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Climate of the Past Discussions

Interactive comment on "Climate change and the demise of Minoan civilization" *by* A. A. Tsonis et al.

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We do not agree that the manuscript has gaps that make it difficult to relate climate change and the demise of Minoan civilization as Dr. Anchukaitis claims. In fact, we think that many lines of evidence converge to make this hypothesis a rather strong one. Here are our replies to his comments:

1. a) Dr. Anchukaitis says that non-random spatial structures can emerge from a random time series evaluated against a climate field with spatial correlation. He sites the work of Livezey and Chen 1983 and Wang and Shen 1999 and he then states that " ...the appearance of a weak but non-random spatial correlation pattern cannot alone be evidence. . ." . First of all, our hypothesis is not based on Figure 2 alone but on several additional multidisciplinary lines of evidence. Second, in Livezey and Chen 1983 the

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climate field is the 700 mb field and in Wang and Shen 1999 the climate field is the temperature field. Both those fields have a much greater spatial correlation than the precipitation field, and they are multi-decadal in time. Our precipitation data used here is multiple centuries in time, reducing the level of significance for the correlation. In fact we have repeated this analysis by correlating a randomized version of the Mann et al. Nino3.4 index to the proxy precipitation field and we find that the coherency observed in the pattern in Fig. 2 is destroyed and that the correlations are smaller by a factor of about 2-3 (see attached Fig. A2; this figure will be included in the revised version). Further, the pattern is reproduced nearly exactly in climate model simulations. These two independent results suggest that it is extremely unlikely that this pattern is random.

b) Dr. Anchukaitis suggests that it would ideal to supplement our Figure 2 with a similar figure using instrumental data. This has been done in prior work by other authors, and we give the references for this work (Fraedrich and Muller 1992, Bronnimann 2007) on page 6. Initially we had re-produced their result in the attached Figure A1. However, we decided that the reference alone will be sufficient. While we have no problem including this figure in our revised version, it seems redundant, as interested readers can certainly pursue this through the literature.

c) Dr. Anchukaitis repeatedaly attempts to cast doubt that an El Nino signal over the Mediterranean exists. However, the model simulations (as well as other models in the CMIP3 archive) in our Figure 3 clearly shows that, on the average, El Nino brings drier conditions over the area of interest and La Nina brings wetter conditions. We have mentioned on page 10 that the North Atlantic Oscillation (NAO) in its positive phase may also bring dry conditions in the Mediterranean. This effect, however, cannot be assessed in the second millennium BC as no proxy NAO data extend that far. Nevertheless, we acknowledge that this possibility could have accentuated the dry conditions.

2. Dr. Anchukaitis doubts the change in dynamics in the ENSO proxy around 3000 BC. However, this proxy record has been analyzed rigorously using modern time series analyses by Moy et al (2002), Tsonis 2008, and Wang et al. 2009. All these studies are

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consistent with each other and point to a significant change in the dynamics of ENSO at around 3000 BC. This result is also consistent with geoarchaeological evidence form Peru, which also suggests absence of El Nino before 5000 BP (3000 BC) and its onset after that (Sandweis et al. 1996). We consider the consistency of all these results an indication that the Moy et al. proxy is reliable.

3. a) It is not professional to downplay the validity of the Magill et al record because it is an abstract in an AGU meeting rather than a publication in a referred journal. Accepted practice allows that conference abstracts to be cited as equal to referred papers. Magill's research group's interests in the cores they obtained from the Mediterranean are different and they may need more work before they submit them for publication in a referred journal. However, these scientists are very reputable and known in the field and we have no reason to disregard their results because they are included in a conference.

b) We apologize for not including the Moody 2005 reference. We will of course correct this. However, arguing that the absence of Tilia pollen during the time of the demise of the Minoans might not support the existence of localized drought is quite simply a hand waving alternative which is unsupported by evidence, and moreover, allows for multiple different interpretations (e.g., the absence of pollen is due to human influence on the landscape).

4) Yes 'coral data' refer to Moy et al. 2002 record. We apologize for the confusion, and clarify it in our revised version figure 5. The idea behind Figure 5 is that if the correlation pattern in Figure 2 is based on one proxy data set (Mann et al. 2000) and the onset of frequent and strong El Nino events at the time of the demise is based on another proxy record (Moy et al. 2002), then in order to have a closure those two proxies should be consistent where they overlap.

Incidentally, the frequency of events for the Mann et al. 2000 record is estimated by the number of El Nino events in a 50-year window (hence the labeling of the x-axis) where an El Nino event is defined when Mann's El Nino index is greater than 1 standard

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deviation. The Moy et al. 2002 record is a direct measure of El Nino frequency. This has been clarified in the revised version.

