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Discussions

Interactive comment on "Isoprene emissions track the seasonal cycle of canopy temperature, not primary production: evidence from remote sensing" *by* P. N. Foster et al.

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We thank both reviewers for useful comments and recommendations for clarification. We are happy to include almost all of the recommendations and list below the reviewer comments and our proposed responses. We noted four principal issues, summarized here:

- The presentation of our results is now modified, to clarify (a) that we are not claiming to be the first to use canopy temperature in a model for isoprene emissions, and that (b) it is not intrinsically surprising that the seasonal cycle of isoprene shows a relationship to canopy temperature. We explain how our results extend the conclusions from leaf-

scale observations and experiments that have previously shown that isoprene emission is (at least in part) a protective response to heat stress.

- Second, regarding the fact that satellite HCHO data have large errors at high latitudes: recognizing this point, we have shifted the greatest emphasis to tropical regions, where most isoprene is emitted, the satellite data are most reliable, and the difference between the seasonal cycles of GPP and temperature is most marked.

- Third, we discuss the research study design with regard to the 'single driver' which one reviewer indicated as a problem. We now state explicitly that the results do not imply that GPP is unimportant in isoprene emission; but rather that GPP does not regulate the seasonal cycle in the quantity of isoprene emitted.

- Fourth, we discuss the statistical results, especially the p-values, in a little more detail now, to clarify our interpretation of high p-values (and low correlations) as implying simply a lack of correlation between isoprene emissions and GPP.

Response to reviewer no. 2

While the general idea to locate the predictors to describe isoprene emission on large scale at the proper scale is very welcome the authors finally try to track down a rather complex system to just one factor. That is, at least at the scale of interest, not possible. The conclusion, to better replace GPP driven seasonal dynamic by a temperature driven one may not be the worst way but a multi-factor system should take the major drivers into account (see comments below) and therefore (very simplified) without biomass (e.g. GPP) there is no canopy-temperature. Naturally we agree that there is no canopy temperature without biomass, and no biomass without GPP. But we were led by what the data showed. We did not set out to find a 'single driver'. Canopy temperature however emerged from our analysis as by far the strongest driver of the quantity of emissions – far stronger than GPP. It was not what we expected. Our first attempt was to fit the 'reference model', which multiplicatively combines influences from temperature and GPP. But the data showed that this model does not provide a good

fit, whereas we found a startlingly high correlation with the canopy temperature and a very low correlation with GPP. Our paper is not meant to imply that GPP, biome type and CO2 concentration are irrelevant. Our results simply show that the seasonal cycle of isoprene emission strongly tracks canopy temperature. This is an observation we consider to be well worth noting, and one that needs to accounted for by process-based models in the future. Also the statement of the title and the aims set can not be rooted on the simple correlation analysis presented here. There is experimental, leaf-scale evidence for a strong temperature-dependence of isoprene emission and this is consistent with the interpretation of isoprene emission as, at least in part, a protection against heat stress. But this understanding of the nature and role of isoprene emission does not immediately lead to a prediction of the seasonal cycle of emissions at the canopy scale. Our contribution is to show empirically that the seasonal cycle of isoprene emission is overwhelmingly dominated by a response to temperature rather than by, for example, the supply of carbon chains in photosynthesis. The title of our paper makes no claim to anything other than this empirical discovery and our paper naturally leaves open the question of mechanism.

The authors claim to "set out to discover general, biome-independent relationships". As there are different definitions of a biome available, it should be specified which one of these definitions was used in the context of this work. It is a bit strange to me that the authors seek that, because their predictors include GPP and canopy temperature. All definitions of a biome include ecosystem(s) or the biome definition equals an ecosystem. The plants within ecosystems, and by that within biomes should have a property like GPP and canopy temperature. How can you get the predictor "biome-independent"? What does that mean? We used the expression 'biome-independent' but the reviewer points out that this is misleading. We agree and have changed the wording to clarify that we mean simply that our results apply across biomes.

