Response to Reviewer 1

We would like to thank the reviewer for their analysis of the GMAO data which we believe greatly improved the analysis discussed in our paper. We apologize for the delay in re-submitting the paper but as discussed next it took some time to obtain the needed GMAO fields, replicate as best as possible the reviewers' results, and propagate these results to our analysis.

In order to address the reviewers concern we downloaded the high resolution GMAO CO_2 data for the spatio-temporal matchups corresponding to OCO-2 measurements used in our analysis. Note that the OCO-2/GMAO data is for 2005-2007, which does not match the OCO-2 observation timeframe, but we would expect the size of the variations to be adequately captured by comparing to the same locations and times from a different year. We repeated as best as possible both the analysis of the reviewer and examined the variability corresponding of the model corresponding to the data. The variations we found, even when matching what the reviewer did, were less than the variability the reviewer found. We show our findings at the end of this response. Our conclusions are therefore effectively unchanged as on average the "true" variability represented by the modeled XCO_2 is still much less than that observed by OCO-2.

To address this concern in the paper we have moved part of the introduction, where we had previously discussed the role of natural variability, into the analysis section and added the hypothesis "*H1: Observed variability is due to natural XCO_2 variability*". We have also added an Appendix where we looked at the expected distributions of observed XCO_2 when accounting for natural variability, calculated noise, and calculated interference error and compared them to what is observed before and after the bias correction.

As mentioned above, a critical caveat to our update is that the variability we find from the modeled fields is less that that found by the reviewer (see Figure below). We have triple checked our book-keeping so we would like to ask the reviewer to check theirs to ensure our results make sense.

Minor Comments

1. P4, L5: It is not correct to say that the OCO-2 instrument always observes the "glint spot" of specular reflection, since, as the very next sentence explains, there are both "glint" and "nadir" modes.

   Response: Fixed (grammar error)

2. P4, L15: How do the authors know that the statistics of the target mode soundings are spurious? What makes them spurious?

   Response: Brought cited papers up to front of reference and changed spurious to "outside of the expected range"

3. P4, L24: The authors use bias-corrected XCO_2 for this exercise. In theory, bias correction should remove long-range correlations in the error in XCO_2 by reducing what the authors call interference error. However, if the bias correction parameters are not chosen correctly, the bias correction itself will introduce slowly varying biases. Can the authors verify that using non-bias corrected (or "raw") XCO_2 from ACOS leads to a larger estimate of the slowly varying error in H3?

   Response: We have added discussion on how the bias correction affects the observed variability in the appendix and as the reviewer suggests it does improve the comparison. Our analysis shows that the bias correction REDUCES the slowly varying gradients as one might expect if the bias correction is correcting errors related to aerosols and because we expect the gradient is caused by an issue related to the aerosols.

   More discussion on the bias correction is in the Wunch et al. papers (cited)

4. P4, L25: As far as I know, the bias correction depends not just on TCCON XCO_2 but as well as on the so-called "southern hemisphere approximation" and a small area analysis where XCO_2 is assumed constant over < 100 km along track (see page 14 of http://disc.sci.gsfc.nasa.gov/OCO-2/documentation/oco-2-v7/OCO2_XCO2_Lite_Files_and_Bias_Correction.pdf).

   Response: Fixed language

5. P4, L28-29: There is emerging consensus in the OCO-2 flux inversion community that filtering by warn levels (WL) only lets in retrievals with significant bias from interference terms. Rather, filtering by **x co2_quality_flag** , which is WL < 15 plus some additional criteria on retrieved aerosol and CO_2 parameters, is a much better way of reducing the number of biased samples. Can the authors confirm this by showing that if they use soundings with **xco2_quality_flag** = 0 they get a smaller contribution from the slowly varying bias of H3?

   Response: I would prefer to keep more detailed, iterative studies like this off-line and instead use the analysis shown in this paper as well as those from Wunch et al. [2016] and Connor et al. [2017] as descriptions of methods and initial results used to test these refined hypothesis about the uncertainties.

