Biogeosciences

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Discussions

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Interactive comment on "Leveraging the signature of heterotrophic respiration on atmospheric CO$_2$ for model benchmarking" *by* Samantha J. Basile et al.

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This study uses globally modeled ecosystem CO2 fluxes and an atmospheric transport model together with observed atmospheric CO2 concentrations to evaluate different soil respiration models. This is done by comparing the inter-annual and intra-annual variability in CO2 fluxes between model and observations. The global scale of the study makes this a rather rough comparison, which however is useful for identifying soil models that perform better at such scales. In general, there seems to be a considerable amount of work and careful analysis carried out and the study had scientifically interesting results. The manuscript has good grammar and few typos. The introduction

goes from the more general to the specific aims and summary of the methods and is well written. On the down side, much in the methods and results remains difficult to follow. Explaining the procedure in more detail will largely facilitate understanding the results and discussion. Generally, the text should be made more readable to a biogeochemistry audience that may not be familiar with the jargon and assumptions used in this specific field. Because there are many variables and procedures being used, it would help to more carefully define each part and consistently use the defined terms throughout the text. The manuscript should reach publication quality after these aspects are taken care of.

We thank the reviewer for their time and comments. We appreciate there are elements of the paper that need more translation between the atmospheric and biogeochemical communities. In response to the reviewer's suggestions, we have clarified definitions and explanations throughout the paper. We will explicitly include the following text and equations in sections 2.1 and 2.3. We further address the reviewer's individual comments below.

Text and equations added to section 2.1:

"The testbed is a chain of model simulations where soil models with different structures can be run under the same forcing data, including the same gross primary productivity (GPP) fluxes, soil temperature, and soil moisture. The testbed produces its own estimates of net primary production (NPP), the difference between GPP and autotrophic respiration (AR; Eqn. 1). Each testbed soil model in this analysis produces unique gridded heterotrophic respiration (HR) values based on its own underlying mechanism and soil C stocks. . . . From the testbed output we calculate the net ecosystem productivity (NEP; Eqn. 3). In the analysis presented here, CASA NPP was used across the testbed ensemble in the NEP calculation, thus highlighting differences in the timing and magnitude of HR fluxes from the individual soil models. From a land perspective (positive NEP fluxes into land), NEP is calculated as NPP – HR, where respiration release of CO2 decreases net carbon gains through photosynthesis. Here, we use an atmo-

spheric perspective for NEP (positive NEP fluxes into the atmosphere) by reversing the sign on the NPP flux and taking HR as positive (Eqn. 3)."

NPP=GPP-AR (1) NEP=HR-NPP (2)

... Text and equations added to section 2.3: "Throughout the manuscript, we refer to CO_2 originating from these NPP and HR component fluxes as $CO2_{NPP}$ and $CO2_{HR}$, respectively. We use a sign convention for the fluxes whereby a positive value indicates a source of carbon to the atmosphere, which means we can combine the CO_2 tracers from NPP and HR to calculate the expected atmospheric variation owing to NEP using (Eqn. 3):

$$CO_2^{NEP}=CO_2^{HR}+CO_2^{NPP} \quad (3)$$

Specific remarks: L44: is this not rather a "spatial" rather than "concentration" footprint?

We thank the reviewer for this comment, as concentration footprint is an atmospheric science term that may be less familiar to the biogeochemistry community. We have therefore changed the usage in the revised manuscript: "The high precision and accuracy of in situ observations and the fact that these measurements integrate information about ecosystem carbon fluxes over a large spatial footprint make atmospheric CO_2 a strong constraint on model predictions of net carbon exchange (Keppel-Aleks et al., 2013)."

L108: The term "phasing" is used in several places. Please describe its meaning on first use.

The term was updated in this line as the following: "For example, at seasonal timescales, improving the parameterization for litterfall in the CASA model improved phasing – i.e., the timing of seasonal maxima, minima, and inflection points – for the simulated atmospheric CO_2 annual cycle (Randerson et al., 1996)."

L114: remove "were"

The sentence was updated.

