The manuscript uses a large number of ground based and aircraft data to evaluate the EMAC methane simulations and to attribute the observed methane trends to changes in specific emission changes.

However, the manuscript lacks of clarity in terms of clear hypothesis, key message and associated uncertainties. The added value compared to earlier studies has to be clearly stated.

The abstract needs to be restructured: 1. Key question, 2. Use of EMAC and observations, 3. Assumptions made for modeling, 4. Key results and added value compared to earlier studies (to what the no-trend period is due and to what the trend-period). Also it misses some quantitative information on the findings, e.g. (lines 28-30) by how much fossil fuel is reduced and tropical wetlands and rice paddies are increased in the posteriori emissions?

Furthermore, there are a number of hypotheses made for this work that require to be clearly spelled out, justified and discussed with regard to the uncertainty they introduce to the conclusions of this study (as also pointed by the other reviewer). This has to be done early in the paper in section 2 where the model set up is explained. An appropriate place would be section 2.2., which could be 'Model set up and assumptions made'. These assumptions are mainly:

1. the no-interannual variability of OH radical and thus the use of a prescribed OH radical concentration, which also implies no chemical feedbacks and linearity in the chemical destruction of CH_4. The use of the prescribed OH is mentioned in the model set up while references that can partially support such choice are coming later in page 12 (lines 440-444). No discussion is really made on the uncertainty introduced in the results from such assumption.
2. the constant interannual CH_4 emissions for the a priori scenario
3. testing different changes in specific sources emission intensity but assuming the constant geographic distribution per source.
4. Neglecting a change in the soil sink? When optimizing for the emissions (or I misunderstood?)

The model description is also unclear at several places. For instance

1. How the prescribed OH field has been derived (line 146 or later on line 212)?
2. How the steady-state global CH_4 simulation for the years 1997-2006 has been performed? (lines 152-153
3. How many and which online simulations of EMAC have been performed ?
4. And how many and which 'solver' fits have been performed?
5. What kind of constrains are imposed to the 'Solver'
    a. which constrains are exactly used in this study?
    b. for which case studies is the Solver used? (A Table could be very illustrative for this).
    c. As currently written, the reader has the impression that multiple (online) EMAC simulations have been performed and the figures presenting the model versus observations show the results of such simulations.

d. In lines 321-324 you discuss tolerance intervals; which are used for the various sources studied here?

6. The tagged simulations (are online simulations?) have to be described in section 2 and not refer to them mainly in section 4 (results). Also give the simple equation (sum of tagged species) to explain how you calculate the total CH4 concentration.

In addition, there are several repetitions throughout the manuscript that have to be removed.

At several places text from other publications is used with quotation marks, for instance lines 82-92, or without, for instance lines 441-443. Such text has to be re-written in author's proper words with appropriate reference to the original study.

Furthermore, there are several long sentences difficult for the reader that can be broken down in 2 or 3 shorter ones and increase readability of the text (for instance lines 21-24).

More specific comments.

1. Line: 13: what means 'in specified dynamics mode'? Is this comment needed?
2. Lines 32- 35: '…added to the posteriori no-trend period emission distribution' rephrase.
3. Lines 37-38: I suggest rephrasing starting by ' A combination of these sources that is statistically most likely excludes ….
4. Line 39: This is not a sentence for the abstract. It can be used in the conclusions.
5. Line 47: 'CH4 variability' do you mean 'CH4 mixing ratios'?
6. Line 60: forcing of CH4 is… (0.6 Wm-2 instead of 0.5Wm-2)
7. Line62: slowed down for about 8 years until sources and sinks quasi balanced
8. Line 65: Please rephrase the last part of the sentence. It could be like the following: This reveals a period without-trend from 2000 through 2006 and one with increasing trend afterwards, which steepens after 2014.
9. Line 73: 'the increase in CH4 since 2007' until when?
10. Lines 96-103: Model evaluation is only one part of the study presented and is not enough for an ACP paper, although a very large dataset is used for this evaluation including the large number of samples collected during the CARIBIC flights. The suggested attribution of observed CH4 trends to potential emission changes seems an additional objective which increases the value of the study and has to be explicitly mentioned here.
11. Lines 154-156 should move once the need for the use of the Solver is expressed i.e. line 158 before the beginning of the sentence.
12. Line 162: 2.3.1 Methane a priori emissions
13. Lines 181-183 contradict with lines 176-177, please rephrase to make it coherent.
14. Lines 187-188 and 194-196 are results of this study (if I understand it correctly), why they are presented in the model -set-up section?
15. Lines 200-202: Why the deposition velocity of CH4 would depend on CH4 mixing ratio? Do you mean deposition flux?

