Author's comments in reply to the anonymous referee for "Decadal changes in global surface NOx emissions from multi-constituent satellite data assimilation " by K. Miyazaki et al.

We want to thank the referee for the helpful comments. We have revised the manuscript according to the comments, and hope that the revised version is now suitable for publication. Below are the referee comments in italics with our replies in normal font.

Reply to Referee #2

1. I certainly appreciate the effort made by the authors to incorporate more data. There is logic to it: more data should be better than just one dataset. It is argued (maybe a bit too emphatically) that non-NO2 datasets improve the NOx emission estimation because they should lead to better estimation of the NOx lifetime in the model. In general, that might be true, but I wouldn't be so sure that it is automatically the case. I find that adding more data from different species might contribute to obscure the interpretation of the results, because the additional data come with their own limitations and uncertainties (including biases) which are not all well characterized. I am not fully convinced that authors understand perfectly the role of the different datasets in the assimilation. I wonder in particular to what extent the NOx emission updates are driven by the non-NO2 datasets. For example, ozone is apparently biased low in the model. Increasing NOx emissions is naturally found to improve ozone. But ozone could be biased low due to other reasons (transport, deposition, NMVOC chemistry and emissions). So, is ozone improved for the good reasons? Who knows? Many other CTMs overestimate surface ozone. I encourage the authors to moderate their claims regarding the advantages of additional data.
That being said, I concur that assimilating non-NO2 dataset should contribute to improve (somewhat) the NOx lifetime in the model, which is a good thing. But I would expect the authors to provide a more quantitative and systematic analysis of how the non-NO2 datasets influence the assimilation results. I also encourage the authors to be more cautious in their discussion, to reflect the possible limitations and complications associated with the use of additional, non-NO2 measurements.

We appreciate the author's constructive comments. First, the possible limitations associated with the use of non-NO2 measurements are more clearly discussed in the revised manuscript. The following sentences have been added to Section 5.3:
"Although the assimilation of multiple-species data influences the representation of the entire chemical system (Miyazaki et al., 2012b, 2015), the influence of model and observation errors remains a concern. In the multiple-species data assimilation, model performance is critical for the correct propagation of observational information between chemical species and to improve the emission estimation, whereas

biases in any of the measurement data sets (including non-NO2 measurements) may seriously degrade the emission estimation (Miyazaki et al., 2013). Improvements in the model, data assimilation scheme, and retrieved observations are essential to reduce the uncertainty on the emission estimates from the multiple-species data assimilation."

Secondly, the impact of non-NO2 measurements is more clearly discussed in the revised manuscript. Table 5 has been added to demonstrate the impact of non-NO2 measurements on the a posteriori emissions. Linear trend estimations from the NO2-only assimilation have been added to Table 4. The relevant discussions in Section 5.1 have been rewritten as follows:

"Table 5 compares the estimated emissions between the multiple-species data assimilation and a NO2-only data assimilation. The estimated emissions differ in many regions if non-NO2 data assimilation is considered because the ratio of predicted NOx emission and NO2 column has been adjusted by non-NO2 observations. The assimilation of non-NO2 measurements leads to changes of up to about 70 % in the regional monthly-mean emissions. The estimated ten-year total regional emissions for South America and Australia are about 10 % lower in the multiple-species assimilation than in the NO2-only assimilation. The RMSE between the two estimates for the monthly total regional emissions is 15.5 % for central Africa, 16.5 % for Australia, and about 5-8 % for major polluted regions during the ten-year period. The estimated monthly mean emissions are mostly smaller in the multiple-species assimilation than in the NO2-only assimilation, especially over the tropical and southern subtropical regions such as South America, central Africa, and Australia, suggesting that NO2-only data assimilation tends to overcorrect the emissions from the a priori. The monthly total global emissions decrease by up to 6 TgN (in boreal summer) if non-NO2 data assimilation is considered. The ten-year linear trend is also different over most industrial areas (Table 4). For instance, the positive trend for India is 34.3 %/decade in the NO2-only assimilation, which is larger than the 29.2 %/decade in the multiple-species assimilation. For the United States, the negative trend is larger in the multiple-species assimilation (-29.4 %/decade) than in the NO2-only assimilation (-23.9 %/decade). These results confirm that the assimilation of measurements for species other than NO2 provides additional constraints on the NOx emissions over both anthropogenic and biomass burning regions."

