

## ***Interactive comment on “Oxygen isotope composition of waters recorded in carbonates in strong clumped and oxygen isotopic disequilibrium” by Caroline Thaler et al.***

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This paper by Thaler et al. is very well written, and I think would be of interest to a large number of people in the scientific community. The finding that disequilibrium precipitation can potentially be used to track ‘true’ equilibrium is novel, and potentially very exciting. I find the paper well written, and I have few comments or questions that the authors do not answer. My only general comment is that I caution the authors to not declare one set of  $\delta^{18}\text{O}$  calibrations/measurement to be representative of equilibrium versus another as of this point. I (and probably many others in the community) am VERY sceptical of assertions that a single datapoint represents isotopic equilibrium for

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d18O and that all others do not. Overall, this is a good paper and I'm interested to see where this goes next.

Abstract: Good

Introduction: Good

Section 2.1: I'd like to see some statement about the mineralogy of the carbonates. It looks like that is reported to some extent in the supplement, but I think a sentence on mineralogy of your precipitates should go in the methods.

Section 3.1

Lines 171 and 172: Maybe mention in the text the equilibrium value and measured value, so that readers do not think -0.27 per mill is the measured value?

Section 3.2

Line 197: Do you know the isotopic composition of the urea? This should be easily testable. I realize it doesn't impact the results of this study, but would be nice to know!

Section 3.3

Line 239: "using the calibration..."

Section 3.4

Lines 278-280: This is true, with the important exception of carbonates in which d13C is also out of equilibrium, which is discussed later in section 3.4.

Line 297: "be explained solely by temperature..."

Line 340: "form in caves from CO2..."

Section 3.6: I'm not sure you can really make any statements here about better matching the Coplen (2007) and Watkins et al. (2013) data at low temperature, as figure 5 does not show error bars. When the errors are plotted (particularly important for the D47-based temperatures), it looks to me like your data overlaps both d18O calibration

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relationships.

Figure comments:

Figure 1: I'm confused by the x-axis. Why is CaCO<sub>3</sub> being presented in units of millimolar, which is appropriate for a dissolved solution? A solid should be presented in units of mass, i.e. milligrams, or alternatively as moles precipitated.

Figure 2: Same comment as per figure 1.

Figure 5: The trendline symbols for Watkins (2013) and Kim and O'Neil (1997) are too similar, and difficult to distinguish. Also I'm a bit confused by the Watkins data, why are the symbols for Watkins on the same line as Kim and O'Neil yet the trendline lies above? Additionally, if it doesn't clutter up the plot too much it would be good to see the error bars on the carbonate datapoints.

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