Anonymous Referee #2

General comments

This manuscript reports a new data set of isoprene depth profiles alongside supporting data from the Pacific and Indian Oceans, which is subsequently analysed for production and loss rates in the mixed layer. On the whole, the data presented in this work is a valuable addition to the existing global isoprene data set, along with the analysis of the results in a novel approach, with relevant supporting data to investigate suggested relationships, and fits into the scope of the journal.

A comparison with available literature parameterisations is made, with the valid conclusion that none are currently adequate for global predictions. To consolidate essentially bottom-up and top-down production rates based on literature, the authors calculate a new field-based production rate, and subsequently suggest that a further adjustment from a significant and variable biological loss is needed to explain their isoprene observations. The analysis of the new data does not produce significant, quantitative correlations, but some interesting qualitative comparisons to several environmental variables appear to support the assignments to stress-related production and to losses to heterotrophic respiration.

The conclusions suggest investigation of different avenues which would add new insights into processes at various levels (semi-qualitative for heterotrophic respiration with large natural variability, quantitative for air-sea gas exchange losses) as well as repeating existing hypotheses supported by the new data analysis (environmental factors affect isoprene production).

We thank referee #2 for the helpful suggestions and comments. We will address the comments in the following (bold). The lines refer to the originally uploaded manuscript.

Specific comments (major)

Line 113: Did you test for matrix effect/purge efficiency differences between MilliQ and seawater?

- **Yes, we did purge efficiency tests with seawater and MilliQ and can confirm that the purge time and purge flow rate we used are sufficient to remove total amount of dissolved isoprene from our samples.**

Line 177: Were detailed light intensities (and light cycle timings) available and comparable for all literature values? How did the authors account for potential effects of temperature variations (and growth stage) between studies?

- **All references for the values we used provided a detailed light intensity description, as well as a light cycle timing, which we used to convert daily rates into hourly rates or vice versa. Shaw et al. (2003) and Exton et al. (2013) used a 14 h light and 10 h dark-cycle, whereas Bonsang et al. (2010) used a 12 h light and 12 h dark-cycle. The phytoplankton cultures from the different studies were reported as being in exponential growth stage. The potential effect of temperature variations was not considered and is discussed in answer #1 in response to referee #1.**

Line 336-341/Table 3: Double-check literature values for Prochlorococcus and diatoms are correct (should exclude Arnold et al., 2009, as described in Hackenberg et al., 2017). The difference between

diatom Pcalc and literature is rather large, but both are described as "low". Prochlorococcus are in fact within a similar low range, using Shaw et al. (2003) production rates.

- **In fact, we did not use the isoprene production rate for *Prochlorococcus* from Arnold et al. (2009) in our calculations (see reference for *Prochlorococcus* in Table 2) but forgot to exclude this value for comparison in Table 3. We changed the value in Table 3 from 9.66 to 1.5 µmol (g chl-a)$^{-1}$ day^{-1}, which is in a good agreement with our field derived calculated isoprene production rates for SPACES and OASIS. Accordingly, we changed the sentence starting on line 336 to: "During SPACES/OASIS the $P_{chloronew}$ values of *Prochlorococcus* (both 0.5 µmol (g chl-a)$^{-1}$ day^{-1}) are slightly lower but in a good agreement with the mean literature value (1.5 µmol (g chl-a)$^{-1}$ day^{-1}, Table 3), whereas…"**
  The literature value for diatoms is also changed (in Table 3 and line 340) from 2.54 to 2.51 µmol (g chl-a)$^{-1}$ day^{-1} by excluding Arnold et al. (2009) from average literature isoprene production rate of diatoms.

Line 372: Are mean radiation values for ASTRA-OMZ equator, as opposed to the lower mean values described for open ocean and coastal regimes in the next sentence (Fig 6 suggests yes)? Also, the global radiation for those two is lower than SPACES, but Pchloronew is higher for both, which is qualitatively consistent within ASTRA-OMZ, but not with the previous description across all cruises - this could perhaps be worded more clearly, e.g. line 373 "production rate was lower than around the equator".