Thirdly, as suggested by the reviewer, the model ozone low bias could be introduced by errors other than those for NOx emissions. To discuss this, the following sentences have been added to Section 5.1:

"Note that the emissions of O3 precursors other than NOx, such as VOCs, and various model processes in atmospheric transport and chemistry influences the model performance. The optimization of additional precursors emissions and the improvement of the forecast model could be important for improving O3 simulations, as discussed in our previous studies (Miyazaki et al., 2012b; 2015)."

2. Regarding the use of 3 different NO2 sensors, it is obvious (and I think the authors know) that the diurnal cycle alone cannot explain entirely the difference between NO2 columns from e.g. GOME-2 and OMI. And even if it would, it is also obvious that the diurnal cycle of NOx emissions is only one among many different processes affecting the diurnal cycle of NO2 columns. This article presents a smart but crude procedure to improve the match with the 3 sensors simultaneously in spite of their inconsistencies: additional control parameters are introduced which allow modifying the diurnal cycle of emissions at every model pixel. Unfortunately, the result is not much credible as it would imply much stronger rush hour emission peaks even in regions where mobile emissions (cars) are not the main NOx source category. Power plans, industries, etc. do not have peak activity around 8 AM. The most negative values of the Etc parameter (Fig. 13) are found in Inner Mongolia, which has only few cars but does have power plants. Even though the diurnal cycle adjustment serves its purpose, it is clearly artificial. The authors should provide a better explanation of why they choose this procedure. Maybe it is the only one which works since we don't really understand the reasons for the inconsistency between morning and afternoon sensors. More discussion is warranted.

I agree that the diurnal emission variation does not solely explain the simulated tropospheric NO2 column differences between the OMI and GOME-2 (or SCIAMACHY) overpass time. Because of model errors and differences between the satellite instruments, the obtained diurnal cycle adjustment can be artificial for some cases, as mentioned by the reviewer. The following sentences have been added to note this point more clearly:

In Section 3.2.1:

"Multiple satellite NO2 retrievals obtained at different overpass times have the potential to constrain diurnal emission variability (e.g., Lin et al., 2010), although differences between the different NO2 retrievals and errors in model processes could introduce artificial corrections (see also Section 5.2). Note that the retrievals from different instruments used are all based on the same retrieval method (DOMINO v2, TM4NO2A v2) and largely consistent ancillary data, which limits the discrepancies between the data sets to large degree (Boersma et al., 2008) (see Section 2.3.1). We also acknowledge that differences between the surface reflectivity and cloud data used may lead to some structural uncertainty between the morning and afternoon sensors, although numerous validation studies pointed out that the three NO2 column retrievals agree well with independent reference data (e.g., Irie et al., 2011; Ma et al., 2013)."

In Section 5.2:

"Thus, the model errors could artificially affect the diurnal emission variability."

Over Mongolia, the soil-type emission diurnal variability function was applied to the a priori emissions

with a maximum in the afternoon. Thus, the negative values of Etc may imply too much afternoon emission because of the applied a priori diurnal variability. To more precisely describe the applied diurnal emission variability function, the model description in Section 2.1 has been rewritten as follows:
"We applied anthropogenic-type diurnal variations for total emissions with maxima in morning and in evening with a factor of about 1.5 (black dotted line in Fig. 1, for which the daily mean hourly emission value is 1) in Europe, eastern China, South Korea, Japan, India, and North America; biomass burning-type variations with a rapid increase in morning and maximal emissions in the mid-day with a maximum factor of about 4 in North and central Africa, southeast Asia, and northern and central South America; and soil-type diurnal variations with maximal emissions in afternoon with a factor of about 1.2 in Australia, Sahara, western China, and Mongolia."

The relevant discussions in Section 5.2 have been rewritten as follows:
"The optimized Etc for biomass-burning and soil emission dominant regions are mostly slightly negative, which may suggest that the applied diurnal emission variability with an afternoon maximum (see Section 2.1) was misleading for some regions. In contrast, they are positive for most of the ocean. These results suggest the need to not only correct diurnal NO2 variations, but also account for the differences in the sampling and bias between OMI and other instruments as well as the influences of model errors."