L133-134: Please explain in more detail what you mean by diagnosing using one standard deviation.

We have modified the text to describe the use of standard deviation to calculate the magnitude of interannual variability: "The magnitude of CO_2 IAV is calculated as one standard deviation on the detrended, deseasonalized timeseries, unless otherwise noted."

L138-142: Six latitude zones are mentioned but 5 used later. Maybe clarify here that 2 zones are aggregated. Also, could you use the same naming for north and south (e.g. NHL, SHL, etc.).

The distinction between latitude bands used for aggregating atmospheric CO_2 observations and the latitude bands used for the flux analysis is made in the revised manuscript at Lines 139 to 142. The hemisphere naming convention has been updated at Lines 140 to 142 to reflect the same abbreviations used later in the text: "Northern Hemisphere high latitudes (NHL: 61 to 90°N), midlatitudes (NML: 24 to 60°N), tropics (NT: 1 to 23°N), Southern Hemisphere tropics (ST: 0 to 23°S), and two Southern extratropics bands: the southern midlatitudes (SML, 24-60 S), and the southern high latitudes (SHL, 61-90 S). The global mean CO_2 timeseries is constructed as an area-weighted average of these six atmospheric zones."

L162-166: CLM can be run to simulate the mentioned fluxes. Why was CASA used instead?

The idea behind the testbed was to generate a more generalizable framework for model comparisons that could take observations (point scale simulation forced with flux tower data) or modeled output from land models like CLM, ELM, DAYCENT, etc and deliver consistent forcings to the soil models (CASA, MIMICS, CORPSE). This would allow us also to look at forcing uncertainty from other models (which is not the focus of the work

we presented). For these runs we chose to use GPP, soil moisture, soil temperature, etc from CLM simulations (CLM4.5-SP with Cru-NCEP forcing) as the input for the testbed, in part because this model configuration has been documented in Wieder et al., 2018. For the analysis presented in this paper, CASA NPP was used across the soil model ensemble in the NEP calculation for consistency in highlighting HR differences. The following sentence was added to the text for clarification in section 2.2: "In the analysis presented here, CASA NPP was used across the testbed ensemble in the NEP calculation, thus highlighting differences in the timing and magnitude of HR fluxes from the individual soil models."

L194: model structure?

We provide a definition of model structure in the updated text: "Overall, while the testbed approach contains necessary simplifications, it provides the ability to query the role of model structure, including assumptions about the number of soil carbon pools, the role of microorganisms, and the sensitivity to environmental factors, in driving HR flux differences when NPP and environmental controls are held in common."

L196-197: The procedure here is unclear. What do you mean by masked to land regions that align with the sampling zones? Do you mean the latitude zones defined earlier? Why do you specify "land regions", is it because there are ocean fluxes that need to be excluded? "mask" is a term that many reader will not understand. Why are monthly averages calculated if daily values are used for the transport model? Please add detail.

We have used the reviewer's comment as an opportunity to streamline the methods, and we have also added a flow diagram based on Reviewer 2's comment. The text has been updated as follows: "The testbed fluxes are used in two ways: first, we analyze monthly-averaged, regional fluxes for net primary production (NPP) from CASA-CNP and HR simulated by CASA-CNP, CORPSE and MIMICS. Second, we use the raw daily fluxes as boundary conditions for global GEOS-Chem runs to simulate the influence of

these fluxes on atmospheric CO2, as described in the following section."

L197-199: A repetition of the zones does not seem necessary.

This has been removed.

L203-204: Please give more detail, either here or below, of how exactly these are used as boundary conditions for the model.