- **We changed the sentences to: "Highest mean values were measured during ASTRA-OMZ (at equator, ~508 W m^{-2})…the isoprene production rate was lower than around the equator (mean global radiation decreased to ~310 W m^{-2})."**

Line 381: A caveat (transfer of dependence from diatoms to haptophytes) has already been noted by the authors, but it may also be worth considering that temperature effects may be just as variable as light effects between different species and hence also PFTs (cf. reference to Srikanta Dani, 2017, line 353).

- **We added the following sentence at line 382 etc.: "Additionally, as mentioned before, the temperature, as well as the light dependence of isoprene production might vary between different species of haptophytes when comparing different ocean regimes."**

Line 430: Would stations where a loss term was not needed not still represent part of the range of required potential additional loss terms, so that they should be included in the averages? Line 443: Both OASIS and ASTRA-OMZ open ocean kAS are 0.1 day-1, while the loss rates are 0.05 day-1 for SPACES and 0.15 day-1 for ASTRA-OMZ - why are SPACES and OASIS considered more comparable to kconsumption than the others?

- **We assume that isoprene production by phytoplankton is the only source for isoprene in the water column. To date, we do not exactly know all different processes of isoprene production/consumption, so there could be other production and loss processes that are not included yet, but would balance each other out. We only used those stations where a loss was needed mathematically, in order to assess loss processes where we expected a large signal. We realize a more thorough assessment would need an iterative approach**

between sources and sinks. However, we focused here on getting a more basic understanding of the important loss processes in the field and we hope to investigate these loss processes in more detail in the future.
We thank the referee for pointing out this mistake in comparing k values and we changed the sentence starting at line 441 to: "…resulting in a lifetime of isoprene of only 10 days, which is comparable to the lifetime due to air sea gas exchange during ASTRA-OMZ (open ocean) and OASIS."

Line 486: Has the effect of salinity been shown before? Could describe that stress (from light and temperature) has also been shown to be a factor.

- **To our knowledge, the possible salinity stress of phytoplankton to produce isoprene has not been shown before. In addition we changed the sentence starting at line 488:" The results confirm findings from previous laboratory studies that the isoprene production is influenced by light and ocean temperature, due to stress, and nutrients, due to their effect on changing phytoplankton communities and their abundances (e.g. Dani and Loreto, 2017;Shaw et al., 2010). Moreover, our data leads to the conclusion that isoprene production rates in the field, irrespective of phytoplankton communities and their abundance, are influenced by salinity and nutrient levels, which has never been shown before."**

Line 497: It is (almost?) impossible to exactly know all the different processes, as there are so many different factors and variations, e.g. just the number of phytoplankton and bacteria species and their exact distribution in the ocean at any one time. Our understanding of global marine isoprene cycling depends on a better knowledge of the involved systems and processes, but I hope that we can make significant progress even without exact knowledge... (The statement also suggests that knowing processes for PFTs in general may not be sufficient, as large variations within PFTs do occur – in contrast to the use of average rates in this manuscript.)

- **We absolutely agree with this statement. However, in this study we could show in the field that, even using average rates, temperature has an effect on the production rates. This is also partially discussed in our answer #1 in response to referee #1. We often caution the reader about possible uncertainties, like large variations of isoprene production within the PFTs (e.g. lines 64, 346, 392), which we still are not able to implement correctly when modelling oceanic isoprene concentration. However, trends and qualitative correlations in the field can already be concluded (and support laboratory studies), without knowing every rate exactly, which will hopefully help to further understand global marine isoprene cycling.**

Line 495 etc: What is the authors' view on the relative importance of uncertainty due to variations within PFTs compared to air-sea gas exchange? The large variation for haptophytes, for example, is much larger than differences in kAS . As a result, could the suggested missing sink not also be explained at least partially by the presence of a much lower-producing species of haptophytes?