3. Although the paper is already quite long, I would expect at least some comparisons with independent NO2 measurements. The reader has no clue regarding how the model performs for vertical profiles of NO2. Also, given the focus of the paper on the diurnal cycle, comparisons with ground-based remote sensing data could be useful. Therefore, although this article is clearly interesting and the methodology appears generally sound, I recommend that the authors try to address those main comments, as well as the other comments listed below, before it can be published in ACP.

Comparisons with independent airborne and ground-based lidar measurements have been added. The data used are described in Section 2.4, and the validation results of the vertical profile are shown in Fig. 4 and discussed in Section 3.4 of the revised manuscript. We will not discuss detailed diurnal variations of the simulated NO2 profiles, since this requires careful discussions and is beyond the scope of this paper.

Page 5, 1st full paragraph: I find odd to apply a unique diurnal cycle to all emissions in a given region, e.g. the anthropogenic-type cycle over Europe, eastern China, Japan, North America. This is strange. Why not make a weighted average based on the fractional a priori contribution of anthropogenic, biomass burning and soil emissions? The diurnal cycle in New York doesn't have to be the same as in Wyoming.

Considering the large uncertainty in the individual inventory, we decided to use the simple diurnal emission variability scheme based on the dominant emission category, as in other global model studies (e.g., Boersma et al., 2008; 2011). Nevertheless, we confirmed that this simple scheme leads to significant improvements on the model performance (Miyazaki et al., 2012a).

Page 5, 2nd paragraph: What is the vertical LNOx profile parameterization?

The following sentence has been added:
"The vertical profiles of the LNOx sources are determined on the basis of the C-shaped profile given by Pickering et al. (1998)."

Page 8, 1st full paragraph: Apparently the retrievals of Boersma et al. (2011, 2004) are used. But then the section goes on mentioning reduced errors based on Maasakkers (2013) even though the retrieval of Maasakkers is not used. This is difficult to understand.

The paragraph has been rewritten for easier understanding.

Page 10, lines 9-10: The assimilation effectively corrected the NO2 columns at the different overpass times. The complete diurnal cycle of NO2 concentration is an entirely different story. The model performance could be checked by comparisons with ground-based remote sensing data.

I agree with this comment. The sentence has been replaced by:
"Considering the short lifetime and rapid diurnal variation of biomass burning activity at low latitudes, these improvements suggest that the assimilation of multiple-species and multiple NO2 measurements effectively corrected the temporal changes in the tropospheric NO2 column between the different overpass times."
Comparisons with ground-based measurements have been added. Please see my reply above.

Page 10, line 13: Here and elsewhere, the observation errors for highly polluted cases are considered very large. But the relative errors there are often of the order of 25-35% which is generally less than at more remote locations. Of course during the winter, things are different due to large zenith angles, clouds, and snow.

We here discuss the absolute errors, which are larger over polluted areas than over remote areas.

Page 10, line 17: Some explanation on why the model lifetime of NOx would be too short would be useful.

The sentence has been rewritten as:
"The remaining errors may also result from model errors such as too short lifetime of NOx through the NO2+OH and NO+HO2 reactions and the reactive uptake of NO2 and N2O5 by aerosols (e.g., Lin et al., 2012b; Stavrakou et al. 2013)."

Page 12, on trends: The total number of observations changes during the 10-year period. Also the different satellites provided data during different periods. Some comments on possible consequences for trend estimation are needed.

The relevant sentence in Section 5.3 has been rewritten as follows:
"Discontinuities in the assimilated measurements (e.g., lack of most TES retrievals after 2010, OMI row anomaly since January 2009, and the limited data coverage of SCIAMACHY (before February 2012) and GOME-2 (after January 2007)) may also affect long-term emission estimates."

Page 12, lines 15-17: The emission factors are indeed uncertain, but so are also the biomass burnt estimates.

The sentence has been rewritten as:
"... large uncertainties in emission factors and biomass burnt estimates used in the inventories."

Page 12, lines 17-18: Note that the year-to-year variations over South America are very large.

To clarify the meaning and in response to a suggestion from another referee, the sentence has been rewritten as:
"The weak year-to-year variations in the a priori emissions are partly attributable to the use of climatology after 2011"

Page 12, lines 30-34: the increases aren't highest just over cities and lowest over remote areas. The entire N-E China and the Guangzhou area show large increases. The Chengdu/Chongqing area (with emission decreases) is certainly not "remote". Over N-E China, given the model resolution, it is not possible to distinguish urban from rural areas. Furthermore, Inner Mongolia shows large increases.