6. P4, L29: The highest WL is 19, not 20. (Fixed)

7. P5, L13: Should be "XCO_2" instead of "X_{CO2}" (Fixed)

8. P5, L21: The CT-based variability in the N-S gradient of XCO_2 was estimated only over North America, yet it seems to have been used everywhere between 30°S and 30°N. How valid is this assumption? (Addressed with major comment)

9. P7, L11-13: In the statement of hypotheses, I think the authors mean "variations in XCO_2" and not "uncertainties". If I understand correctly, the entire point of the manuscript is to see whether variations in XCO_2 within a small area are consistent with XCO_2 errors being primarily from random noise, correlated noise, or a slowly varying bias. So the choice of words in L11-13 is important, and I'd like the authors to either confirm or refute my understanding that "uncertainties" should be replaced by "variations in XCO_2". Response: Changed

10. P9, L18-20: Can the onset of this strong inverse relationship between calculated and actual uncertainty below a certain threshold be used to filter out seemingly low noise (high SNR) soundings over the tropical oceans that might be biased?

    Response: possibly and we have communicated this issue to the OCO-2 team (several of who are co-authors).

11. P10, L1: I think the authors mean "Figure 2" (or 3, or 4) instead of "Figure 1". (Fixed)

12. P11, L3: Why the lag of 0.3 sec? Is it because OCO-2 cross-track "strips" are spaced 0.3 sec apart along track? If so, that should be mentioned. (Fixed)

13. P12, L18-20: Recent results shown at OCO-2 science team meetings and telecons suggest that over small areas, surface elevation has a strong impact on retrieved XCO_2. Is this included in $\mathbf{GK}_{\mathbf{y}}$, i.e., is surface elevation in the vector $\mathbf{y}$?

    Response:. Isnt this the same as an error in surface pressure? If so my understanding is that this error is included.

14. P13, L1: Each of the distributions (Gaussian, Lorentz, Laplace) considered by the authors

has a physical basis, i.e., there are reasons why a quantity might follow one of the three distributions. E.g., if two independent variables each follow an exponential distribution, then their difference follows a Laplace distribution. Can the authors speculate why the slopes in Figure 8 might behave like such a quantity?

Response: We (the authors) discussed why one shape or another had a better fit but could not come up with any reasonable explanation / hypotheses. For this reason we do not speculate in this paper the reason for the shape of the distribution. However, an update to the Connor et al. (2016) analysis which uses the same small neighborhoods we use, along with the observed distribution of these shapes could shed light into the primary sources of uncertainty in the XCO_2 data that are not currently accounted for by the uncertainty calculations. We have added a statement to that effect in the conclusion.

Response to Reviewer 2

Comment: Most of the issues concerning this paper have already been flagged by referee#1 and I will not repeat them here. However an additional point that I feel needs more elaboration is the difference in land and ocean results. When testing the first hypothesis, we see a 0.4 ppm bias shift between the observed land data and what could have been expected from the calculated measurement uncertainties. However if we look at the ocean data we see no bias in the >0.4 ppm calculated uncertainty bins. Are the potential components that yield the land bias [synoptic variation, non-linearities in the retrieval, etc] truly all absent over the ocean? If not, the calculated error components over the ocean might very well be overestimated. In the summary this is again touched upon. Calculated ocean errors are simply deemed correct, while land (including glint-land) are deemed to be underestimated. The authors need thus to explain why the potential sources of error play no (or insignificant) role in the glint ocean retrievals.

Response: Mechanistically explaining why these differences exist is well beyond the scope of this study as it requires systematically updated a sophisticated radiative transfer model in order to test each effect. Rather we document the differences here which can then be used to test the uncertainties in a future study. We add a statement in the abstract and the summary that these ocean/land differences lead us to suspect that surface pressure and albedo are the likely issues affecting the accuracy of OCO-2 data because we would expect surface pressure and albedo to vary more strongly over land than ocean. We have added a sentence in the abstract and the following statement in the summary to address these issues:

This analysis sheds further light on the sources of uncertainty of the observed XCO_2 data. For example, the XCO_2 gradient variability in the small neighborhoods over the ocean as compared to the land suggests that the largest uncertainty in OCO-2 XCO_2 data is related to surface properties such as surface pressure or albedo because we expect larger variations of these geophysical parameters over land. The observed gradients could also be related to the variation in solar zenith angle as OCO-2 data takes observations because the effect is manifested as a slowly varying quantity in addition to increased random variability. The observed distribution of these XCO_2 gradients over the whole globe, which has a Laplace distribution, is also a potential clue as any bottom-up or future analysis that attempts to model the XCO_2 uncertainties should also replicate this distribution. A future study in which the calculated uncertainties for OCO-2 discussed in Connor *et al*. (2016) repeats the steps shown in this paper in conjunction with the OCO-2 / TCCON data will hopefully reveal and characterize the likely sources of these uncertainties.