- **The calculated emission factor for haptophytes was derived from three different laboratory studies, using with four different species within the group of haptophytes cultivated under three different light levels and temperatures (Figure S3, Table 2). We think that is a good example for the variation of isoprene production under different environmental conditions**

within one group of PFT. The uncertainty of this emission factor (error of log squared fit) is ~56%, hence also for the P_{direct} value. The uncertainty (standard deviation) of k_{AS} using three different parameterizations is dependent on the wind speed and is 10-15% in a wind speed regime of 8-12 m s^{-1} and can be 30% and higher at wind speed >15 m s^{-1}. Applying 15% uncertainty to the loss due to air-sea-gas-exchange (average: 2.88 pmol L^{-1} day^{-1}) and 56% uncertainty the production by haptophytes (average: 0.89 pmol L^{-1} day^{-1}) yields in an absolute error of 0.43 pmol L^{-1} day^{-1} and 0.50 pmol L^{-1} day^{-1} for the loss due to air-sea-gas-exchange and the production by haptophytes, respectively. As these two losses are both in the same range and following this approach and assuming that 56% uncertainty can be applied to all PFTs (and not only haptophytes) by using P_{direct} it may be possible that the large variations within one PFT could account for the missing sink.

However, we computed P_{need} values based on isoprene measurements, which allows us to disregard the uncertainties on P_{direct}. The resulting chl-a normalized isoprene production rates ($P_{chloronew}$) where highly variable among PFT (e.g. haptophytes) depending on the ocean region (Table 3). We hypothesize that these variations already reflect the influence of light, temperature, salinity, and nutrients. Hence, the uncertainty of the newly derived rates should be less than 56% (error of light dependent log squared fits from different laboratory studies using different temperatures and species), because these natural variations are already included. For this reason, we think that there has to be at least one missing sink, which accounts for the difference in P_{calc} and P_{need}.

Specific comments (clarifications/additions needed)

Line 56: Please also cite Moore and Wang 2006 and Hackenberg et al. 2017; both also show depth profiles.

- **Thank you for pointing that out. We added Hackenberg et al. (2017), but not Moore and Wang (2006), as the sentence is about the comparison of chl-a and isoprene in a depth profile and they do not provide any chl-a data.**

Line 57/Table 1: The correlation shown in Kurihara et al. 2010 is for isoprene between 5 and 100 m depth, not only surface waters.

- **Sentence changed to: "…and furthermore, Broadgate et al. (1997) and Kurihara et al. (2010) show a direct correlation between isoprene and chl-a concentrations in surface waters and between 5 and 100 m depth, respectively."**

Line 100: Can you give more details for the vials used? (e.g. custommade/ manufacturer, how is the headspace achieved)

- **The sentence has been changed to: "10 mL of helium were pushed into each transparent glass vial (Chromatographie Handel Müller, Fridolfing, Germany) replacing the same amount of sea water and providing a headspace for the upcoming analysis."**

Line 139: Can you re-word " to relate... diagnostic pigments" to clarify the sentence? I can't follow what it means.