Thank you for this point. The sentence has been rewritten as:
"At the grid scale, the estimated emissions are higher than the a priori emissions over northern and eastern China, such as Beijing (+58 % at the nearest grid point), Tianjin (+97 %), Nanjing (+30 %), and around Guangzhou (+78 %), whereas they are lower around Chengdu and Chongqing (Fig. 10)."

Page 14, 1st full paragraph: The results regarding the trends in Europe are difficult to understand. Could you compare with previous studies for Europe, e.g. Curier et al. (Remote Sens. Environ. 2014, doi:10.1016/j.rse.2014.03.032)?

The following sentence has been added:
"Strong negative emission trends over these regions were similarly found by Curier et al. (2014) for 2005--2010."

From Figure 3, the OMI observations indicate a positive NO2 trend, whereas GOME-2 shows the opposite trend. Such difference cannot be due the diurnal cycle of NO2. Apparently those instruments have drifts which can be interpreted as emission trends. Please comment on this.

Firstly, as the estimated emission does not follow a simple linear trend, the comparison of linear trends between the three sensors estimated for different time periods is difficult. Secondly, the positive trend in OMI is partly attributed to very high concentrations in November 2011. We confirmed that the weather for November 2011 was unusual in Europe. For instance, in the Netherlands, it was the driest month in 100 years of measurements, with ten times less rainfall than the climatological mean. Strong high-pressure systems prevented rain from washing out the NO2 pollution. GOME-2 and SCIAMACHY do not reveal such high concentrations in November 2011, although the reason for this is unclear (the different sampling rates between the sensors could partly explain this). This difference can partly explain the different trend between the sensors. Thirdly, we point out that the trends (or year-to-year variations) found from the three sensors are mostly consistent, except for Europe. Further efforts are required to investigate the observed NO2 trends from the three sensors. However, this is beyond the scope of this paper.

Page 14, line 23 "The summertime peak enhancement is obvious over remote regions": Could you substantiate that claim?

The sentence has been rewritten as:

"The summertime peak enhancement is obvious over remote regions such as high-temperature agricultural land over the South Atlantic, the East South Central, and the Southwestern United States,..."

Page 15, line 10: Couldn't this be verified with e.g. MODIS fire counts or other biomass burning proxies?

The cited paper (Venkataraman et al., 2006) confirmed the fact using MODIS fire counts.

Page 15, line 27: Although temperature has some effect, the shorter NOx life times at tropical latitudes such as India are primarily due to higher photolysis rates and specific humidity.

The sentence has been rewritten as:
"In contrast, tropospheric NO2 columns over India are much lower compared to those in northern midlatitude polluted areas, as a result of the high values of temperature, photolysis rates, and specific humidity, leading to shorter NO2 lifetimes throughout the year (Beirle at al., 2011)."

Page 16, line 3: Why would high resolution analysis be required? This shouldn't be so complicated. For example, biomass burning has a distinct seasonality which can be probed at coarse resolution.

This sentence describes point sources (i.e., power plants) over Southern Africa. For such cases, high-resolution analysis is required to distinguish between different emissions sources, whereas coarse resolution models are known to underestimate NO2 concentration (Valin et al., 2011). To clarify these points, the sentence has been rewritten as:
"The various emission sources may have experienced different variations, and high resolution emission analysis is required to understand the detailed spatial variation in these emissions and to obtain unbiased emission estimates (Valin et al., 2011)."

Page 16, end of section 4.2.6. The high temporal correlation between N. Africa and Central Africa is interesting. Would this be related to biomass burning or to soil emissions (or both)? Examination of MODIS fire counts could help, also possibly temperature data. Is this correlation also found in the NO2-only assimilation?

A similar correlation is found in the NO2-only assimilation. This is indeed interesting. However, detailed analysis of the causal mechanisms for individual cases is beyond the scope of this study.

Page 19, first paragraph: Basically, the improved ozone is due to the general increase in NOx emissions over all regions, whereas the a priori model seems to have a negative bias in surface ozone. In the study of Travis et al. (ACPD, 2016, doi:10.5194/acp-2016-110), NOx emissions over the U.S. are found to be largely overestimated in comparisons with aircraft data.