Minor comments

P1L28: ...in reasons that ‘are’ not well understood (fixed)

P4L3-4: remove ‘with’ and ‘observes’... (fixed)

P12L25: the calculated random noise or ‘actual’ noise? (fixed)

Expanded Discussion on XCO_2 variability in response to Reviewer 1 Comments

Model analysis from GMAO 7 km model fields

Again, we thank reviewer 1 for pointing us to this model output which is much more relevant to our analysis than the 1 x 1 degree CarbonTracker output. We looked at the 7 km data for July 7, 2006 at UTC 20:00 from 24-50N and 127-64W (file ftp://ftp.nccs.nasa.gov/Ganymed/7km/c1440_NR/DATA/0.0625_deg/inst/inst30mn_3d_CO2_Nv/Y2006/M07/c1440_NR.inst30mn_3d_CO2_Nv.20060707_2000z.nc4). The data was classified as land or water using a land surface map from the UW/CIMS infrared emissivity database (Vidot and Borbas, 2014). XCO2 was calculated based on pressure weighting with pressure from the corresponding pressure file (c1440_NR.inst30mn_3d_DELP_Nv.20060707_2000z.nc4). Every 0.2 degrees latitude and longitude, a pair of points spaced by 15 points (or 105 km) north/south are selected. A histogram of the differences of XCO2 between these points was plotted, selecting either ocean or land subsets. The same analysis was also performed for the observations used in the paper, which span September, 2014 to May, 2015 and do not include summer, which has the most variability.

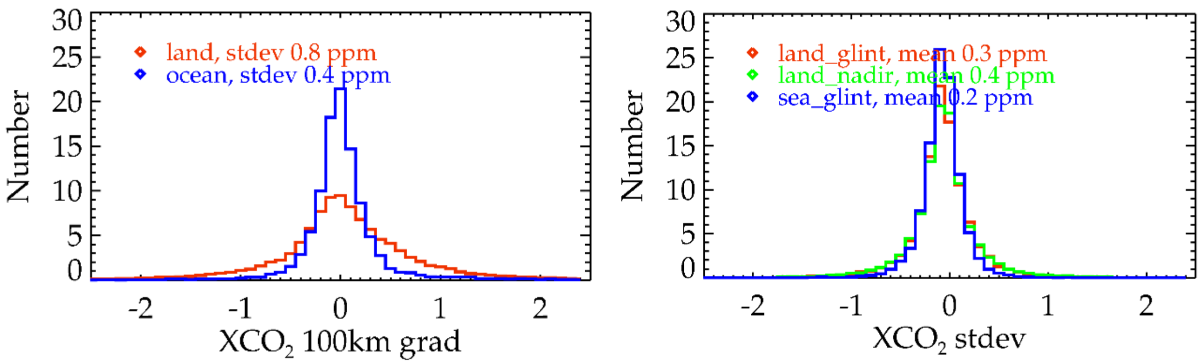


Figure 1. 100 km N/S XCO_2 differences for land (red) and ocean (blue) from NASA GMAO 7 km run for XCO2 for Summer 2006 (left) and locations/times matching OCO-2 observations (offset by year) (right)

The XCO_2 north/south 100 km differences found by the reviewer is 0.8 ppm (land) and 0.4 ppm (ocean) for the 0.5 degree resolution. For the 7 km resolution, the reviewer found 2.2 ppm (land) and 0.9 ppm (ocean). Our results for the 7 km resolution GMAO model agree with the reviewer's result for the 0.5 degree resolution but show much smaller distribution of gradients than the reviewer's results for the 7 km resolution. Our results for the 7 km resolution GMAO model have about, on average, twice the gradient as seen in the 1x1 degree CarbonTracker model shown in Fig. 1 of the discussion paper and Figure 2 below. However, when matched to the observation locations/times used in this paper, gradients of 0.2 ppm to 0.4 ppm are seen, which is smaller than the variability seen in the OCO-2 data shown in Fig. 7 of the discussion paper (now Figure 8). Therefore the conclusion that the larger variability and gradients seen in the OCO-2 data do not result from natural variability is not changed by the additional study of the high resolution model fields.