- **In the following we have explained in more detail this method. However, we think all this information can be easily obtained from the given citations in this text, so we would prefer to only slightly change the text (by adding only "to the concentration of monovinyl chlorophyll *a* concentration. The latter is an ubiquitous pigment in all PFT except *Prochlorococcus* sp. which contains divinyl chlorophyll *a* instead.." to the text) in order to keep the paper focused: "PFT was calculated using the diagnostic pigment analysis developed by Vidussi et al. (2001) and adapted in Uitz et al. (2006). This method uses specific phytoplankton pigments which are (mostly) common only in one specific PFT. These pigments are called marker or diagnostic pigments (DP) and the method relates for each measurement point the weighted sum of the concentration of seven, for each PFT representative DP to the concentration of monovinyl chlorophyll *a* concentration and by that PFT group specific coefficients are derived which enable to derive the PFT chlorophyll *a* (chl-a) concentration. The latter is an ubiquitous pigment in all PFT except *Prochlorococcus* sp. which contains divinyl chlorophyll *a* instead. In general, the chl-a is a valid proxy for the overall phytoplankton biomass. In the DP analysis as DP concentrations of fucoxanthin, peridinin, 19'hexanoyloxy-fucoxanthin, 19'butanoyloxy-fucoxanthin, alloxanthin, and chlorophyll *b* indicative for diatoms, dinoflagellates, haptophytes, chrysophytes, cryptophytes, cyanobacteria (excluding *Prochlorococcus* sp.), and chlorophytes, respectively, are used. With the DP analysis then finally the chl-a of these PFTs were derived. The chl-a concentration of *Prochlorococcus* sp. was directly derived from the concentration of divinyl chlorophyll *a*."**

Line 140: Specify that [PFT] in the remaining text refers to the chl-a concs of each PFT.

- **Every time we talk about the actual chl-a concentration of a PFT in the manuscript we now changed "PFT concentration" to "PFT chl-a concentration" to be more specific.**

Lines 150-153: Can it be made clearer which steps were a separate step and which were a more detailed description of a previously mentioned step? Also, line 153-155: could clarify by deleting "last" and changing to "... profile, the Ctot and Zeu values from this last integration" (it was not immediately clear whether the last or second to last set of values was referred to). Line 152: How was determined which equation needed to be used?

- **We have rephrased the whole paragraph and hope to have improved what exactly done. This should also clarify the two points mentioned below.**
  For clarification which equation was used: You first apply Equation 2. When your Z_{eu} is larger than 102 m you start again with the calculation using Equation 3 and taking the outcome of Z_{eu} from there.

Line 157-163: Is EdPAR(0-) in W m-2 before conversion to PARsurface ? If so, please explain the unit conversion more clearly. The text changes from using subsurface irradiation to surface irradiation without giving details of why these are equivalent. Also, why was the measurement used in those units if it was also available in umol m-2 s-1 (line 146)?

- **Please see above.**

Line 163: Does EdPAR(0+) refer to surface irradiance as initially defined? If so, why is it used in a depth profile, while a correction is necessary for subsurface radiation EdPAR(0-)?

- **Please see above**

Lines 172 and 484 and Table 3: This suggests that Booge et al. 2016 contains new laboratory data; please specify that it is a collection of literature values, also in Table 3.

- **Done. Thanks for pointing that out.**

Line 181-187: This paragraph was slightly difficult to follow. Which depth does "each depth" refer to (isoprene sampling depth? 1-m bins?)? If pigment data and hence [PFT] was only available at a variable, small number of depths within the MLD at each station, how does this affect Pdirect given that it is calculated as the "sum of all products", which presumably means at all measured depths? Would a sum of two depths not result in higher production than a single depth, if all depths display similar [PFT] and production rates? Please clarify the paragraphs on these calculations, including how they relate to the introduction to section 2.7 (one production rate per station vs. different numbers of depths used).

- **"Sum of all products" does not mean "sum over all depths". Following Equation 7 we multiplied for every sampled depth z the concentration of each PFT (PFT_i) with its (light-depth-dependent) $P_{chloro,i}$ value resulting in a production rate for PFT_i at sampled depth z. To calculate the total isoprene production P_{direct} at sampled depth z we summed up all individual production rates of all PFTs measured. In order to use only one production rate per station, we integrated the derived production rates of all measured depths z for each station over the total MLD. Scaling with the MLD gives us the total "mean" isoprene production within the mixed layer.**
  We agree with referee #2 that these calculations are not described clearly in the text. For clarification, we changed the text, starting at line 180:"In order to calculate the isoprene production at each sampled depth (z) at each station, we used the scalar photosynthetic available radiation in the water column, PAR(z), (see section 2.6) as input for I, which was used with the respective, calculated EF of each PFT using Equation 6. The product was integrated over the course of the day, resulting in a P_{chloro} value (µmol isoprene (g chl-a)$^{-1}$ day^{-1}) for each PFT and day depending on the depth in the water column (Figure S4). The light and depth dependent individual $P_{chloro,i}$ values of each PFT at the sampled depth z were multiplied with the corresponding, measured PFT concentration ($[PFT]_i$). The sum of all products gives the directly calculated isoprene production rate at each sampled depth z:

