

Interactive comment on “The covariation of Northern Hemisphere summertime CO₂ with surface temperature at boreal latitudes” by D. Wunch et al.

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This paper uses TCCON and GOSAT data to infer a summary statistic for the depth of the summer drawdown in northern hemisphere column-integrated CO₂. It investigates a number of explanatory variables for the interannual variability in this drawdown. It first calculates a relationship between the drawdown and respiration-weighted anomalies in northern hemisphere surface temperature

They find that interannual variability in atmospheric dynamics, with its concomitant changes in surface temperature, is responsible for about half the variability in drawdown

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with the rest coming from changes in net ecosystem exchange. They find the impact of fires and fossil fuel contributions to CO₂ drawdown to be small.

the paper is well-written, focused and generally well-argued. There is one point about the results which perplexes me and one suggestion for perhaps strengthening the results. I am surprised by the weak response of the drawdown to the interannual variability of transport acting on fossil fuel sources, the dynamical effect the authors mention during the description of the fossil results. I suppose my mental picture of the large-scale action of transport on column-averaged CO₂ would be some kind of one-dimensional advection-diffusion where changes in the meridional flow would act on the large-scale north-south gradient to "pile up" or deplete CO₂ at a given latitude. If that picture made any sense then the dynamical effect on fossil fuel CO₂, with its permanent (though changing) north-south gradient might be expected to oppose that of the total CO₂ which can have a reversed gradient at this time of year. Perhaps the authors might expand a little on the analysis of the fossil fuel impact.

My other suggestion is actually hinted at by the authors themselves which makes me think they considered then rejected it. Keppel-Aleks et al., 2012 provide a recipe for removing local dynamical noise from the pointwise TCCON data by looking at air mass tracers such as potential temperature. First, what would happen if the authors treated the data and models here the same way? I see one obvious objection, this is aliasing some of the dynamical signal they want to look at but I suspect, provided the averaging was kept fairly local, it might improve signal-noise. It might also be worth looking at the dynamics fields to see whether, indeed, the dynamical response is cleanly explained by this air-mass mechanism. This is genuinely a suggestion; if there's an obvious reason not to do it I recommend the authors deal with it in their response rather than the paper.

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