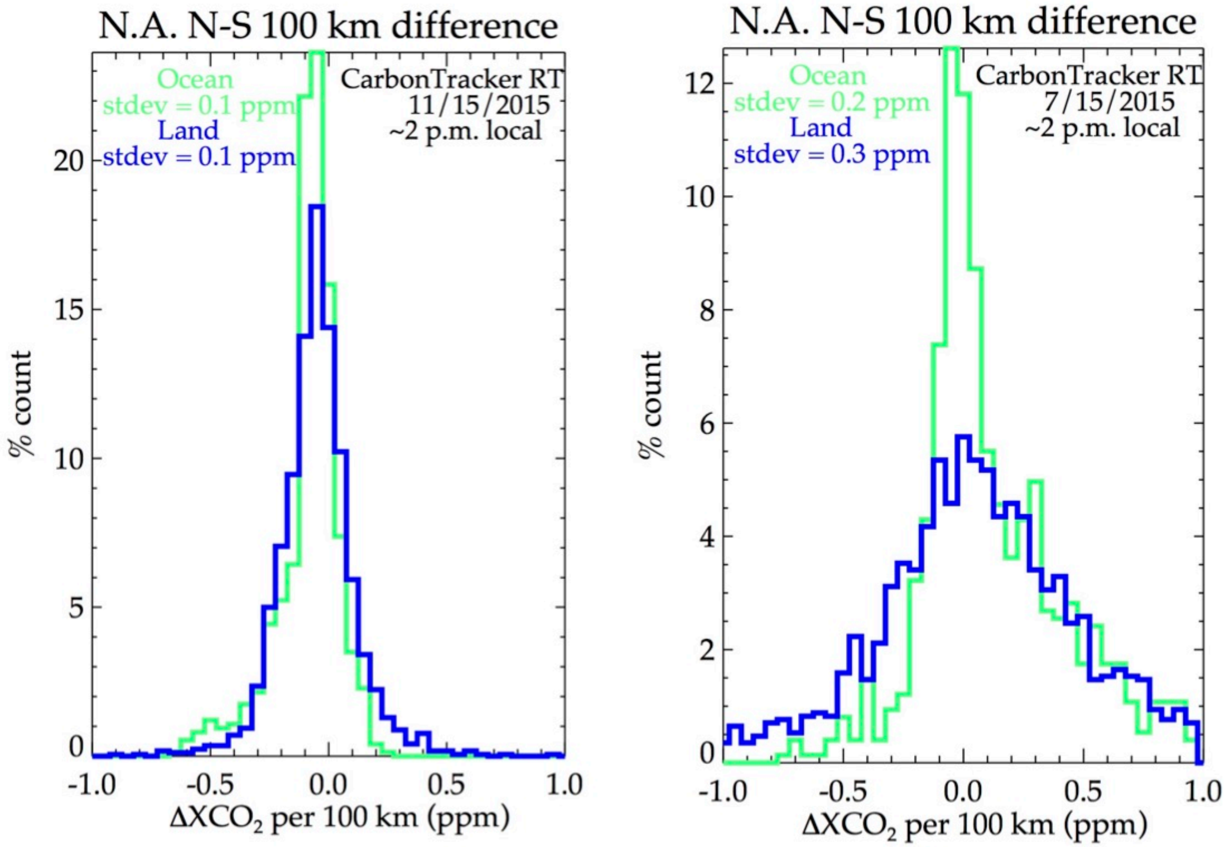


Figure 1: Distribution of latitudinal XCO_2 gradients as calculated by the high resolution, "Real Time", Carbon Tracker model for November 2015 (left panel) and July 2015 (right panel) over North America and the nearby oceans. The latitude grid is 1 degree or ~110 km. The gradients are re-scaled to 100 km for comparison to the XCO_2 gradients discussed in this paper.