We understand some models have a positive ozone bias, and these models may have NO2 biases that also differ from our results. The description in this paper is of the MIROC simulation results, and we do not attempt to generalize the implication of this to other models.

Page 19, last full paragraph: Could you also provide the global tropospheric chemical lifetime of methane (or methyl chloroform) in the model?

The methane lifetime was not calculated in the simulation.

Page 20, first sentence: "The inverse lifetime is expected to be proportional to the ratio of NOx to NO2". It's the other way around. Increase the NOx to NO2 ratio should increase the fraction of NO (which does not react with OH) and therefore decrease the sink of NOx, i.e. the inverse lifetime. The main effect of a NOx emission increase is (most often) increased OH levels and therefore shorter NOx lifetime. The point which is made in this paragraph is unclear.

To clarify the meaning, the paragraph has been rewritten as follows:
"To elucidate the changes in the NOx chemical lifetime, Table 6 compares the lower tropospheric OH concentration and the ratio of the regional mean surface NOx emissions and lower tropospheric NO2 concentrations (averaged from the surface to 790 hPa) between the multiple-species data assimilation and the model simulation (NOx-emi/NO2) in the boreal summer. It was confirmed that both the concentration assimilation (mainly TES O3 and MOPITT CO measurements) and the changes in surface NOx emissions lead to an increase in the OH concentration in the lower troposphere. Meanwhile, the increased ratio of NOx to NO2 (i.e. increased fraction of NO) in the multiple-species assimilation compared to the model simulation indicates that the HO2+NO reaction, which is the source of OH, is enhanced in the multiple-species assimilation. These results suggest that NOx chemical lifetime is decreased because of increased OH concentrations (through the NO2+OH reaction, which acts as the main sink of NOx) in the multiple-species data assimilation over most industrial and biomass burning areas, and demonstrate the utility of the multiple-species assimilation to constrain the tropospheric chemistry (i.e. chemical regime) controlling NOx variations and to improve surface NOx emission inversions."

Page 20, lines 20-25: The large adjustments are first said to suggest a change in diurnal evolution of the emissions. Then they are said to suggest other possible causes related to the model or the retrievals. Correct, but then the first suggestion is not necessary. Values of Etc as negative as -0.6 or -0.8 are found at some locations, which are impossibly large. Large Etc should be found only in areas where traffic is the dominant source. This does not appear to be the case (Fig. 13).

I understand that some results are difficult to understand based on a change in the diurnal evolution of the emissions, but both possibilities remain valid. We cannot ignore either of them, without any evidence. Therefore, we believe that the current statement is reasonable. Please also see our reply above.

Page 22, line 4: Is the given observed NO2 concentration trend for OMI or for all sensors? The trend appears very different between GOME-2 and OMI.

Yes, the statement is based on the OMI observations, which is described in the revised manuscript. As shown by the black straight line in Fig. 3, the trend is different between the sensors. To explain this, the following sentence has been added. Please also see my reply above.
"Note that the linear trend in the observed concentration is different between the instruments over Europe (c.f., Fig. 3)."

Page 22, line 6: Why would NO2 have become more long-lived? Does OH show a negative trend in this region? If so, what are the causes for this trend? Note that the fraction of NO2 to NOx is determined mostly by ozone and the photolysis rate of NO2. A shift in NO2:NOx emission ratio does not matter much except directly over emission areas (titration effect). The paragraph seems to imply that the NO2:NOx emission ratio in the model has changed over the 10-year period. Is this true?

This paragraph describes how the NO2 concentration/NOx emission ratio has changed over the ten-year period over Europe. OH trends and the causal mechanisms are not discussed in the manuscript, since there could be many factors affecting them, as suggested by the reviewer. Therefore, as described in the manuscript, further efforts will be required to explain these mechanisms (in a separate study).

Technical corrections

Page 1: "Forkert" should be "Folkert"

Corrected.

p. 1 l. 6: "biased" should be "biases"

Corrected.

p.1 l. 8: "the development" : do you mean the evolution?
Yes, replaced.

p. 2 l. 2: "traffic rush hours, economic activity. . ." those are not "source categories". Sentence is confusing, please rephrase.

