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Interactive comment on "New analytical and data evaluation protocols to improve the reliability of U-Pb LA-ICP-MS carbonate dating" *by* Marcel Guillong et al.

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This is a well written, well organized paper with two main points: 1) it describes the down-hole fractionation vs drill rate of various carbonates (primarily calcite), and 2) introduces a new calcite standard: JT. It is relative to a broad community and this is the appropriate platform for publication. I have only a few comments, one which I think is an important point that should be sure to be understood by the calcite geochronologist community. With a few changes, this manuscript should be ready for publication. Comments: 72-73. This is my major comment on the manuscript, and it's possible that I misunderstand the text. It is not entirely clear to me what the authors mean by, "integra-

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tion intervals of both reference materials and samples are subsequently adjusted. . ." but nevertheless it is critical to point out that because the samples are not corrected for down-hole fractionation, one cannot adjust the integration interval of the samples or unknowns unless they adjust it to use exactly the same interval. Because the premise of this paper is based on the difference in fractionation vs drill rate, it is critical to let the reader understand that when reducing the data using a glass standard, and using a secondary correction on the entire 206Pb/238U ratio, it must be assumed that the same part of the down-hole fractionated analysis is used for both RM and unknowns. One can imagine a scenario in which the user selects the first half of the analysis for all reference materials, and the second half of the analysis for all the unknowns. All the raw dates for the unknown will be younger than the average, because the 206Pb/238U date gets older down-hole (in most cases - and this is demonstrated rather well in this paper). All the analyses for the unknowns will be older than the average, and thus the unknowns will be inaccurate by half of the down-hole fractionation percentage from the top to the bottom of the hole. Not good. I'm not entirely sure if this is what was done by the authors or not; their RMs yield accurate ages, possibly because I misunderstand this line, or because the adjustments were random enough not to make enough of a difference in the final age. Nevertheless, if this was done the data should be reprocessed before publication, even though the ages will change imperceptibly (almost surely the case). 74. "Due to the low 207 count rate" This is a smart way to reduce the data and a key point in reducing geochronologic data with low count rates. When the data is particularly noisy, the mean of the ratios yields inaccurate data. This is especially true with data with a noisy background, as sometimes raw ratios can be negative, which is physically impossible. Figure 1. The scaling is oh so close, but if you can scale the two figures so that one can make a direct comparison between the isochrons of each method, that would be nice. 119. sessions respectively 139. "repetition rate from 110 μm and 5 Hz for the primary RM WC-1 up to 250 μm and 5-10 Hz for validation RM and samples" Please rewrite this for clarity. Did vary the rep rate and spot size differently for the primary and the unknowns. Just state that variance for each. 143. I

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don't know that I'd ever recommend to someone that they run different spot sizes on standards vs. unknowns. Is there a reference here? The demonstration done here is an excellent example of the differences between overall fractionation with different pit depths, but I don't think anyone should recommend that different spot sizes or ablation rates be used between samples and unknowns. 151. But you must not have done the same range in spot sizes and pit depths for the unknowns based on the graph. Please describe a little more clearly the experimental setup. Figure 3 is fine, but Figure 4 should be normalized to the age; i.e., is should be showing the percentage of age offset vs. aspect ratio. Personally, I think it should also go through the origin, that is, one would expect no down-hole fractionation with an infinite width-depth, and the age offset increases from there. This is a more intuitive way of thinking about the process which is being demonstrated. That aside, the a), b) and c) figures are not comparable (slopes), because they are measured vs absolute offset instead of relative offset. Note that the slopes decrease with decreasing age. With relative offsets, one would then be able to note that the slopes are not the same, though they are probably equivalent within uncertainty; it's just that the uncertainty of the latter two datasets in very large. It would be proper to state the slope and its uncertainty and better yet, also add a error envelope on the figure. One can argue that you could draw a horizontal line between the bottom two datasets (though I agree that there must be a slope - it just doesn't match the pit depths in figure 5; both slopes, taken without uncertainties, imply that the pits are deeper in both ASH-15 and JT than they are in WC). 190. I do not endorse this methodology, primarily because of the statement on line 197. Because the ICP parameters and laser parameters can drift, it makes much more sense to use the same spot size for the standards and unknowns. If the unknowns are so low in U such that they need an enormous spot, we should be finding appropriate RMs to deal with that instead of varying the spot size and rep rate. Too many things can go wrong. How many samples are out there that don't work with the currently available RMs? If the authors really want to suggest this methodology, I suggest that they explore the limits of what is possible with RM U, Pb concentrations versus that of unknowns, to show that

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there are some samples that cannot be measured with the same laser parameters.

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