One problem using HCHO column data over a large range of latitudes is that in high latitudes the "systematic errors" of the product yield up to 50% and that cloudy intervals

can lead to even higher errors (de Smedt et al. 2008). Therefore, the use of such a product has some limitations. However, these possible limits and their impact on the empirical predictors aimed to develop here are not well addressed. As example, analysis of column data includes application of averaging kernels, inverse modeling approaches and linearization techniques on the data (Rodgers, 2000), i.e. some parts of the "systematic error" rely on the state of the system or the data itself, therefore they are not like "a uniform offset". Given that, the argument in section 2.3 which relies on the fact that the standard deviations remain the same regardless where the mean values are shifted does not hold. Explicitly, how do you cope with the situation that in high latitudes your eye is half blinded? What are the consequences on your findings due to that? How do these errors impact on your main assumption that HCHO = f(isoprene in air) = g(emission) = h(predictor) outside the tropics? These are good points, to which we have responded as follows. 1. We have modified our conclusions so as to emphasize the tropics. This is the region of most isoprene emission, it is the region where previous work has been the least conclusive, and it is where the satellite data is most trustworthy. Furthermore, it is in the tropics that we see a major phase difference between the seasonal cycles of GPP and temperature, which allows us to draw a strong conclusion.

2. For the extra-tropical regions, we have now made it clear that: a. The satellite error can be up to 30% in mid-latitudes and can be as high as 50% in boreal regions (as per de Smedt et al's results for March). b. However, in the high isoprene emission season, sunlight is strong and this improves the HCHO signal dramatically. c. Averaging over regions, as we do, is expected to further reduce errors. d. Standard deviations of the inter-annual variability are quite low (below 17%: see Supplementary Material). e. The postulate that isoprene is a function of canopy temperature in extra-tropical regions is consistent with the results in the tropics. We tentatively extend our conclusions about tropical isoprene emissions to a global phenomenon, while recognizing caveats about the data in mid- and high latitudes.

3. We have removed the statement that systematic errors are unlikely to affect the seasonal cycle.

It is, from a plant physiological viewpoint, not a really new or surprising finding that isoprene emission correlates better to leaf/canopy temperatures than to air temperature or light for example. We agree with this point, although our work is the first to demonstrate it using satellite data. We have modified the text to make our meaning clearer.

A clear fact is that, light driven photosynthetic carbon flux is three orders of magnitude larger than the isoprene emission flux on leaf scale (_mol vs nmol). In that sense, even with strong changes in photosynthesis (maybe due to drought) there is enough potential to emit some isoprene as long as there is energy and these changes must not translate into the same changes in isoprene emissions. This is an excellent point: in a sense, it is possible for isoprene emission to be uncorrelated with GPP precisely because it represents only a tiny fraction of GPP. We have added a note to this effect.

Another point in that context is, that GPP represent the biomass of the area in question. As the major emitting tissues are leaves (or needles), there has to be a basal amount of leaf biomass to get that emission. In terms of a seasonal scenario, as aimed in this manuscript, there should be some discussion on possible changes and shifts in the impact of the possible factors that promote emissions. As example, without leaves, there are no isoprene emissions for deciduous emitting trees. In pringtime, a portion of the emissions might correlate well to GPP as the emitting tissues are formed. Once established the leaf area is not changed very much anymore until senescence. For sure, that is modulated by temperature as predictor over all the season. The latter will be mostly valid in boreal and temperate zones. Moving to the tropics, the GPP dynamics will be another one as the trees are evergreen. Their change in seasonal leaf biomass may be rather constant and temperature effects may even more modulate the emissions. We certainly agree that there must be leaves present in order to have any isoprene emission, and we have clarified this in the revised text. As this reviewer pointed out earlier, however, photosynthetic carbon flux is three orders of magnitude larger than

