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Climate of the Past Discussions

Interactive comment on "Stable isotopic evidence of El Niño-like atmospheric circulation in the Pliocene Western United States" *by* M. J. Winnick et al.

Anonymous Referee #2

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General comments:

I have reservations about several aspects of this study, particularly in regard to the underlying assumptions that: 1) the topography across the region is largely unchanged since the Miocene, and 2) the modern patterns of seasonality can be used as an interpretive basis for the Pliocene data. Also, the western United States is a large and physiographically complex region, and the number of modern sample locations and the number of Pliocene localities are very small relative to the degree of detail that the authors aspire to in the reconstruction of the features of the past climatic circulation.

Specific comments:

page 2, line 23 through 28: "It has also been suggested that these wet conditions may have been the result of lower topography in the North American Cordillera (Bonham et al., 2009). The assumption of lower topography however, is not supported by paleoaltimetry studies of the US, which show that large- scale topography reached modern elevations by the early Miocene (e.g.: Mulch et al., 2006; Mix et al., 2011; Chamberlain et al., 2012). This is followed in lines 28 and 29 by "(see Supplementary Information for full discussion)". Perhaps I'm missing something, but I did not find a discussion of this in the supplementary materials.

After reviewing the papers cited in support of the idea that the topography is largely unchanged since the Miocene, I did not find it as clear cut as stated here. In particular, the uplift of the southern Sierra Nevada and Transverse Ranges in California may have occurred during the Pliocene and Pleistocene, and if so, would likely have had a large effect on the flow of moisture from the Pacific Ocean into the interior western United States. In this regard, here is the discussion from Chamberlain et al., (2012, p. 239): "There are two tantalizing sets of data that suggest that surface elevation may have increased in the southern Sierra Nevada in the last two million years. One study of hydrogen isotopes of fluid inclusions from a calcite vein in the Death Valley region show a decrease of 40 per mil in the past 2 Ma. This decrease was attributed to about a 1 km rise of the Sierra Nevada (Winograd and others, 1985). The other study of hydrogen isotopes in volcanic glass (Mulch and others, 2008) contains a few hydrogen isotope data from hydrated volcanic glass collected immediately east of the crest of the southern Sierra Nevada for the time period of 4.83 to 2.09 Ma. These isotopic data have high δD values, in contrast to those typically found in the area of the strongest rain shadow, and permit the absence of a rain shadow at that time interval. Therefore it is not excluded that the southern Sierra Nevada experienced a period of Plio-/Pleistocene surface uplift."

page 3, lines 6 through 9: The authors state: "Unlike floral-, faunal-, and sediment-based reconstructions that record only local environmental conditions, isotopes in

precipitation recorded in authigenic minerals are controlled by a combination of local and upstream conditions and therefore offer unique insights into synoptic-scale atmospheric circulation." I don't agree with the sentiment expressed here, because: 1) having a mixture of local and upstream conditions may result in the researcher having no effective knowledge of the areas contributing to the mixture, the proportional contributions of the different areas, or whether these contributions are stationary through time; and, 2) there is no exposition or discussion of what "unique insights into synoptic-scale circulation" result from these mixtures of materials.

Page 3, lines 28 to 29: in my opinion, five sites cannot provide an adequate basis for reconstructing regional patterns over an area as large and physiographically complex as the western United States.

Page 4, line 2: "regional temperature evolution over this time period is poorly constrained". I agree with this statement, and in light of this, it seems unwise to assume (as the authors do later in the manuscript) that the modern patterns of seasonality were present during the Pliocene.

Page 4, lines 9 through 20: were the data from La Niña years excluded from the analyses? Is so, how do the authors know that patterns associated with La Niña might not be relevant to their examination of Pliocene conditions? Are they starting with the assumption that only El Niño is relevant?

Also in regard to this paragraph: the authors do not include any discussion of the potential effects of elevational differences among the modern sites on the isotopic data. Again, this is a region of high relief (often over short distances), and can it be assumed that the physiographic context of the sites is unimportant?

And, the kriging interpolation can produce complex (and probably misleading) patterns that are based on a relatively small number of sites (for example, in Figure 3, the JFM map shows a broad band of dark blue across northern California and northern Nevada. There are no sites in this band, and the nearest sites to the north and south

of the band are in the light blue color, so the dark blue band must be an artifact of the kriging routine.).

Pages 5 and 6: It seems unwise to assume that all sites experienced the same direction and degree of change in precipitation through the Pliocene. Evidence from the instrumental record and back through the Holocene into the Late Pleistocene demonstrates a high degree of spatial heterogeneity of local and regional responses to large-scale climatic changes in the western United States. My belief is that would be a likely feature of the Pliocene climate as well.

Page 5, lines 20 through 26: the modern data set covers only six years (although the authors' simulations for a 53 year period "largely match" the features in the data set). This short time span, coupled with the relatively sparse spatial coverage of modern data points, leads me to wonder whether the modern patterns are as robust as the authors believe.

Pages 7 and 8 (Section 4.1 Seasonality of carbonate formation). This is the section that I have the most trouble with âĂŤ what is the rationale, and where are the supporting data for, the assumption that modern seasonality patterns are applicable to the Pliocene? If this assumption is invalid, then the interpretations of the isotopic data will almost certainly be invalid as well.

Pages 8 and 9: It is difficult for me to believe that the small number of paleo sites presented here is adequate to describe the upstream (in atmospheric terms) positions of the major features of atmospheric and oceanic circulation discussed here. A broad-scale temporal pattern of wetter Early Pliocene, drier Late Pliocene, and wetter Early Pleistocene from a small number of terrestrial sites is not sufficient evidence for the reconstruction of the position of features such as the subtropical jet or the EEP Cold Tongue.

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