Response to Editor (Ilse Aben) Comments

Comment: It seems that sometimes slightly different terminology is used for the same thing. This unnecessarily complicates the reading and understanding of the paper. It would be very helpful to stick to the same terminology throughout the paper. (an example : p.8-9 calculated measurement noise, calculated measurement error, measurement uncertainty due to noise, are these all different things or are they indicating the same thing ? If they mean the same thing please use one term)

Response: I have replaced all instances of "measurement error" with "measurement uncertainty" and where appropriate added a caveat, i.e. measurement uncertainty due to noise (as opposed to interferences).

INTRODUCTION

Comment:- The analysis with Ctracker is limited to the US, which means roughly speaking latitudes higher than 30 N. Whereas the OCO-2 data analyses that follows focuses mostly on neighborhoods between 30S-30N. To what extent are the variabilities as obtained for N-America then useful to compare with ?

Response: We have removed the comparison with Ctracker and replaced with GMAO

Comment: p.3,l.16 'while in-situ ...', are these column integrated variabilities or are these in-situ measurements at a certain height or .... ? - p.3, l.16 'while in situ and model data ..' what model data do you refer to here ?

Response: This should be fixed in the subsequent version as we now exclusively compare to the GMAO model fields

Comment: OVERVIEW OF THE OCO-2 DATA - p. 5 I am bit lost now. In the refered document a description is given on the Bias correction for OCO data. Here also corrections are based on the small areas and variability seen within these small areas, and a correction based on main parameters influencing that. Has such a correction already been applied to the data here ? If so, how does that affect the neighborhood analises here ? - for which period OCO-2 data is analised ? I don't think it is mentioned anywhere. I think it should be mentioned quite early in the paper.

Response: We added language in this section stating that the analysis is bias corrected data and how it is bias corrected. We also show in the Appendix how the bias correction improves the comparison between expected and actual variability.

EVALUATION OF UNCERTAINTIES

Comment: L17-19, p.5 the data that is used in a neighborhood is presumably taken from one orbit and are thus very close in time ? So never data from different moments in time that happen to fall in the same 100 x10.5 km area are compared as part of one neighborhood ?

Response: Correct. We have added "taken consecutively" when describing the data in a neighborhood for clarification.

Comment:L19-22 p. 5 please provide map to show the locations of these neighborhoods. If not in the paper than at least in the response such that it is visible to interested readers.

Response: Maps of the neighborhoods for each observing mode are provided below. As stated in the text most neighborhoods where there are at least 50 consecutive data points that also passed the quality flags are in the sub-tropics.

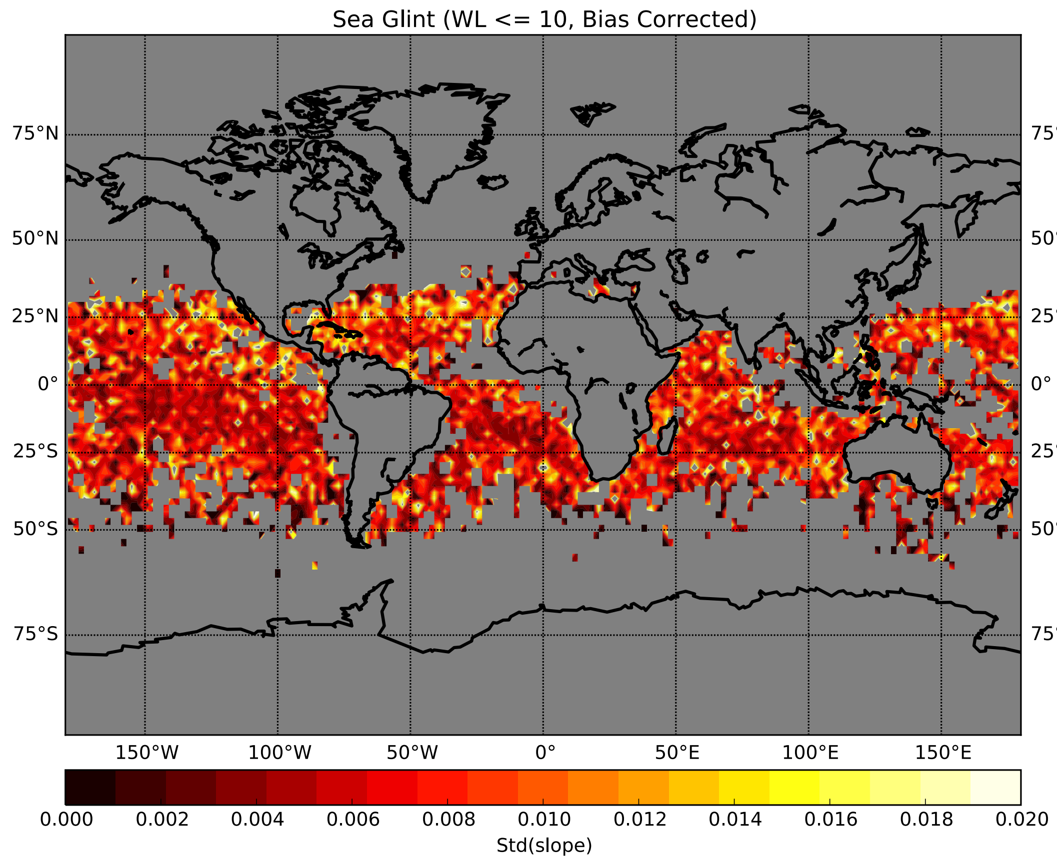
Comment: I think the basic info to verify the average 190 observations are not in the manuscript. Please provide, and briefly explain why indeed you have ~190 observations per neighborhood on average to work with. Is there also a way that we can understand why you get roughly 39000 neighborhoods to work with ?

