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Interactive comment on “Analysis of emission data from global commercial aviation: 2004 and 2006” by J. T. Wilkerson et al.

Anonymous Referee #2

Received and published: 21 April 2010

Overall referee position:

In principle this is a valuable contribution to the science community, but the change in methodology between the two years without quantifying the impact of this change does not allow for a one-by-one comparison of these two years, and even less to derive regional growth rates from these data. This general approach is not convincing and even more other data (OAG, IEA) do not support the regional growth rates found within this study. The manuscript can be published after major revision and a second review, without these major changes I do not support the publication of this manuscript in the actual status.

Review Criteria

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1. Does the paper address relevant scientific questions within the scope of ACP?

A new air traffic database for two recent years is a valuable contribution to the scientific community, particularly with a temporal resolution of only 1 hour and without extrapolating data of only a few days to a whole year.

2. Does the paper present novel concepts, ideas, tools, or data?

The high temporal resolution of the data is unique in comparison with other air traffic inventories, furthermore, the employment of very detailed data, abandoning the need of extrapolation from a few days to one year.

For the 2006 air traffic inventory a new source of flight track data is included in addition to 2004 basic data, but it is not explained, whether these represent more data(flights) or only a different degree of detail. It is not very clear, why two years of data (2004 and 2006) are shown, as one should not compare the two years one-by-one anyway, as there have been plenty of changes in the methodology and in the data sources included.

Furthermore, the artificial over-count of flights in 2004, which is mentioned in section 3.4 and illustrated in Figure 7a makes the comparison of the two years even more questionable. The artificial over-count of flights in 2004 seems to reach about 10% more flights in all months between April and October (see Figure 7a). If the data were corrected from this “over-count”, maybe the two years were better comparable. And if for example the same trajectory generation between origination and destination were used for both years (statistical distribution vs. known airways and great circles).

I highly suspect the “decrease” of air traffic emissions and flown distances from 2004 to 2006 is only due to the many changes within the methodology of compiling the air traffic inventories. Therefore I would either make the two datasets most comparable as possible or I would refrain from comparing the two datasets and interpreting possible artefacts. Other reliable data sources do not report a decrease of air traffic (or aviation

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fuel consumption) from 2004 to 2006, e.g. the IEA (2009) reports a global increase of CO₂ emissions from international aviation bunkers from 368.07 (2004) to 400.16 (2006, the OAG (www.oagaviation.com) also reports an increase of the number of seats and the number of operations worldwide from 2004 to 2006. Such numbers must be revealed in this article and the findings of this paper can only then be discussed and set in context.

3. Are substantial conclusions reached?

The points given in the summary section which are related to regional increase or decrease, or changes in the vertical distribution of flights or average flight length, are questionable findings and may only be caused by the changes in the methodology used. If one aims to compare regional air traffic density of different years to derive regional growth rates, the methodology used for the compilation of the data must be as similar as possible.

As explained above, I highly suspect the “decrease” between 2004 and 2006 to be an artefact caused by too many changes within the methodology, and I highly recommend these issues to be clarified, before the manuscript is published. Because of the changes of data additionally incorporated and differences in the compilation of routes between 2004 and 2006, I do not trust the comparisons of regional changes between the two years, what the authors interpret maybe caused by regional growth rates but may also well be caused by different data sources, different generation of trajectories.

The findings with respect to regional growth rates are not in agreement with data from OAG between 2004 and 2006, where an increase of the number of seats and the number of flights is found between 2004 and 2006 for all regions except for flights within North America, but even for flights from and to North America an increase is found from 2004 to 2006.

4. Are the scientific methods and assumptions valid and clearly outlined?

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Why are so many changes in the methodology employed between the two years, without quantifying the differences?

Why are different EI given in Table 1 for 2004 and 2006, without discussing the change? And even more, NOx, SOx, etc. is not shown for 2004 anyway. It is not clear, why the EI of PM is changed in such an extreme way between 2004 and 2006. Please give reasons for this change, I don't believe the emissions or the fuels have changed so much without any reliable statement. Please do not only give "personal communication", but discuss the chosen values in context of values given in the literature...

5. Are the results sufficient to support the interpretations and conclusions?

No. See above.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

The description of the changes of the methodology between 2004 and 2006 should be more detailed. Furthermore a quantification of the changes of methodology would be helpful. Is it possible to calculate the 2006 data set twice, once with the old and once with the new methodology? Or maybe a correction of the 2004 data ("artificial overcount") could then enable the authors to make a more reliable comparison between the two years?

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Other air traffic inventories in addition to the SAGE inventory should be mentioned, too, e.g. TRADEOFF, AERO2K, QUANTIFY.

It is not very clear, what the difference in fuel burn between 2004 (SAGE) and 2004 (Wilkerson) causes. The authors only very vaguely state, that they included an improved estimation of unscheduled flights. Maybe this could be explained a bit more detailed.

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8. Does the title clearly reflect the contents of the paper?

Ok. But the title depends on the final version of the paper. If focus is on regions, this could be mentioned in the title, if focus is on e.g. improved methodology, also.

9. Does the abstract provide a concise and complete summary?

Ok for the moment, but the abstract says nothing about regional differences, although this subject fills 3 pages of the article, 5 tables and 1 figure.

10. Is the overall presentation well structured and clear?

Yes ok, if other points are resolved.

11. Is the language fluent and precise?

Generally ok. Only some points (see below).