$$\mathbf{P_{direct}(z)} = \sum\left(\mathbf{P_{chloro_i}} \times [\mathbf{PFT}]_\mathbf{i}\right). \quad (1)$$

  Integrating over all measurements within the mixed layer and scaling with the MLD results in a "mean" direct isoprene production rate (P_{direct}) for each station."

Line 198: Mean wind speed/temperature taken from satellite in situ or from 24h of shipboard observations (not at the same site as CTD)?

- **For clarification we changed the sentence to: "…, we used the mean wind speed and the mean sea surface temperature of the last 24 h of shipboard observations before taking…"**

Line 305 etc: Please specify if these calculations (and any others in the manuscript) were performed only for MLD data. This is not always clear where results are referred to after the initial presentation of the profiles.

- **In paragraph 2.7, lines 166 etc. we state: "For all calculations made we came up with one production rate per station within the mixed layer. This was either due to…" We give this information right in the beginning of the method section to make clear that this is valid for the whole paper. For clarification we added this information again at line 305: "Therefore, we calculated new individual chl-a normalized production rates of each PFT ($P_{chloronew}$) within the MLD."**

Line 425: Can you re-word "these cruises" to be more specific? OASIS is mentioned separately due to a higher kAS (Wanninkof and McGillis, 1999), so it can't mean all three cruises in this work?

- **We changed the sentence to: "However, during SPACES and ASTRA-OMZ the wind speed was…"**

Line 449-451: While the statement that rates should be evaluated in water (and possibly in seawater, due to matrix effects?) is valid, the singlet oxygen reaction rate in Palmer and Shaw (2005) is in fact for chloroform (from Monroe, 1981).

- **Correct, we changed the sentence to: "It must be noted that the loss rate due to the reaction with OH is a gas phase reaction rate (Atkinson et al., 2004) and the used rate for reaction with singlet oxygen derives from measurements in chloroform (Monroe, 1981), meaning that these rates might not be suitable for isoprene reactions in the water phase."**

Line 464: Should this be "isoprene concentration is no longer correlated to bacteria abundance", rather than referring to the isoprene production rate?

- **Yes, we changed the sentence to: "…, the isoprene production rate is much higher than the degradation rate by bacteria and, therefore, the isoprene concentration is no longer correlated to the bacteria abundance."**

Line 467: Please clarify "it is important to scale the loss" - why is it important/in order to do what?

- **The loss rate constant of bacterial degradation is variable looking at the different regions (cruises). This means that this loss is not just a static number and therefore is dependent on something, such as environmental parameters or bacterial cell counts. For clarification, we changed the sentence starting in line 465: "Due to the different loss rate constants of bacterial degradation […] in the different regions it is important to identify their dependence on environmental parameters. "**

Line 468: Caused by the presence of different bacteria or by differences in their ability to use isoprene (or both)?

- **For clarification we changed the sentence to: "…, which may be caused by different heterotrophic bacteria, each with a different ability to use isoprene as an energy source."**

Lines 473-475: This point has effectively been previously made in other studies. Environmental factors/stresses such as temperature and light are already known to influence biological activity, and that in turn is already known to influence isoprene production.

- **Yes, the referee is absolutely right, it is known that environmental factors influence the isoprene production. The point we wanted to make is that the trend of higher loss rate**

constant and higher AOU values might be a hint that also isoprene loss/consumption is actually influenced by biological activity and not only by air sea gas exchange or chemical loss.