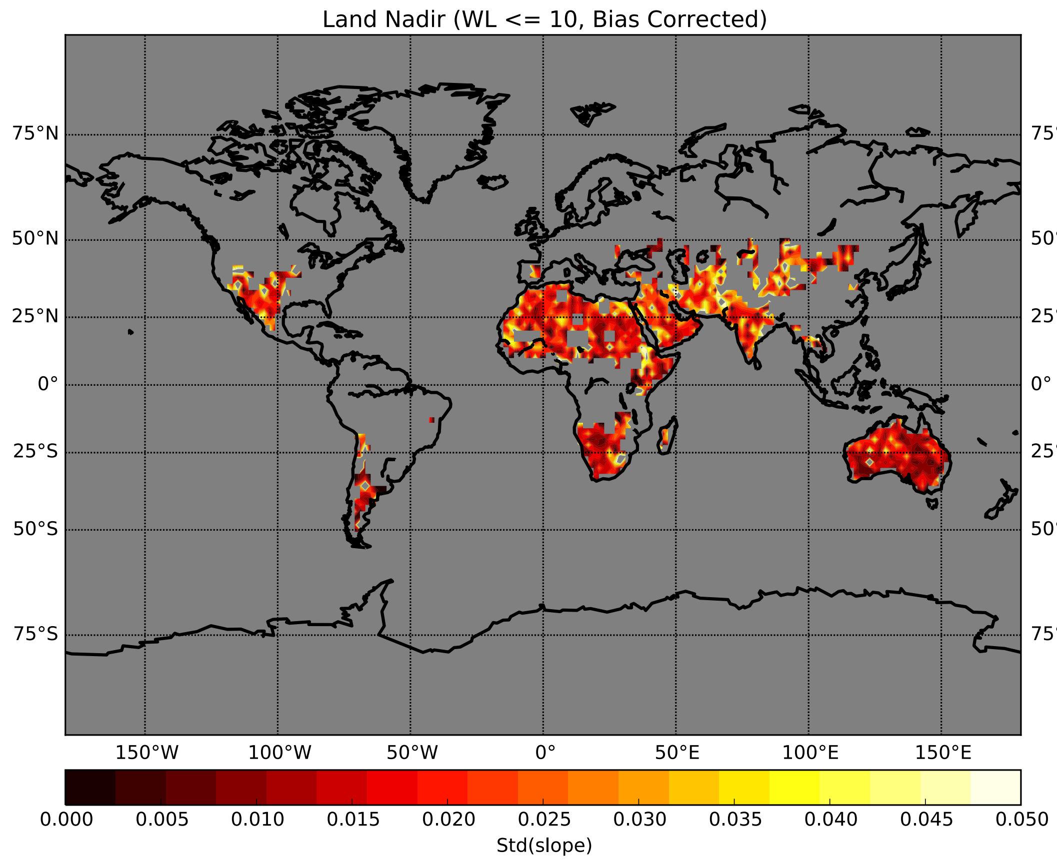
Response: Within about 100km (the size of the neighborhood), OCO-2 takes approximately 190 observations. We added language to clarify