16. Line 208: ‘photolysis and chemical reaction system’ change to ‘photochemical reactions system’
17. Line 214: global distribution of tagged methane or total?
18. Line 215 ‘model samples’ this deserves a bit of explanation because is adding value to the comparison of model results with observations.
19. Line 251: from January 1997 (?) up to December 2016 … online model samples…
20. Line 254 ‘slowing increase’ please rephrase
21. Line 262: For the period after 2007…
22. Line 268: what you mean with ‘biogenic agricultural’ ?
23. Lines 279- 281 – repetition with lines 256-260
24. Lines 284-287: move to data section 3.
25. Line 291: all source categories (?)
26. Line 298-299: repeated 3rd time here.
27. Line 309: refer to figure 3
28. Page 9: (1st and 3rd paragraph) Make clear how you calculate the interhemispheric difference and how much is in the a priori and how much is in the posteriori simulations.
29. Line 333:’ all station’ does this mean all (daily?) data from all studied surface stations? Please specify.
30. Lines 343-344. Please rephrase for clarity, as is appears wrong to me.
31. Line 355: do you mean ‘assuming that the total sink if unchanged’
32. Line 364: CH4 appears to ..
33. Lines 370-371. Move the sentence before ‘however’ in line 372.
34. Line 374: upper troposphere
35. Lines 376-378: this is also true for the horizontal scale, please rephrase
36. Lines 375-376 and 381 have repetitions.
37. Lines 381-383: Please rephrase for clarity.
38. Line 404. Start new sentence with ‘Turner et al.’
39. Line 414: ‘same’ with what?
40. Line 417: …performed with EMAC (?) with these sources…
41. Page 12: TRO,SHA, FAE better spell out – it is very hard to follow since the reader needs remember all these acronyms.
42. Lines 440-447: this argues in favor of neglecting the interannual variability of OH. Should be moved very early in the paper where presenting the overall assumptions made and arguing for them (see major comments). Also provide information on how much interannual variability is found in these studies.
43. Lines 458: ‘additional a priori emissions’ how they are derived?
44. Lines 460, 466: spell out ΔNH/SH
45. Lines 462: which are the four combinations and how they are chosen?
46. Lines 470-473: rephrase for clarity. ‘Indications about the role of fossil…’ to what?
47. Conclusions: here it is expected to present your finding with a critical view, i.e. provide also some information on the uncertainties associated with them.

I also recall the relevant comments of the last review on the earlier version of your manuscript, which seem not to be taken into account for the current revision and have to be addressed.

1- Conclusion: " the presentation of the conclusions and outlook should better summarize the real conclusions (and limitations) of the study. E.g. the discussion of the "2nd order polynomial extrapolation predicts steady state after 13 years" seems rather hypothetical and the statement " NOAA/AGAGE station methane data are updated annually so further updates are expected" rather trivial." Please correct accordingly.

2 '"Enhanced precipitation in the regional summer season (Nisbet et al., 2016; Bergamaschi et al., 2013) may be a possible cause of growing tropical wetland emissions". The reviewer mentioned that: 'none of the 2 cited papers analyzed in any detail the tropical precipitation patterns, nor do Zimmermann et al. present any own meteorological analyses. While some studies in the literature found some correlations between tropical wetland CH4 emissions and ENSO induced anomalies in precipitation [e.g. Pandey at al., 2017], the reported anomalies appear directly related to the ENSO patterns (with a typical duration in the order of 1 year) - but to my knowledge do not support any persistent increase over the entire 2007-2016 period.'' So please rephrase that sentence accordingly (lines 407-408).

3. '"Kirschke et al. (2013) and Turner et al. (2016), however, found that an increase by 17-22 Tg/y could explain the renewed methane growth and 30-60% of this could be attributed to increasing U.S. anthropogenic methane emissions, which supports our results with 20.70 Tg/y emission increase including 8.38 Tg/y", but do not mention that the results of Turner et al. (2016) have been questioned by Bruhwiler et al [2017], highlighting in particular several methodical issues in the Turner et al. (2016) paper. In general the discussion of the results should be put much more in context with the existing literature."

I do not see how this point of the reviewer has been addressed in the current version. (lines 404-406).

4. Furthermore the reviewer states:

'A further issue of the paper is that the applied optimization technique is rather simple. In principle such simple techniques (with a very limited number of parameters) can be useful for quick analyses and for illustrative purposes. However, this should be put into context with more sophisticated inverse modelling techniques and should include a discussion of the limitations of these simple techniques. The optimization technique used in this study is rather similar to simple synthesis inversion techniques, typically used ~20 year ago (e.g. [Hein et al., 1997]) - using fixed global spatial and temporal emission distribution patterns. A very critical issue of these synthesis inversions, however, is that they are prone to the so-called aggregation error [Kaminkski et al., 2001].'

I think that for clarity a comment in this direction is needed, i.e. that the applied optimization technique is rather simple, but can be useful for quick analyses and for illustrative purposes. However, we need to keep in mind that this type of inversions is prone to the so-called aggregation error [Kaminkski et al., 2001].