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Interactive comment on "The long-solved problem of the best-fit straight line: Application to isotopic mixing lines" *by* Richard Wehr and Scott R . Saleska

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Review of Wehr and Saleska, BGD, 2016, "The long-solved problem. . ."

Congratulations on writing a terrific paper: well researched, well written and very much needed. I have just a few questions and comments.

General comment:

How can we, or should we, consider variability in CO2 and δ13C arising not from instrumental noise but from the environment? As pointed out in Miller and Tans, in some real-world situations, assignment of analytical uncertainties to CO2 and δ13C may result in poor goodness of fit, i.e. a large value of reduced-chi square, suggesting that analytical

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CO2 and δ13C uncertainties are too small. This is of course important because small CO2 and δ13C uncertainties will lead to too small slope and intercept uncertainties. Note that while not so common now, as analytical precision improves, instances where natural variability significantly exceeds instrumental precision will need to be dealt with more. In MT2003, we attempted to deal with this by starting with an initial estimate of the best fit line, although we used a GMR instead of fitexy (for speed, and because we only knew the analytical uncertainties). We then proceeded to scale the standard deviations of the x and y residuals to produce a reduced chi-square value of 1; finally fitexy was used to calculate the slope and intercept uncertainties. Nonetheless, a problem persists, which is that the slope of the best fit line depends on the initially assigned x and y uncertainties. I'm very interested to hear your ideas of how to address this. (Maybe I'm missing something obvious, like using an OLR regression as a starting point.)

Specific comments: P4 l8. Note for future reference that the Keeling plot equation is valid not just for a single source (or sink), but delta_s can be interpreted as a the flux-weighted source (sink) signature. Eq. 3. The derivation of this was not obvious. It's not critical to the argument, but since you have an appendix, can you add this? P7 l5. I am surprised by (and skeptical of) an instrument with 0.01 ppm and 0.01 per mil uncertainty. Can you provide a reference in the literature for this, especially since this is characterized as 'common instrumentation'? P7l8. Change 'latter' to 'last'. P7l34 and Table 2. I'm confused as to why CO2 ranges from 100 to 5000 are relevant and why CO2 uncertainties greater than 1 are relevant. I understand that soil chambers could give such high CO2 enhancements, but as seen from the table, uncertainties become very small. Perhaps you could add a column of 100 ppm in Table 1 and then summarize the rest of the Table2 results in the text. P8. L1. Why are the MT results a bit better at these high values? Or maybe better to say, why are the KP biases occasionally significant? P8l29. Factor of 2 seems a bit too generous. The biggest offset from Monte Carlo I see is 0.67. P8l35. What are the 'adjusted data points'? P9.l7 Isn't G simply reduced chi-square? If so, why introduce a new term for this?

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