

Interactive comment on “Impact of Spaceborne Carbon Monoxide Observations from the S-5P platform on Tropospheric Composition Analyses and Forecasts” by R. Abida et al.

Anonymous Referee #2

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This manuscript describes an Observing System Simulation Experiment to assess the impact of Sentinel 5p CO observations on CO concentrations over Europe. Their guiding principle—a laudable one—is avoid the common critique that OSSE results are overly optimistic. They have partially succeeded in that approach. They have gone through the effort of choosing two different nested chemical transport models, one for their nature run (NR) and the other for their assimilation (AR) and control run (CR). They also have a reasonable retrieval model simulator to construct observational operators for S5p. They sample the NR using an S5P simulator and then assimilate those pseudo retrievals into their AR and CR. They have been careful to compare their NR to independent observations to assess its realism. However, this is where I have some

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concerns. While they establish a bias and variance between the NR and independent observations, their fundamental threshold is that the difference appears "reasonable". What they have not done is to relate the errors between the NR and independent observations to the interpretation of the performance of the AR. So, if the accuracy and precision of the NR is twice as bad, what should one infer about the performance of the AR? I would argue that a more important implementation of their guiding principle is to assimilate real observations, e.g., MOPITT and/or SCIAMACHY, into their system and compare the analysis fields to independent observations. Then, they could do an OSSE for the same observing system and assess the statistical difference between the AR and NR sampled at independent observations versus AR(real) against independent observations. That would provide a better sense of what the OSSE limitations actually are. As it stands, I'm still suspicious of the overall performance. Furthermore, we don't know how well S5p will perform given other sensors, e.g., MOPITT, CrIS, are already taking CO data with comparable performance.

Otherwise, the overall work is reasonable and the authors have performed some nice statistical analysis of the results. Of course, in principle, this OSSE should have been performed *before* S5p was funded to assess its potential. But, practice is still catching up with theory. I've attached comments of the manuscript in the accompanying pdf.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/acp-2015-924/acp-2015-924-RC2-supplement.pdf>

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