the “documented distribution of archaeological sites” in “archaeological record”.

COMMENT: Pag. 3 In the Method section, a comment should be added on how to compare pollen from many different study analyses (old and very new papers, different pollen sums, different elevations etc).

REPLY: As explained above, in order to avoid introducing our subjective interpreta-

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tion of the data, we have preferred to refer exactly to the independent interpretations presented in the original publications of the pollen records.

COMMENT: Line 10: how you obtained the data of cores that were studied from others than you? (this is only understandable from legend of fig. 2, isn't it?). It is not clear if 'Anthropogenic Indicators' refers also to coprophilous fungi (this is evident then, along the text). Have you used the (different) sums made by the different authors, or you refer to the occurrence of anthropogenic taxa without considering sums, or you made a new sum of anthropogenic taxa? Please, explain these points.

REPLY: Pollen records for which pollen counts are not available to us, have been digitized from the published pollen diagrams. As to 'Anthropogenic indicators' we used the interpretations presented in the original publications of the pollen records. Although these may not always be consistent, they are independent from our interpretation and provide a mediated view of the interpretation of the scientific community.

COMMENT: Line 25: I think the 'Middle' of 'MBA' can be written in capital letters, because it is well established as an archaeological phase beginning at 3600 cal BP (see the table in Mercuri et al 2015)

REPLY: Changed as suggested.

COMMENT: Pag. 6 = line 19, about the chronology of RF93-30, please consider that the first age depth model was in Oldfield's papers while the definitive chronology has been published in Mercuri et al. 2012, p. 355. Please note that in the more recent paper it is reported that "We have reconsidered the original data to develop a new age depth model that is slightly different to the previous one.", "The original chronology of the core was developed by using the secular variation record and two 14 C dates (which were newly calibrated) carried out on benthic and planktic foraminifera at 527 and 599 cm.", "only the most prominent and well dated features of the magnetic inclination were used for the new age-depth model". The age of the sediments at 527 cm (3970 – 147 years cal. B.P.) is well in agreement with the presence of the tephra at 529 cm but

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the model was NOT built using the Avellino tephra attributed date.

REPLY: Our text is consistent with the reviewer's comment.

COMMENT: Pag. 9, line 16: referring to Pergusa, you should add also Sadori et al. 2013.

REPLY: In the methods section we have written that we have selected only the records with at least two dates between 2000 and 6000 cal BP. The record from Pergusa by Sadori et al. (2013) has only one date. Instead, the record published by Sadori and Narcisi (2001) has three dates in the same time span.

COMMENT: Pag. 11, line 4 = with 'geographical location' are you meaning also 'elevation'?

REPLY: Yes, in a sense, as the elevation influences the mean annual precipitation.

COMMENT: Pag. 12 Line 14: "Similarly, the intensity of deforestation was generally stronger in the southern sites than in the sites located at latitudes >39°N". This assumption is not verifiable – if you use the word 'deforestation' you are thinking to a human action and the use of trees during the Bronze Age is clearly astonishingly high in North Italy – the sentence cannot be supported by archaeological evidence (based on palynology, you can compare Mercuri et al. 2006, 2015; Cremaschi et al. 2016).

REPLY: We have replaced "deforestation" with "forest decline"

COMMENT: Line 25: "a further factor that may have influenced the vegetation of the southern regions in the central Mediterranean is an extensive and intense human impact on the landscape, documented by archaeological sites and anthropogenic indicators in pollen records" This is absolutely true as a general statement BUT you cannot do such a distinction between north and south Italy without ignoring hundreds of local analyses and archaeobotanical research (for human impact inferred from archaeological sites/archaeological studies/archaeobotany see, for example, at the database BRAIN, www.).

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REPLY: The www of the BRAIN data base is unavailable in internet. We have added a reference (Mariotti Lippi et al., 2018), instead.

COMMENT: Line 27: “values for anthropogenic pollen indicators are higher in southern Italy and Sicily than elsewhere, indicating that human activity was particularly intense during these events of forest decline.” I invite to re-formulate because it depends on the anthropogenic pollen indicators that mirrors different land uses. The role of pastoralism and the pressure of pastoralism on different types of vegetation is determinant in these dynamics (e.g. Florenzano 2016; Mercuri et al. 2010).

REPLY: As explained above, in order to avoid introducing our subjective interpretation of the data, we have preferred to refer exactly to the independent interpretations presented in the original publications of the pollen records. The authors that have studied the sites in southern Italy have especially emphasized the role and amount of anthropogenic pollen indicators.

COMMENT: Pag. 13 Lines 14-17: these sentences about RF93-30 must be deleted (see my note above, and the detailed explanation published in Mercuri et al. 2012 p. 355). The acme of AP, greatly deciduous oaks, is dated to 3900 cal.BP and precedes the fall down to 3600 cal. BP, which corresponds to the onset of Terramare culture in the Po Plain, the main basin of sediment supply of this core (Piva et al. 2008). As explained in the discussion of the pollen diagram, and its comparison with the terrestrial trench from Terramara di Montale (Mercuri et al. 2012), the marine core RF93-30 collects pollen from eastern Italy, with a main sediment supply from the northern Po valley, and most airborne pollen from the southern regions. For this reason we can see the drop of trees of the Middle Bronze Age, and the development of Mediterranean macchia and spread of open-pasture environments largely characterizing the pastoralism effect in southern regions. The core reports on a very general situation, more than other terrestrial or coastal cores.

REPLY: Changed as suggested.

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COMMENT: Pag. 15, line 20 “In Sardinia and Corsica,; : :” ? this sentence is not clear: what means “the margins of an area affected by a climate change”? Or: why this area seems to be not affected?

REPLY: We have rephrased this sentence.

Federico and Donatella

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