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Interactive comment on "Constraining regional greenhouse gas emissions using geostationary concentration measurements: a theoretical study" *by* P. J. Rayner et al.

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Response to Referees' Comments

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14 May 2014

We thank the two anonymous referees for their comments which have allowed us to clarify various points in the paper. Since the referees make different points we will respond to each in turn. Below we place the referees' comments in typewriter font and our responses in Roman.

Anonymous Referee 1

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The manuscript investigates the use of geostationary
measurements of CO2, CO and CH4 trace gases for constraining
carbon sources and sinks at regional scale.  The authors
test a proposed observing system for GEOCARB by calculating
posterior un- certainty for theoretical emission sources over
Shanghai, China.  Despite improved sampling density compared to
orbiting satellites, CO2 alone does not lead to signifi- cant
uncertainty reductions at 3 km grid scale.  This is because CO2
can't disentangle urban sources from power plants.  The authors
use theory and practice to show that a joint inversion with CO
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helps constrain combustion sources and can therefore signifi-
cantly improve knowledge of fluxes. Key innovations in this
work include a high resolution satellite inversion, development
of a plume model applicable for column CO2, and careful
accounting of effects due to slantwise column measurements
(which are typi- cally ignored at coarser resolutions).
Additionally, the text is lucidly written and easy to follow.
I highly recommend this paper for publication after addressing
a few conceptual points below.

General Comments

I am a bit naïve on the subject, but the plume concentration
model seems quite a novel way to represent the statistics
of column integrated concentrations and thus a poten- tially
powerful tool in our rapidly expanding GHG satellite era.
Despite the detailed description of the model, however, I found
it a bit difficult to visualize how the parame- ters in Eqn
(4) represent the 3D structure of the plume. As mentioned by
the authors, this task has received little attention but is
likely to receive much more, so it may be useful to provide a
schematic to help illustrate the concept.

We don't think this is very helpful here. It's important to stress that noone would use
a model like *SATPLUME* to infer fluxes from real observations. The model is far too
simplistic for that. For OSSEs we did need a model that was efficient enough to run
a series of tests but it is possible that we will never use *SATPLUME* again. The sim-
ple residence time calculation we ran Section 3 (P1377L15–20) suggests that the be-
haviour is physically realistic. What is needed next is some tests against a serious

tracer transport model.

Please comment on potential limitations of the proposed
observing system for con- straining winter emissions at high
latitudes; i.e., is it feasible to reduce uncertainties at high
spatial resolution given high solar angles and uncertainty of
winter boundary layer dynamics? This is a very good point. We have
added a paragraph at the end of the discussion highlighting this limitation and stressing
the need for complementary measurement approaches.

Specific Comments

Please provide more detail regarding the normalization
constant Q in Eqn (4). We have clarified this to say "where Q is a nor-
malization constant which guarantees that the integrated mass equals the integrated
emission"

In describing the prognostic equation for spread starting on
P1374L7, rates of dis- persion due to turbulence, divergence
and shear are referred to in Eqn (6), but only terms due
divergence (ϕ_D) and shear (ϕ_S) are shown. Please check
on this. All three terms are described in the subsequent
discussion, so it is likely the terms was mistakenly omitted
in Eqn (6).

In fact it is the wording which is incorrect. turbulence is not treated in the linear prog-
nostic model so it should not have been mentioned when discussing Eq. 6. It appears
shortly later. We have corrected the wording.

At the beginning of Sec 3, tracer emissions are described as
occurring near the center of the domain. In Fig. 1, however,

the source is centered at x = y = 101.5 km, which appears to
be in the northeast corner of the domain. Please clarify.
A star in Fig. 1 indicating the power plant location could
be helpful. It is also not clear whether the power point is
intended to be in the same location in Figs. 1 and 2-4.

This was a graphical decision. Because of the wind direction a figure with a single
plume starting at the centre of the domain would be mainly empty. Thus we only plotted
roughly the bottom left quarter of the domain. This has been clarified in the caption.
It is interesting that peak values of uncertainty reduction in
Fig. 3 occur upstream of the power plant. If the prevailing
wind is northeast to southwest, it seems highest re- ductions
would be centered more on the power plant and/or downstream of
the source. Please comment on this.   The highest reduction is for
the pixel of the power-plant itself (x=y=91.5km). For other points there seems not
much difference between upwin and downwind points. One would, in fact, expect
higher uncertainty reductions upwind since those estimates can use measurements
"uncontaminated" by the large uncertainty injected by the power-plant.

 I am trying to get my head around the combined effects
of viewing geometry and pre- vailing wind direction on
signal-to-noise and error reduction. If the satellite is
sitting equatorward of the power plant and looking north
into the prevailing wind, presumably the effect of wind shear
is to tilt the plume into the satellite such that it aligns
vertically with the slantwise measurement. I wonder if this
will increase the signal and hence to- tal error reduction,
compared to a prevailing wind which moves away from the
satellite. This may be a moot point, but in case it affects
our interpretation of results, it could be worth commenting

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on.

The problem is even worse than this because it's not just the viewing direction of the satellite but the position of the sun that will affect the total traversal of the plume by a photon. This was another thing we chose not to worry about. The problem will not arise in a real model which produces the 3-dimensional distribution and, for *SATPLUME* it seems equally likely to enhance or degrade the visibility of a plume so should not affect the overall result.

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 All figures need some labeling on the x- and y- axes.
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Corrected.

Technical Corrections

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 P1369L14:  Misspelled 'Mesurements"
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Corrected

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P1375L7:  First instance of WRF should be defined here.
Currently defined later, on P1379L10.
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Corrected.