isoprene emission flux on a leaf scale; so there is scope for large variations in isoprene emission whether leaf biomass is small or large. Moreover, it was not obvious a priori that isoprene emission would be essentially uncorrelated with GPP. Models previously have assumed that isoprene emission quantitatively depends on GPP as well as on temperature. Our results do not support this assumption. Introduction: It seems, the authors take it for granted that elevated CO2 lead to a reduction in isoprene emission. That response is yet only confirmed on small scale and per leaf area as immediate response to oncentration change. Centritto et al. (2004) and Sun et al. (2013) as examples report different behavior where the enhanced leaf number and leaf area either rule the isoprene's down regulation out or lead to enhanced emissions on whole plant level. Especially on larger scales, these isoprene down regulation schemes should be taken carefully and graded to be preliminary. To my opinion, the uncritical application of the leaf scale short term effect on large scales lead to wrong results. This is a good point. We have changed the relevant sentence in the Introduction. We have left the reference model as it is, with its explicit inverse relationship between CO2 and isoprene emission, so as to allow direct comparison with Arneth et al (2007); but we have added a note concerning the (uncertain) generality of this response.

Methods: The fraction of monoterpene emitting species vs isoprene emitters in boreal zone may lead to the fact that the HCHO column is not linked to isoprene as there are no or very few isoprene emitters around. Is then, the HCHO column still a good estimate for isoprene emission? Or is it a more general "terpene" emission? To claim that there is "one order of magnitude" less non-isoprene sources is especially a problem as that number is according to the cited literature and therefore it is the estimated or assumed basal emission factor that is one order of magnitude less for monoterpenes as example. But that does not mean that the actual source strength is as well lower because beside the bare emission factor the emitting area (LAI), species distribution, energetic state etc play a role. Again I would like to mention that this system is a multi factor system. Grading it by just one factor will be wrong or lead to wrong assumptions for the chemistry model inputs. Examples can be found in Bourtsoukidis et al. (2014)

where spruce in a hemiboreal forest emit 14x more monoterpenes than isoprene or Noe et al. (2012) where ambient concentrations of isoprene are one order of magnitude lower than monoterpene concentrations. As noted above, we have shifted our main emphasis to tropical regions. We have also added a proviso on this point about monoterpenes, referencing Bourtsoukidis et al (2014). Specifically we have changed the original wording: "We ignored the production of formaldehyde from non-isoprene sources as it is at least an order of magnitude smaller than the isoprene source (Lathière et al., 2006)." to:

"We ignored the production of formaldehyde from non-isoprene sources as isoprene dominates the hydrocarbon flux from the biosphere to the atmosphere (Sharkey et al. 2007), although this may not be accurate in boreal regions where monoterpenes can exceed isoprene emissions."

Supporting Material: Why do you use a linear relationship to relate HCHO columns seasonal cycles to temperature? I guess you use something like Pearsons correlation coefficient (?) which is a linear relation. But then, it would be better to use a log transformed seasonal cycle to make a valid statement on the correlation. For simplicity, and because the data do not suggest anything more complex. We now clarify that we are using Pearson's correlation coefficient. and thereby fitting a linear relationship. We also mention that we considered an exponential relationship as well, in line with Arneth et al (2007), but that the results were indistinguishable.

What are ecobands? We have removed this term, but now we explain that we grouped the sites according to latitude and further broke down the tropical regions into grasslands and forests.

Response to reviewer no. 4

Authors show better correlation of formaldehyde with canopy temperature when compared to correlation with air temperature and suggest using canopy temperature in the models. In fact this finding has already been adopted. Emission models simulate

canopy temperature using the dynamic vegetation or canopy environment model (e.g. Arneth et al., 2007; Guenther et al, 2006; Guenther et al., 2012). This study is a valuable support to this model approach. Authors may want to rephrase and include this fact in the manuscript. We did not mean to imply that the use of canopy temperature as a driver was novel – indeed, we took the idea directly from Arneth et al (2007). We have rephrased this accordingly.