The sentence has been replaced by:
"Examples include traffic rush hours, economic activity, biomass-burning activity, wintertime-heating of buildings, and rain-induced emission pulses of NOx".

p. 2 l. 25: Kalam should be Kalman

Corrected.

p. 2 l. 33: insert a hyphen between "multi" and "constituent" same line: replace advancement by advance or progress

Corrected.

p. 3, l. 21-22: The sentence "The OH magnitude and gradient is the primary chemical pathway for propagating observational information..." does not make much sense. Rephrase or delete.

The sentence has been rewritten as follow:
"The changes in OH are the important chemical pathway for propagating observational information between various species and for modulating the chemical lifetimes among these species."

p. 3 l. 31: Replace maybe "an EnKF technique" by "a variant of the EnKF technique"

Replaced.

p. 4 l. 25: Explain "background spread"

The sentence has been replaced by
"...of background error covariance in the stratosphere, as estimated from ensemble model simulations,.."

p. 4 l. 30: Isn't Yienger and Levy (1995) the correct reference for GEIA NOx? Please check.

Corrected.

p. 6 lines 12, 16, 22: I think "Ets" should be "Etc"

Corrected.

p. 6 l. 31: GOME-2 (not GOME-II)

Corrected.

p. 10 l. 24: Here and at other instances, replace "c.f." by more standard phrasing (e.g. "see")

Corrected.

p. 11 l. 12: the sentence seems to imply that the chemical lifetime of NOx might be underestimated, which is not what you mean here. Rephrase.

The words "(and/or chemical lifetime of NOx)" have been removed.

p. 11 l. 20: I suppose you mean GFED 3 here, not EDGAR 4.2 (see section 2.1)

Corrected.

p. 12 l. 9: "southern parts of the Eurasian continent" : don't you simply mean China? The seasonal variation over Southeast Asia does not show a summer maximum, so it does not fit into the point made in this sentence.

Corrected.

p. 12 l. 18 "assumptions applied for the a priori emissions" I think you could be more specific (use of climatology after 2011)

Corrected.

p. 12 l. 27: "The EDGAR v4 emissions are too low": that statement is too blunt for several reasons. Replace "EDGAR v4" by "our a priori inventory" (since EDGAR for 2008 is used after 2008, and since soil emissions are not from EDGAR). Furthermore, add something like "Our assimilation indicates that. . .".

Replaced by "Out a priori inventory is..."

p. 12 l. 28: "too low by a factor of 0.6": awkward. Should be too low by a factor of 1/0.6 (i.e. about 1.7)

Replaced by "by about 40 %".

p.12 l. 29: emissions are maximum in June, not July.

Replaced.

p. 13 l. 34: "in the reported mobile emissions": why specifically in this source category?

The sentence has been removed.

p. 14 l. 1: replace "reveal" by "show"

Replaced.

p. 14 l. 2: replace "by" by "after"

Replaced.

p. 14, l. 18: "around Atlanta (...) and Denver": this seems to indicate that increments are found mostly over cities, which is not true. Consider replacing by "Southeast US and most of Western US"

Replaced by “the Southeast United States (e.g., +23 % near Atlanta) and most of the Western United States (e.g., +26 % near Denver),”

p. 14 l. 19: delete “over” after “around”

Removed.

p. 14 l. 20: Los Angeles

Corrected.

p. 15 l. 9: I think the word “boreal” is superfluous here (and at many other instances in the text)

Removed.

p. 15 l. 20-21: “particularly strong increase around Delhi” but the changes over Delhi are lower than the regional average!

Delhi has been removed from this statement.

p. 16 l. 5: Replace “by data assimilation” by “due to data assimilation”

Replaced

p. 16 l. 33: replace “reflection” by “reflecting” same line: replace “when” by “whereas”

Replaced.

p. 19 l. 17: LNOx (instead of LNO)

Corrected.

p. 19 l. 34: here and elsewhere in the manuscript, insert hyphen between “multiple” and “species”

Corrected.

p. 20 l. 17: Replace "rom the three. . ." by "constrained by the three. . ."

Replaced.

p. 21 l. 8: "using either model after data assimilation" : awkward, the model is used for data assimilation

Replaced by "either model and data assimilation".

p. 32: Table 2 and elsewhere: Replace "Australis" by "Australia"

Corrected.

p. 41 Figure 6: it is impossible to distinguish black and dark blue on the "South America" plot. Consider using other colors.

Changed.

p. 44, Figure 9: the title of the middle panels should be "A posteriori – A priori". Same for the title of the right panels, the minus sign is missing.

Corrected.