Line 489: Ideally, use a different word instead of "show" - the results support existing theories/knowledge that these influences exist (described just before this), as opposed to showing something new. The salinity and nutrient relationships specifically do appear to support the hypothesis of stress-related isoprene production.

- **Changed to "The results confirm findings from previous studies…".**

Lines 499-502: What exactly do you mean by this? Do the parameterisations need to be assessed, i.e. are specific factors for isoprene needed? Generally agreed values are not even available for the most common gases studied. It is worth pointing out that the parameterisation chosen will affect each study, so that perhaps it is useful to present different results if possible/relevant in a study.

- **As isoprene is a very insoluble gas, like CO_2, we think the existing parameterisations are applicable to isoprene. We wanted to point out that there are different commonly used wind speed based k-parameterisations (i.e. Nightingale et al. (2000) or Wanninkhof and McGillis (1999)), which lead to different emissions, especially in a high wind speed regime (>10 m s^{-1}), which we discussed in lines 420-429. To clarify this point in the conclusion we changed the sentence to: "Furthermore, the most appropriate wind speed based k parameterization to compute air sea gas exchange, the main loss process for isoprene in the ocean, must be used in future studies."**

Line 502: Could "The evaluation [...] should be examined" be worded differently?

- **We changed the sentence starting at line 502 to: "Isoprene loss processes, in conjunction with the complexity of isoprene production, should be further examined in order to predict marine isoprene concentrations and evaluate the impact of isoprene on SOA formation over the remote open ocean."**

Line 694 (Table 1): bold/italic is defined, but what are the R2 values that are neither?

- **The authors do not state in their publications if these correlations are significant or not. We added this additional information to the table caption.**

Fig 1: Why are not all station numbers shown? Where they are shown, it is often difficult to assign them to a particular dot. There also seem to be stations omitted or not visible? If they cannot be shown (same location as another one) or were not sampled (as suggested by Fig 3), please add this information to the caption. It may also be useful to add station numbers to Fig 3 to connect the two pieces of information.

- **For a better readability we added not all but almost all station numbers to Figure 1 and added the following sentence to the figure caption: "Numbers indicate stations where a CTD depth profile was performed. Stations 6 & 8 (SPACES) as well as stations 4 & 6 and 13 & 14 (OASIS) have almost the same geographical coordinates. If a station number is omitted (SPACES: stations 5 & 7; OASIS: station 3, 5 & 12; ASTRA-OMZ: stations 4 & 9) no CTD cast was performed."**
  Station numbers are added to Figure 3.

Fig 5: Can you please show n in this figure for each set of data and add some details to the caption about the left vs. right part of the graph or refer to the main text (especially 5b) to clarify? Also, why are most of the whiskers for SPACES and OASIS in 5a different once the outliers have been excluded (other values should not be affected if one point is removed)? (For 5b, the new calculations can explain the changed whiskers, but are only mentioned in the main text.)

- **We updated Figure 5 by showing the number of stations that were included for each set of data in the boxplot and provided some information in the figure caption: "Percent differences […] for the different cruises / cruise regions. Left of the vertical black line data is divided into the three different cruises, right of the vertical black line data is shown for the three cruises where outliers from left part are excluded. Additionally, ASTRA-OMZ was split into three regions (equator, coast, open ocean). Number of stations (n) used for each set of data is shown in italics. The red line represents the median, the boxes show the first to third quartile and the whiskers illustrate the highest and lowest values that are not outliers. The red plus signs represent outliers. The number indicated after \ denotes a station that has been excluded from the analysis."**
  The referee is absolutely right, the whiskers should not be affected for SPACES and OASIS in Figure 5a when excluding the outliers. Accidently, the data for SPACES\1 and OASIS\10 in Figure 5a were interchanged. We have now fixed the figure.

Fig 6, 7, 8, 10: What do the error bars show? Error on measurement or standard deviation of the average? Please add this information to the caption.