Anonymous Referee 2

General


```
This paper studies a new concept of CO2 and CH4 emission
measurement from space.  An application is shown for an
idealised city.  The paper is well written and informative.
However, the city toy model is very simple and does not really
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support the high-profile conclusions.  It is actually not clear
what one should conclude.  More work may be needed to make the
model capture some of the main features of the real world,
i.e.  not those from the clear-cut and emission-homogeneous
city hosting a single power plant and set in a background
without CO emissions (i.e.  without any road) represented
here.  For instance, CO measurements are said to "play a vital
role in constraining combus- tion source" (p.  1380), but this
could just be a consequence of the artificially uniform CO:CO2
emission ratio.  Indeed CO:CO2 from traffic varies with fleet
composition and speed, hence with the road type:  it is not
homogeneous within a true city.  From the re- sults presented,
one may conclude the opposite of what has been written:  the
concept fails for CO2 because it requires a uniform CO:CO2
emission ratio.
```

This comment is based on a misunderstanding of the treatment of CO. We note at P1371L21 that the CO emission factor may vary at the same scale as S. Thus there is no assumption of a uniform emission ratio. this point is important enough that we have made it explicit in the text.

```
The toy model is even simpler for CH4 , and the corresponding
text is reduced to 12 lines only, results included.  Urban CH4
emissions may come from leaks at unknown locations to a large
extent, a specificity which is not accounted for in this simple
set-up.
```

We solve for CH_4 emissions on a grid. the "unknown locations" will occur in one of these gridcells and the solution method makes no assumption about which one. Thus we think this uncertainty is accounted for. Uncertainties in urban emissions, such as leaks, are likely to be larger than the case we study thus more rather than less likely to

be visible.

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Last, throughout the results, the atmospheric flow is supposed
to be perfectly known, which dramatically helps the inversion.
None of these assumptions are justified or tested in the paper,
which is hard to understand in a pure simulation context
(measurement free) where the chosen set-up fully drives the
results.
```

We note at P1382L20 that the retrieval performance reported by Polonski et al. is considerably better than the uncertainties we use in the inversion experiments. The conservative treatment of these input uncertainties is based, in part, on uncertainties in atmospheric transport modelling. This point is made at the bottom of P1372. At P1383L3 we point out one method for quantifying transport errors and describe a test case. At P1378L8 we highlight the importance of testing *SATPLUME* against full-featured transport models. We do not think more clarification is necessary here.

Detailed comments

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p. 1369, third paragraph: the evocation of Carbonsat is not
completely fair be- cause it misses its imaging capability (in
clear sky) that damps the issues raised.
```

This is a fair point. An argument about what Carbonsat can or cannot do is not our main point here. We have altered this to say that imaging offers advantages in localizing sources. We cite the study of Law et al., 2003 to suggest that capturing the time evolution of the concentration field, on time-scales relevant to the spatial scales we are trying to resolve, will further strengthen this capability.

```
Section 2.1.2: the authors should also report typical pixel
resolution at mid and high latitudes, and the specific pixel
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resolution for Shanghai, a city used here for inspiration
(Section 4.1).
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This has been added: The equation in section 2.1.2 and the values for our experiment in Section 4.1.

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p. 1377, l. 19: the authors should explain how the
steady-state response yields the discontinuous pattern of Fig.
1.
```

This is caused by a combination of the simplified observation operator, the model timestep and the colour-scale in the figure. The model timestep of 30 minutes can occasionally advect plume centroids several gridboxes. This can dilute them. This is most obvious with the gridcell observation operator (left panel). With the smoother observation operator for the real case (right panel) this does not happen. We could shorten the timestep to avoid this but since the left panel is only illustrative we did not think it worthwhile.

```
Section 4.1, first paragraph: The unit of the uncertainty
is surprisingly given for a full year, while the simulation
includes six days only. Depending on temporal correlations,
the given numbers may correspond to many weekly values. As-
sumptions about spatial and temporal correlations should be
explicitly written and justified.
```

the y^{-1} is simply a choice of unit, not a reflection of the study period. Thus we are *not* claiming these reductions for a year but they pertain to the six day observational period. Uncertainty reductions for a year are likely to be much larger but their calculation, as pointed out by the referee, relies on assumptions about temporal correlations, assumptions we have not made here.

```
p. 1379, l.9-18: the authors should indicate the sampling
```

density that they get, given the thresholds on aerosols and
clouds chosen in Section 2.3.1.

We have added this to section 4.1.

Fig. 3: the authors should write the temporal reference of
the fluxes (I guess six days).

We have added this throughout section 4 and to the figure captions. It also pointed out
to us that we had not quoted which of the 4 blocks of six hours we were using. This
has also been clarified.

p. 1380, l.7: how do the authors extrapolate their weekly
result to a full year? Again, assumptions about spatial and
temporal correlations (and corresponding justifications) are
missing.

We don't extrapolate, see response above.

Section 5: the uncertainty description is even rougher than
for CO2 .

We have made the same changes as for the CO_2 case.

p. 1382, l.21–22: does the offset also need to be homogeneous
in time within the presented framework? Anyway, I do not see
any reason why the aerosol field would be constant.

A good point, at the moment we don't know how aerosol contamination will affect re-
trievals at different scales. We have added a sentence making this point.

p. 1383, l.15: This "ameliorating factor" may actually have
little power because the plume spreads over time and merges
with other plumes.

The diffusion of plumes is scale-dependent, large plumes spread more slowly than small ones. It's beyond the scope of this paper but previous work with global models showed that 1° night time sources *could* be recovered the next day, there was enough structure remaining in the atmosphere. This is likely to work better with a geostationary measurement where we can target measurements soon after dawn to capture as much structure as possible.

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p. 1384, l.1:  the assumption used here is strong, not weak.
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this refers to the asumption of the structure of the emission ratio. We have addressed this previously.