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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Adjoint inverse modeling of a CO emission inventory at the city scale: Santiago de Chile's case" *by* P. Saide et al.

Anonymous Referee #1

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Review of: Adjoint inverse modeling of a CO emission inventory at the city scale: Santiago de Chile's case by Saide et al.

The main goal of this paper is to improve the surface emissions fluxes of CO in a very urbaized site: Santiago de Chile. The main problem pointed out by the authors at this spatial scale is the co-localisation of measurements stations. They propose a methodology based on a new factor to be applied on the error covariance matrix. They showed that, in average, their results diagnosed a decrease of CO surface emissions of 8% to be more consistent with observations.

The subject is original and to improve urban inverse modeling methodologies is a very difficult task. Unfortunately, the first step is to use a model giving realistic results before the inversion. Because the colocalisation effect is more an second order effect,

compared to the vertical mixing, the transport and the surface emissions fluxes due to traffic.

In this study, the direct simulations showed (both for meteorology and surface concentrations) that the model is not able to really reproduce the measurements. In this case, the inverse modeling process is not suitable: the differences between modeled and measured concentrations are so important that it is not realistic to report all possible error on CO surface emissions fluxes. The meteorological fields could be improved before retrying an inverse modeling experiment: if not, we can not be confident in the optimized emissions fluxes. Even if a validation was done for another period, the better results are certainly due to the fact that the same kind of errors are corrected for both period: but probably not only emissions fluxes. In addition, a lot of model choices are done and debatable (not deposition, short spin-up etc.), leading to increased uncertainties in the results.

General remarks: ——————- Many studies were already done about the CO inverse modeling. For example, some really (old but) important studies are:

- Mulholland, M. and J.H. Seinfeld, 1995, Inverse air pollution modelling of urban-scale carbon monoxide emissions, Atm. Env., 29, 497-516

- Chang, M.E. and D.E. Hartley and C. Cardelino and D. Haas-Laursen and W.E. Chang, 1997, On using inverse methods for resolving emissions with large spatial inhomogeneities J. Geophys. Res., 102, D13, 16023-16036

- Bergamaschi, P. and R. Hein and M. Heimann and P.J. Crutzen, 1999, Inverse modeling of CO mixing ratios, J. Geophys. Res., 105, 1909-1928

The authors say CO is a "long turn-over time of 2-4 months" species. Is it realistic to model the complete Santiago area with only a 24h spin-up model time? In addition, the authors say they started the run with null-values? Why? Is it possible to have a correct modelling of the pollution episode with this model set-up? without any realistic initial conditions? The results could be really improved by initializing the model with the available measurements, interpolated over the model domain (using kriging for example).

Even if dry deposition and scavenging are not dominant sources, it seems strange to not calculate these sinks for the CO behaviour durint the studied period. To prove that this choice is correct, it is necessary to present time series of precipitation and deposited fluxes of CO. The relstive amount due to this sink must be discussed and compared to the global model error.

The main purpose of this paper is to improve CO inverse modeling at the urban scale. The author stated than, decreasing the horizontal scale, the colocalisation of surface measurements becomes a problem. This effect depends on the resolution but also the meteorological conditions (dispersion effects) and the species reactivity. In the case of CO in Santiago, is it possible to quantify the relative weight of this co-localisation effect compared to all other model uncertainties? This has to be due to prove that the goal of the paper is not a second order problem compared to the emissions inventory uncertainty.

The figure 2 shows comparisons between measured and modelled temperature. The vertical shape of the profiles tends to show a difference of inversions altitudes. This may induce large discrepancies on the boundary layer estimation (using the Louis formulation). It could be useful to see vertical profiles of temperature and wind, richardson number for selected times (such as 07:00, 10:00, 13:00, 17:00, 20:00, 23:00) and the diagnosed boundary layer height (BLH). For the inverse modelling of CO, the appropriate estimation of BLH is a key point: the differences between model and observations may be only due to this representation of the atmosphere mixing state.

The authors said that the model failed to represent very stable noctrunal conditions. This is not a surprise and certainly a major problem of all existing regional models. But this could have a non negligible impact on inverse modeling results: are the authors

able to quantify the impact of this kind of error several hours before the peak time of CO? An error made during the night will affect the transport and thus considerably change the modelled concentrations when they are compared to measurements.

The Figure 3 shows not really good comparisons results between measured and modelled CO surface concentrations. The sentence, line 3, p.6333, 'we evaluate the model performance as adequate for the air quality simulations we are interested in, in particular during daytime' do not reflect what the curves show.

In addition to nighttime modeling problems, the authors explain the model is not able to give correct results during the week-end. This is certainly due to a major change in the traffic fluxes (often observed in megacities). Is the inventory taken into account the week-end effect? If not, a first step would be to improve a little the inventory following this direction. To remove the week-end part of the modelled and measured concentrations for the inverse modeling is not correct: this is not possible to model a complete period and to inverse only the periods when the model gives realistic results. The inverse modeling process must be continuous in time to taken into account the transport and possible recirculations.

In section 4.2, the authors restrict their inversion to fluxes greater than 0.5 mug/m2/s. Does it mean that, depending on time, the number of model grid cells (when an inversion process is performed), is not constant in time? This assumption is linked to another one: the inventories errors at one place are not correlated to errors at another place. But, this is certainly not the case since the same methodology is applied to built emissions over the whole domain. How the authors justify to spatially split the inversion process like this?

Minor remarks: ————————- l20, p.6328: "the zonal fl ow", correct to flow. l8, p6337: correct "uncertainities"

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