Comment: L. 27 mean CO2 column à mean CO2 column in a neighborhood ? （yes, fixed）

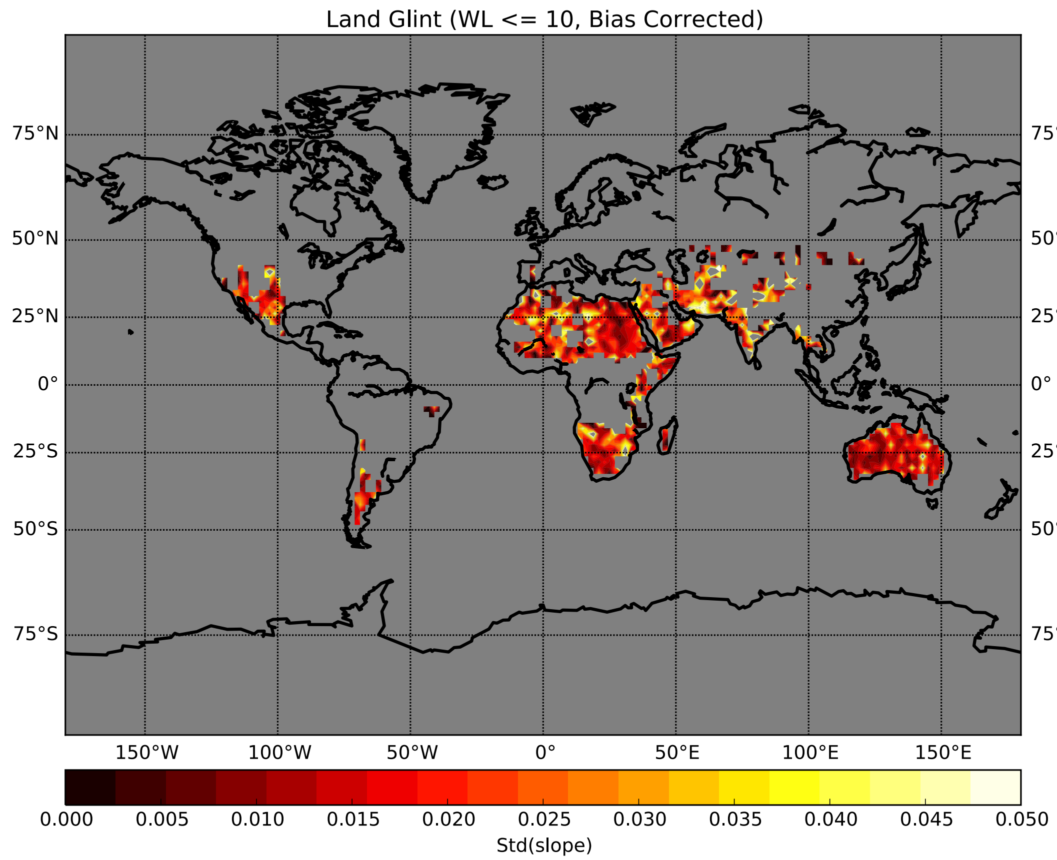
Comment: P. 7 is 'small area' the same as 'small neighborhood' ? if so please use one term throughout the paper. (fixed)

Minor textual: (all fixed)


Sea Glint (WL <= 10, Bias Corrected)
75°N
50°N
25°N
0°
25°S
50°S
75°S
150°W
100°W
50°W
0°
50°E
100°E
150°E
0.000
0.002
0.004
0.006
0.008
0.010
0.012
0.014
0.016
0.018
0.020
Std(slope)



Land Nadir (WL <= 10, Bias Corrected)
75°N
50°N
25°N
0°
25°S
50°S
75°S
150°W
100°W
50°W
0°
50°E
100°E
150°E
0.000
0.005
0.010
0.015
0.020
0.025
0.030
0.035
0.040
0.045
0.050
Std(slope)


Land Glint (WL <= 10, Bias Corrected)
75°N
50°N
25°N
0°
25°S
50°S
75°S
150°W
100°W
50°W
0°
50°E
100°E
150°E
0.000
0.005
0.010
0.015
0.020
0.025
0.030
0.035
0.040
0.045
0.050
Std(slope)