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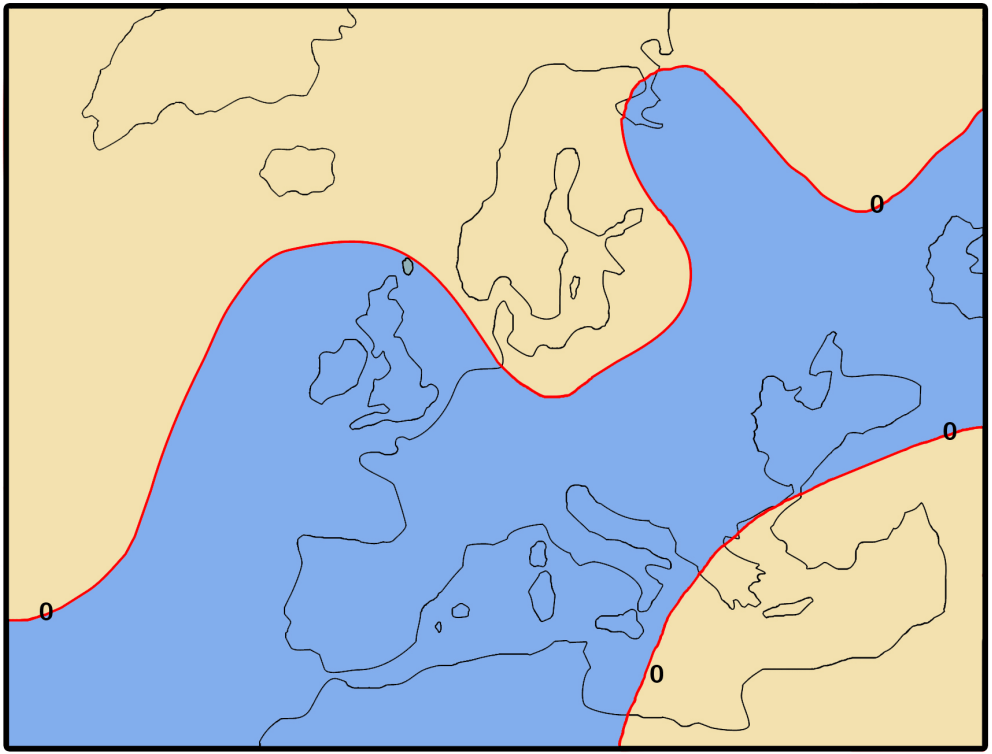

Fig. 1. Figure A1. Composite map of winter precipitation during 26 El Nino events from 1880 to 1988. Blue indicates above normal precipitation and light brown below normal precipitation (adopted from Fraedric

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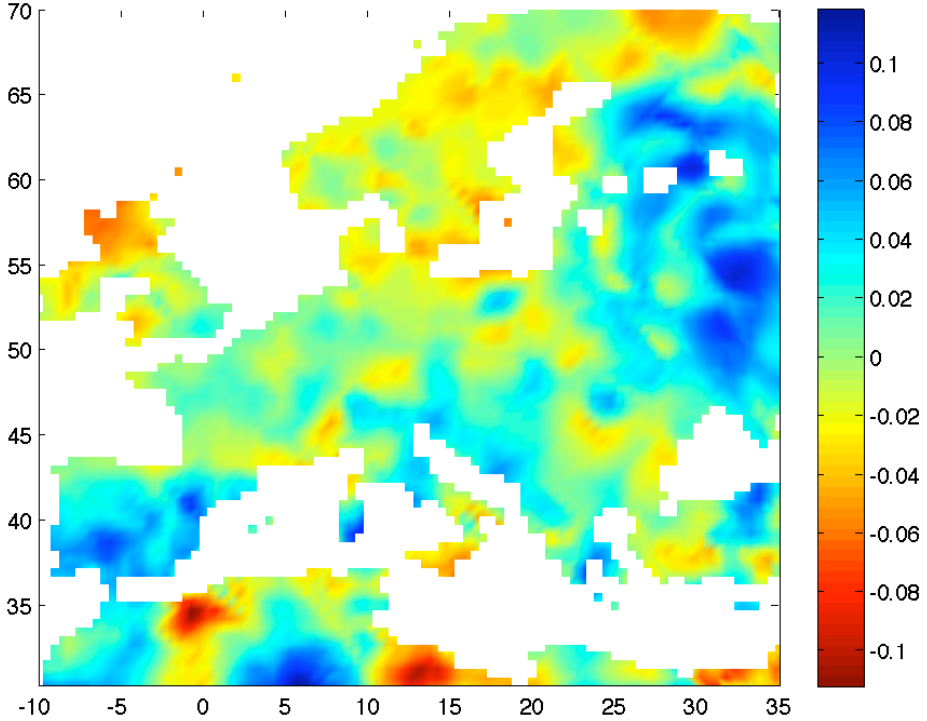

Fig. 2. Fig. A2. Same as Fig. 2 in our manuscript but now we correlate a randomized version of the Mann et al. 2000 Nino3.4 index to the proxy precipitation field. Clearly the coherency of the pattern observe