However, the conclusion regarding the relationship between isoprene emissions and GPP (fPAR) is not that clear. As shown in the Table S1, the p-values are often higher than 0.05, which leads to doubts about statistical significance of obtained results. E.g. discussion of negative correlation of GPP in Australia supporting the authors' conclusion of isoprene emissions following temperature rather than GPP in temperate region corresponds to p-values equal 1. Similarly, high p-values are found for correlations between formaldehyde and modeled isoprene emissions, GPP and fPAR in many of the listed tropical regions. Nevertheless, these correlations are used to prove that there is no relationship between isoprene emissions and GPP in the tropics. To my opinion, high p-values for these results are not sufficiently discussed in the manuscript. The reviewer correctly points out that the p-values for the relationship between the seasonal cycles of GPP and formaldehyde (HCHO) are non-significant in Australia and in many tropical regions, but seems to misinterpret how these values were interpreted. Our argument is that GPP is not a driver of the seasonal cycle of isoprene emissions – in other words, we cannot reject the null hypothesis that there is no relationship between GPP and HCHO in these regions. This is a conventional (and surely non-controversial!) use of p-values. Furthermore, these large (i.e. non-significant) p-values are accompanied by small values of the correlation coefficient, as we would expect. The study also shows similar dependence of formaldehyde concentration on canopy temperature and on precipitation (only opposite) in the tropical region. This is an interesting result that, to my knowledge, has not yet been presented. We agree that it is interesting, and we have added some words about it. But the relationship is much less straightforward than for canopy temperature as the correlation switches from positive to negative

in water-stressed ecosystems. It seems plausible that the canopy temperature is the physical variable with the most direct relevance for isoprene emission, as the response is consistent across ecosystems.

Specific comments: 1) The authors may want to specify which isoprene emissions were used and how were they calculated in the chemical transport model TM5 when conducting the test of the seasonal cycle of the net chemical modulation of formaldehyde (results presented in Figure S1). The modeled formaldehyde production depends not only on the applied chem. mechanism, but certainly on the input emissions as well. We have added more information about the isoprene emissions used in TM5.

2) Since the p-values are important part of the results, I'd suggest moving Fig. 4 to the Supplement and replace it by Table S1. We have moved Table S1 to the main text in response to this suggestion. But we would like to retain Figure 4 in the main text because we believe this visualization helps comprehension of the results.

3) There is no reference to and no discussion of the Table S2 and Figures S3-4 in the manuscript. Without a discussion, it is not very clear to me what is the added value of the results in Table S2. Why were the sigma values in Table S2 calculated only for the modeled variables and not for the observed ones? We calculated the standard deviations of the modelled variables to see if they would indicate any large variations that might compromise the results. Similarly, we made Figure S3 in order to identify possible biases in the correlation analysis. (We presume the reviewer's mention of Figures S3-4 refers to the four panels of Figure S3; there is no Figure S4.) But as we don't specifically refer to these issues, we have omitted these Figures in the revised paper.

4) The statement "Our results however suggest that current models are unlikely to capture the over-riding dominance of canopy temperature as a predictor of the seasonal cycle of isoprene emission, especially in the tropics" (P19587-25) is maybe too strong considering the fact that the study is using only one emission model and the statistical

significance of the results is questionable. As noted above, we do not agree that the statistical significance of the results is questionable. But it's true that we have not made a systematic comparison wth current models, and so this statement is too strong. We have toned it down in the revision, to indicate simply that models must show they are capable of reproducing the seasonal cycle of isoprene emissions in the tropics. 5) In the discussion authors state that the "remotely sensed observations provide a bridge between the global scale of interest in the relationship between atmospheric chemistry and climate, and the more local scale of direct emissions measurements" (P19587-27). Can the presented results based on relatively large areas (240 km) be representative also on smaller scales? We are not implying the satellite data are representative on local scales, but rather that these data begin to fill a 'gap' between point-source observations and global numbers. We have clarified the wording on this point.

Technical comments 1) Please unify abbreviations of region names (boreal, temperate, tropical forest, tropical savannas) in Table S1 and Table S2. Our use of names was inconsistent between the Tables. However, we have now deleted Table S2 as mentioned above.

2) Background of the Fig.4 and color for boreal region are quite similar. I'd suggest changing the background color to white. We have altered the background of Figure 4.