First I want to apologise for the delay in my responses, I know it's frustrating to wait for feedback. But on to the review: It is quite obvious how much effort went into the revision of the paper which is great and in my opinion really improved the manuscript compared to the version before! However, I still have some (very minor) comments, which I listed below.

L. 1: You have a typo there: BOVCs instead of BVOC
L. 37: At the end of the paragraph I think you could add a sentence stating that BVOC emissions vary between years (with a reference), and this might then link better to the next paragraph where you talk about climate variability associated with ENSO
L. 64: Similar to my suggestion above, make you could highlight the knowledge gap you address in your paper here? I.e. you could talk about uncertainty in ENSO associated BVOC emissions in a future climate.
L. 79-80: Can you rephrase this sentence 'Although global changes in these variables can indicate broad global trends, anomalies associated with the ENSO are often observed at regional scales' . I find the first half of the sentence a bit confusing
L. 83: I know you say this later on in your manuscript, but I think to motivate your introduction you can include here also '[…] North East Australia (NEAus), and these regions are commonly thought to be hotspots for ENSO-associated climate anomalies' or something similar
L. 84 – 94: I think this paragraph would fit better as the second last paragraph? And then after this one you can give an overview about what you do in your study (which you currently do in L. 65 – 84). But of course this is your decision to make, and I'm also happy if you leave it the way it is.
L. 108: Can you define the acronym LPJ-GUESS?
L. 124 – 130: Do I understand correctly that with 'fully-coupled' you mean here that there isn't a feedback from the vegetation to climate variables (temperature, precip, incoming SW radiation?)
L. 144: If soil moisture is such a strong influence on the BVOC emissions, why didn't you use the soil moisture output from LPJ-GUESS but the aridity index instead? To be clear, I'm not asking you to change it, I'm just wondering why.
L. 148 – 155: I think this is great but to me it sounds like it belongs in the discussion. Up to you though!
L. 161: 'greater or equal 0.5° for five' – you're missing a C here
L. 171: Maybe I misunderstood the methods but I thought before you said that the CO2 concentration for 348ppmv is representative of 1983 (here you say 2000)
L. 178: You write here SW USA but in other places it's without a space (i.e. SWUSA)
L. 184: I think you can drop 'with respect to time' here as you state later on you're looking at temporal simulations
L. 183-188: I'm really sorry, I should have thought about this earlier, but I wonder whether you could add a third panel in your methods figure (i.e. Fig. 1) with a schematic that shows the approach you describe in L. 183-188?
L. 196: Instead of 'These simulations' you could also write 'The simulations conducted in this study'
L. 201 – 206: Could also go into the discussion
L. 217 – 220: It is not clear to me what you mean with 'the monthly order of months may be disrupted, meaning that month 1 in the simulation could be March'?
L. 221: Better than what?
Fig. 2 caption: Typo (resposne)

Fig. 3 (and 8) caption: I don't think you're describing everything you show in this figure? You say it's the Pearson correlations but to me it looks like a scatter with a linear regression fitted through the points AND the pearson correlations printed in the figure. Sorry I should have seen this earlier on but only noticed it in this revision!
Table 2: I'm sorry I'm only picking up on this now but two questions: 1. Why did you not include any estimates for significance in this table (which you did for the figures)? 2. Why did you analyse the correlation coefficient between standardised anomalies in Table 2 but in Figure 3 they are not standardised?
L. 259/260: I also wonder about interactions. Again, I'm not asking you to redo this but would you expect different results if you accounted for interactions in your statistical approach (i.e. if you applied partial correlation which allows you to control the effect of other related variables)?
L. 283: Typo: NWAus instead of NEAus
L. 290-292: Do you have any references that support your claim of a potential for either sustained El Nino (or La Nina? It's not clear from your text) in a changing climate?
L. 325: 'indicating a small boost in primary productivity' – here it might be worth noting that this is not statistically significant. In general the NPP changes seem to be not significant in most cases except for the La Nina anomaly for SWUSA? Might be worth mentioning
Fig. 6: Why did you choose these four regions?
L. 383 and 384: Again typo: NWAus instead of NEAus
L. 385: Can you explain your PCA method in the methods section?
L. 404: 'mostly resemble a potentially natural vegetation' – isn't switching LUC off the definition of potential natural vegetation?
L. 405: Do you have a reference for your statement about the dry/ moist biases?
L. 414: I was quite curious about the asymmetry in the response as well when I reread your manuscript. Do I understand it correctly that you suggest that the anomaly magnitude in the climate forcing differs between El Nino and La Nina and that might cause the asymmetric response? Then why is the asymmetry not the same for all regions? Could it be linked to different vegetation types, and/ or does LPJ-GUESS simulate different sensitivities to water stress/ water pulses depending on the underlying vegetation?
L. 470: temperature bias = temperature anomaly?
L. 553 – 560: Great discussion! One thing I wonder: You highlight LAI as a dominant driver for BVOC emissions. Maybe it is different in your model set-up, but if I remember correctly in the 'standard' LPJ-GUESS, LAI does not vary throughout the year for evergreen species (which are dominant in Amazonia and SE Asia) and the carbon allocation happens on an annual timesteps. Do you think this could influence your results?
L. 561: 'can be found in the supplementary'