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Clarification: Please quantify the differences between 2004 and 2006 and what part is due to “fewer flights” and what due to methodology used.

Clarification: Figure 2, regional differences between 2004 and 2006 should be discussed in more detail (maybe a difference plot would be helpful) and other information should be consulted to support the statements (e.g. OAG, IEA, to prove why here or there more or less flights in 2006 compared with 2004).

Reduction/Elimination:

Table 1: Emission indices for 2004 not necessary, if the changes are not discussed.

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Figure 6 not really necessary. Figure 3 and Figure 9 may also be not necessary.

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Table 9 not necessary.

14. Are the number and quality of references appropriate?

See Point 7.

15. Is the amount and quality of supplementary material appropriate?

Ok.

Further points:

For the whole document: If rounded numbers are presented in the table, please use the same numbers in the text or use the numbers discussed in the text also in the tables. And please refer more often and more detailed to numbers in the tables (e.g. column...)

Page 2946

Line 16: Give percentage for dominant regions.

Line 17: Quantify the impact of the methodology change between 2004 and 2006.

Line 25: Be more specific: CO2 emissions

Page 2947

Line 9: Per what? Per year? Related to what?

Page 2948

Line 22: ... as much measured data as possible: Too vague, and what is meant by "measured"?

Line 24: ... data from several sources...: What sources?

Line 25: ... radar coverage...: Only US?

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Line 25: Military later excluded?

Line 26: ...captures every scheduled flight...

Page 2949

Line 2: ... records and estimated 50-60 percent...: How is this estimated?

Line 3: Please clarify: Does this mean additional flights in 2006 or just more detailed data with additional information?

Line 11: How do you know ETMS is incomplete?

Line 12: ... statistical distribution...: Please be more precise: according to ac-type, according to flight distance?

Line 20: ... then a GC track was used...: How often does this happen? Please quantify. Why don't you apply the same statistical distribution in 2006 as in 2004? Or why has GC method not been used in 2004?

Line 26 ff: Meaningless and dispensable sentence.

Page 2950:

Line1: Say a few words about temporal resolution of base data also in this section.

Line 11: Why are cancelled flights taken into account?

Line 11: 9 Percent: How is this number estimated?

Line 11: Unscheduled flights are regionally very different, how is the scaling done, are regional differences included?

Line 22 ff: Discuss with other PM EI from peer-reviewed literature. Give reasons for the "radical" change of PM EI.

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Line 2: "If" instead of "While"

Line 13: "emissions species" instead of "chemical"

Page 2953

Line 4: In view of other reliable data sources (OAG, IEA) I question whether the decrease between 2004 and 2006 is possibly only due to the change of the methodology and not by less flights.

Page 2954

Line 9 ff: How is the usage of another EI (e.g. BC) justified? Please clarify.

Line 20: "enhancements": Please be more precise.

Line 26: "improved estimation of unscheduled flights": very vague, please be more precise.

Page 2957

Line 1: ... decreased ... (please refer to Table 7)

Line 2ff: ... increase in short-haul or local flights...: Not convincing, maybe much more caused by changes in methodology.

Line 28: ... the total US region on average falls second... to Europe, whereas over the Eastern US the highest emissions per unit area are found.

Page 2958

Line 6: ... third of emissions compared with the US box region...

Line 26: Sentence makes not really sense. CO2 emissions are always spreaded uniformly because of their long lifetime. Maybe this could be clarified by comparing the air traffic density or the fuel consumption. But Figure 6 is not really necessary anyway.

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Line 1ff: Misleading, as transport of emissions is not investigated within this study.

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Line 5ff: Please refer to Table 8.

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Line 6: ... similar to 2004 at 318 kg/km²... : no, not similar, almost 10% difference to 2004 values of 347.

Line 25: Please refer to Figure 7a. How much does the over-count make up? In Figure 7a it looks like about 10% off-set. What is the reason for this over-count? Why does it not happen in 2006? Is it possible to correct the 2004 data? Have you looked at regional differences of this over-count? Maybe interesting / important if a correction is envisaged!

Page 2960

Line 20 ff: very meaningless section. If you don't write more about this and set this in context to the rest of the text and other air traffic inventories, this section can be discarded.

Page 2961

Line 6: ... may be in large part due to the difference in horizontal track generation . . . : Please quantify to what extent caused by difference in methodology.

Line 12: . . . in most regions. . . a small decline or very small increase.

Line 20f: Quantify differences in height distribution caused by methodology.

Table 1: Why EI changed between 2004 and 2006? NOx, SOx, etc is not shown for 2004 anyway.

Table 4: Please discuss absolute numbers also in comparison with other emission inventories (e.g. AERO2K, TRADEOFF, QUANTIFY). Potentially this table could be reduced to distance, fuel and number of flights, as the other species are not discussed and are only dependent on EI and fuel consumption.

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Table 6: If shorter trajectories are calculated for 2006 due to differences in the methodology, it is no wonder, the average flight length is shorter. Please quantify the differences in methodology. I don't think this is a statistically significant difference in flight length. For short-haul flights and total flights the difference is of the order of 1% only...

In Table 6 write "Average" instead of Avearge"

Table 7: Headings of last two columns are not in right position.

Figure 2: Please discuss the differences between 2004 and 2006 more detailed in the text. Maybe difference plot would be helpful. What happened at southern hemisphere, Australia, Asian Continent, northwards of Russia: Why such a large reduction there? Please discuss and present reasons for decrease...

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 2945, 2010.

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