The work by Frieling and colleagues is strong framework and a much-needed study that will open a new opportunity for applications of organic microfossil 13C analysis. Like single species foram analyses (the benchmark for modern carbon and oxygen isotope studies) single or several organic microfossil 13C limits the breadth of sources to sedimentary organic matter and limits the degrees of freedom in a highly advantageous way. This study is the gateway to the deeper geologic record that will allow broad application of the dinocyst proxy to ancient carbon cycle studies. The questions below are meant to enhance the discussion, but the work, as it is, stands on its own as it is presented.

From a methodological perspective I appreciate the details provided here. Controlling for size and process length is great approach but do you see relationships between $d^{13}$C_{cyst} and cyst size?

To what degree do you feel that the time averaging affected your data? Do you have access to any 14C dates of the surface sediments? From here you could potentially model the expected range of 13C values of DIC accounting for Suess Effect. More details in the manuscript on your rough correction would be helpful.

What do you think is the background blank source? Is it from atmospheric aerosols that adhere to all surfaces regardless of precautions or is it from within the nickel plate? (Does the nickel plate show scoring from the laser?). Regardless, the approach to signal size to noise, considerations of the blank and other corrections seem reasonable. These consideration are important not only for your study and approach but for the future potential of this kind of analysis for sample return from Mars and elsewhere.
From this discussion I think I favor your argument that intercyst variability reflects individual differences. One can envision that individual cells or cysts have significantly different $^{13}$C values owing to the randomness of cellular growth, changes in microenvironments of growth that also affect DIC and CO2 $^{13}$C. Add in the time averaging from core top sample collection it is not a surprise that you see large variance. In fact, I would be worried if you did not. Your suggestion of controlling for size, as much as one is able, is a good idea.

For standards have you considered dissolving a standard material like caffeine in water and allowing it to dry onto a surface and analyzing that (you could spray it or something). At the very least here you could assume that the starting composition is isotopically uniform. I supposed $^{13}$C differences could arise from the drying process, but it may be better than PEF.

Have you investigated the compositional differences between cyst and motile cells? I am familiar with the references you report on this issue but what specifically are the differences? What proportion of the carbon from the cell transferred to the cyst? Is this known?