Our responses to these comments (in blue italics) are given below:

1. It's not clear which canopy environment model has been used. You suggest on p6986 that you have used the dePury/Farquhar model, but you also describe the sunlit/ shaded fraction algorithms from the Guenther 1995 (G95) paper, which is rather confusing - does this mean that you used the fractions from G95 in conjunction with DePury/Farquhar? If so, are the two canopy structures compatible?

We are pleased to clarify. Equations from DeP&F are simply used to calculate values of sunlit and shaded PAR (and its diffuse and direct beam components), whereas those of G95 are used to calculate the sunlit and shaded fraction of the canopy LAI. Both approaches are fully compatible. We will revise the text to be clear on these points.

Your statement in the introduction on p6985 that neither G95 nor MEGAN account for differences between diffuse and direct radiation was also confusing. G95 contained algorithms for sunlit/shaded fractions and my understanding is that the canopy environment model that is used by NCAR with the full MEGAN algorithms does consider diffuse/direct PAR (as it's based on the BEIS canopy model). You perhaps need to clarify that the models have not been used/developed explicitly to investigate the effect of altering diffuse/direct radiation.

The sunlit/shaded fractions equations of canopy LAI given in G95 do not include a treatment of diffuse and direct beam radiation. We are also reiterate that there are no explicit equations in the MEGAN Guenther et al. ACP paper for dealing with Idiff and Idir. Regardless, the novelty of our MS is recognizing that changes in Idiff and I dir could be important for isoprene emission and showing how they might be investigated.

2. MEGAN algorithms: Which "MEGAN" are you using? On p6986 lines 8-9, you state you have used an existing isoprene emissions model and reference both the 1995 and 2006 Guenther et al papers - yet these described different models. Sure, the 2006 model MEGAN builds on the earlier G95 algorithms but they are distinct. Just reference the one you have used in this study (MEGAN).

We will be pleased to clarify issues of equations used in our revisions.

If I have understood the method description on p6986-6987 correctly, you've used the gamma-p activity factor from MEGAN (which is appropriate as it is the taken from the full MEGAN algorithms that are designed to beused with a canopy environment model). BUT the gamma-T activity factor from the PCEEA version (ie the MEGANv2.04 coded version). The PCEEA versions of the algorithms incorporates a a simplified canopy model and was specifically developed for use without a canopy model as the effects of a canopy are already included to a certain extent. It is different from the gamma- T activity factor in the full MEGAN algorithm and the two gamma-T factors do give different results , although I have no idea how significant these differences would be in the context of diffuse/direct PAR. However I would speculate that changes in the diffuse fraction of light would also affect the temperature profile in the canopy. This seems to me to be a major, and unecessary inconsistency. Do you have any idea how significant the differences would be? It may be that the overall conclusions are robust but the magnitude of the changes may be under or overestimated.

We will be pleased to clarify issues of equations used in our revisions. We do not consider effects due to temperature, which is kept constant in all of the cases we considered. We could therefore have simply neglected gamma-T (i.e. just set gamma-T = 1), but we decided to use a proper equation for it so that the emission rates we produced were more realistic. But in all our experiments it is the qualitative trends not and quantitative results that are relevant. We could add a few sentences on secondary temperature effects of changes in diffuse and direct PAR in the discussion. However this is a difficult issue to quantify because Gu et al. reported their diffuse and direct beam radiation data for different years with different climatologies. We are willing to be advised by the editor on whether to comment on this in the revised version of the MS.

3. Inconsistencies between Case 1 and Case 2: It concerns me that Case 1 and Case 2 have been modelled at different latitudes (55N vs 42.5N) as this would presumably affect the penetration of light into the canopy as the solar angles are different for the two locations. While the two scenarios are intended to be distinct, it does lead to lack of comparability between the two. What difference would it make to the changes seen if Case 1 was also run at 42.5N (I presume you chose this location for Case 2 as it is the location of the Gu observations).

We have undertaken calculations at many different latitudes. The trends in the graphs are the same. Total isoprene emissions increase with increasing %diffuse radiation. In revising the manuscript, we will include results from the same latitude (42.5N) for both cases.

You state that Case 1 was run at a temperature of 290K - isn't clear what is meant by temperature here - daily average?

In all cases temperature refers to a daily average and this was the same in Case 1 and Case 2. We have also calculated effects of diurnal temperature in both cases and the results we report are robust.

4. Diurnal cycle: The biggest difference between the two cases is the application of a diurnal cycle to the change in diffuse/direct PAR in Case 2. This also seems to be the factor that has the biggest overall effect on total changes in isoprene emissions in Case 2. This cycle is not included in Case 1 and yet it could be expected to have a major impact on the results in this situation too. While I realise that they were modelling different future projections a complete study would include a third case that either kept total PAR constant but applied the diurnal cycle as Case 2, or allowed PAR to drop as in Case 2 but did not have a diurnal cycle to the ratio of direct to diffuse PAR. This would allow a comparison between the two projections and also give some measure of attribution (ie is it the overall change in diffuse/direct or is the diurnal cycle in the changes that has the biggest effect).

We agree the wording is unclear, but actually in both cases 1 and 2 there was a diurnal radiation cycle at a fixed temperature. We could mention this in the discussion.