Biogeosciences Discuss.,
doi:10.5194/bg-2016-223-RC1, 2016

Interactive comment on "Feasibility for detection of ecosystem response to disturbance by atmospheric carbon dioxide" *by* Bjorn-Gustaf J. Brooks et al.

P. Rayner (Referee)

prayner@unimelb.edu.au

Received and published: 4 August 2016

this paper examines the feasibility of detecting signals in the atmosphere caused by changes in CO2 flux. the signals are of atmospheric concentration and the changes in flux notional versions of those arising from disturbance of the ecosystem. The problem is methodologically important since atmospheric measurement is a widely proposed technique for detecting such disturbances. The question is also in scope for the journal. the paper is well written and its methods clearly presented.

I do not, however, feel that the paper makes a significant contribution to the question it addresses. In my view it frames the question wrongly and hence produces results that won't apply in a real case. I do not believe that the results it does produce will give much guidance to the correct answers.

this is a harsh assessment so I will try to describe my objections in some detail. the paper calculates what atmospheric signal can be detected for a given disturbance flux. This is not a question we care about directly. we want to know instead what size of disturbance flux can be detected using atmospheric measurements. It is hard to write these two questions in a way that exposes how large the gap between them is. The paper crosses the gap by ratioing flux and response then multiplying by some detection limit. In the real case the gap between the two questions is occupied by the whole machinery of atmospheric inversions with their many techniques.

So do we need this machinery to answer the question of the paper? We do for three reasons each of which make this paper's conclusions impossible to apply.

the first reason is stated by the paper itself: "In our simulated experiments the detection thresholds do not consider confounding effects from biospheric fluxes" (p9). But these confounding fluxes dominate the signals observed at any station. If we regard any contribution to the observation beyond the signal we seek as noise then we can roughly decompose this noise into three parts. the first is instrumental noise, the second signals from uncertainty in transport and the third contributions from other fluxes. For an active biospheric region the third of these is almost certainly dominant. given this, the problem of locating and quantifying a flux is far harder in the real world than the size of the signal would suggest and some measure of a detection limit does not represent the difficulty.

fortunately the problem of detection is also, in another sense, easier than the paper suggests. the atmospheric network of concentration measurements, coupled to atmospheric transport models, is an instrument for detecting fluxes. It's not correct to characterize this instrument with a detection limit like the threshold of some digital detector. Heuristically it detects flux signals by looking for expected patterns arising from their projection onto atmospheric concentrations. Provided the structure of these patterns

Printer-friendly version

Discussion paper

is faithfully represented by a model we might well detect them at quite small amplitude. the most dramatic example I can think of for this is ocean acoustic tomography. Here we manufacture the pattern (encode the signal) and can thus extract signals orders of magnitude smaller than the acoustic noise in the ocean. I'm less certain but I suspect the detection of gravity waves using interferometry extends the same approach to nearly unimaginable levels. Our more prosaic problem relies heavily on the same property. I suspect many of the atmospheric signals interpreted by atmospheric inversions over the years were, considered alone, smaller than the instrumental and model noise. But it's the pattern of these signals which all of the inversion methods automatically extract. Exposing these patterns in the observations is the underlying reason for the shift from analysing monthly mean concentrations to higher-resolution time series. the methodology for establishing what can or cannot be detected by a concentration network is well-established. It would answer both this and my previous point.

my final point concerns the comparisons the authors did make. they compare atmospheric signals arising from disturbances of different size (spatial extent). I don't think this is a valid comparison physically or a particularly interesting one methodologically. Disturbances are heterogeneous events. By their nature their signals (in flux density units) often become weaker as we average to larger areas since we include more undisturbed regions. Also, the tests that were carried out leave an important question unanswered. As we increase the area of the disturbance (with constant flux density) we both make it more visible (since we increase the angle it subtends for winds reaching any detector) but also add more CO_2 to the atmosphere. How much do each of these effects contribute to the detectability? I think the formal methods I advocate above would be required to test this effect since a broad-scale but weak disturbance is more likely to fail the detection limit test used in this paper while its broad but subtle shifts might well be detectable in an inversion.

As a reviewer I realise it is part of my task to suggest improvements to the paper that make it more valuable, preferably without the authors having to do everything from scratch. Here this is difficult because my objections are pretty fundamental. If the authors want to persist with this approach, I suggest they at least consider the impact of other fluxes on the observations. this can be done by inserting some other fluxes separately into the transport model then calculating the concentration "noise" generated by uncertainty on these fluxes. there is enough statistical expertise among the authors that I can leave the mathematics to them.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-223, 2016.