- **Error bars show the standard deviation of the average. This information was added to the figure captions.**

Fig S2: Why was EdPAR(0+) calculated if there were also measurements available (binned data implies measured)?

- **Measurements were not available for all stations, therefore EdPAR(0+) was calculated and verified with stations where measurements were available.**

Fig S3: Why are chlorophytes and cyanobacteria functions not shown (EFs are listed in Table 2)? Please add to plot or add reason to caption.

- **We added chlorophytes and cyanobacteria to figure S3.**

Technical comments

Line 49: Change to "the concentrations generally range", as the following sentence presents different concentrations.

- **Done.**

Lines 76 and 454: reference should be Acuña Alvarez

- **Done.**

Line 131: Use "Phytoplankton functional types..." as heading for consistency

- **Done.**

Lines 133, 146 and 150: Change to "same stations as isoprene was sampled"; "subsurface irradiation", to define EdPAR(0-); and to "...the total chl-a concentration integrated..."

- **Done.**

Line 139/140: Replace "By that" with something like "This was used to derive..." or "The chl-a concs... were derived that way"

- **Done.**

Line 143 etc: Can PAR stand for both photosynthetically active radiation and photosynthetic available radiation? The latter does not seem commonly used.

- **Yes, it can. In our manuscript we use "photosynthetic available radiation" consistently.**

Line 163: EdPAR(0+) should have superscript and be in italics? (also in Fig S2?)

- **Done.**

Line 167: Suggest changing to "...due to aÂˇn shallow mixed layer depth (MLD) resulting in only one..."

- **Done.**

Line 254-256: Either the numbers or the description appears to be the wrong way round; dividing the mean by the concentration at a certain depth would give >1 for a smaller specific concentration.

- **Fixed the description to "…we normalized the measured values by dividing the concentration of each depth of each station by the mean concentration in the mixed layer from the same station profile."**

Lines 300, 318, 453: punctuation before "2)" is almost invisible; remove comma after "which"; add comma after halocarbons

- **Done.**

Line 308/318: Is there a difference between >80% of "total PFTs" and "total phytoplankton chl-a"? If not, this statement is only needed once.

- **There is no difference and the second statement (line 318) was deleted.**

Line 334, 357, 487: change "than" to "from"; "stations"; "in-field production rates"

- **Done.**

Line 388: "more saline" or "higher salinity"

- **Done.**

Line 441: Add "Here, [the loss rate constant...]" to start of the sentence to clarify.

- **Done.**

Line 499: must be further assessed? Furthermore, air-sea [...] has to be assessed?

- **Done.**

Line 504: evaluate "their" impact (of the isoprene concentrations - if this refers in fact to the evaluation of the processes, the sentence is not very clear and should be reworded)

- **We changed the sentence to: "Isoprene loss processes, in conjunction with the complexity of isoprene production, should be further examined in order to predict marine isoprene concentrations and evaluate the impact of isoprene on SOA formation over the remote open ocean."**

Line 507: A link to the database would be useful.

- **As there is no data uploaded yet, we cannot provide a link, unfortunately. We will update as soon as possible.**

Lines 704 and 738: (Table 3 and Fig 5 captions): remove the first "that"

- **Done.**

Fig 1: x-axis values partially obscured for OASIS/SPACES

- **Done.**

Fig 4 and Line 252 / Fig 8 and Lines 417-434: A darker shade of green would be easier to see (Fig 4); dotted lines are quite faint and legend covers error bar (Fig 8). Legend and description duplicate the information needed, details are also not needed in main text. ASTRA-OMZ details are also already given above the plot; check (c/d/e) (Fig 4).

- **Done.**

Fig 6 caption: Pchloronew , not Pchloro , according to main text?

- **Done.**

Fig S1: y-axis is umol m-2 s-1, while caption refers to W m-2. If a conversion was made, please specify.

- **Done.**

References

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