Based on the reviewer's comment, we modified the order of this description to section 2.3 and provided the following details: "The testbed fluxes from 1980 to 2010 are used for land emissions to simulate the imprint of these different soil model configurations on atmospheric CO2 (Fig. 1). In our simulations, HR and NPP fluxes were separated into the five regions listed above (NHL, NML, NT, ST, SE) so that the influence of carbon fluxes originating from these individual regions on global atmospheric CO2 mole fraction could be quantified. We initialized separate species of CO2 in the atmospheric model, one for each flux (HR or NPP) and region (NHL, NML, etc.). Since we considered four fluxes (CASA NPP and three types of HR) originating in five regions, we simulated a total of 20 species. These species were tracked throughout the simulation as their spatiotemporal distribution changed due to the combined influence of CO2 fluxes at the surface and atmospheric weather. Although these species are simulated individually, we can simply sum the regional atmospheric species for a given flux (e.g., CASA HR) to determine the atmospheric CO2 arising from all fluxes over the globe."

L204: Do you mean the transport model is run for lat zones separately? How does is deal with the lat boundaries in this case?

Simulations are all run globally, so there is no issue with latitudinal boundary conditions. The text has been updated in the previous comment to explain the use of land masking to isolated latitudes from each other in the emissions component of the model simulation.

L211: "data" rather than "fields"

This change was made in the revised manuscript.

L215: Explain here the model CO_2 flux inputs and outputs (presumably the boundary conditions mentioned before?) and clarify what fluxes are omitted (fossil fuels, etc)

We updated the text to reflect that we did simulate the contribution of ocean and fossil CO_2, but neglect the CO_2 mole fraction owing to these fluxes: "We also simulated the fossil and ocean imprint on atmospheric CO_2 using boundary conditions from CO_2 CAMS inversion 17r1 (https://atmosphere.copernicus.eu/sites/default/files/2018-10/CAMS73_2015SC3_D73.1.4.2-1979-2017-v1_201807_v1-1.pdf). However, at the temporal scales of this analysis, ocean and fossil fuel fluxes had a much smaller influence on regional patterns of atmospheric CO_2 than did land fluxes. Across the six latitude bands, the detrended CO2NEP annual amplitude ranges from a factor of 1.5 (in the tropics) to an order of magnitude larger (at high latitudes) than CO_2 from ocean fluxes and fossil fuel emissions. Likewise, the IAV from fossil and ocean-derived CO_2 was at most 25% that of NEP-derived CO_2 at most latitude bands. These results are consistent with previous studies that have demonstrated that NEP drives most of the atmospheric CO_2 seasonality (> 90%; Nevison et al., 2008; Randerson et al., 1997) and interannual variability (e.g., Rayner et al. 2008; Battel et al. 2000). Given that patterns of IAV in ocean and fossil CO_2 partially cancel each other and the large uncertainty in ocean fluxes, we choose to omit these CO_2 species from our analysis."

L216: "minimize the"

The text has been updated following this and the next comment.

L217: Confusing. Anthropogenic emissions are not modeled, so they cannot have an influence. Presumably you are relating this to the observations. Please reword.

The reviewer is correct that this was done to improve the utility of atmospheric observations as a benchmark for the simulations, and we have revised the text to read: "We sample the gridded atmospheric simulation output at the 34 marine boundary layer

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(MBL) sites identified in section 2.1, using the 3rd vertical level to minimize influence of land-atmosphere boundary layer dynamics. We then calculate the latitude zone average, median annual cycle and interannual variability using the methods described for CO_2 observations (see section 2.1). Averaging CO_2 from all sites within a latitude band is consistent with our hypothesis that atmospheric CO_2 may provide constraints on large-scale patterns of heterotrophic respiration, but individual sites may be too heavily influenced by local characteristics not accounted for by the testbed fluxes. As such, averaging simulated and observed CO_2 across latitude zones smooths local information while retaining information about regional scale fluxes."

L219: "closest to the observation sites, i.e. the"

Wording correction was addressed in response to the previous comment.

L221: remove "calculations"

Text was removed.

L222-225: This clarification should come before, when the averaging is explained. Although, as stated, atmospheric [CO_2] over the ocean should integrate the signal over large regions, it is not clear why the zones and averaging was necessary. Since model data can be obtained for the observation points, the residuals of these could have been analyzed directly.

Given the generalized framework of the testbed, and the fact that it doesn't account for highly local processes that might affect the CO_2 observations, we prefer to use the zonal averaging approach we outlined rather than compare with individual sites. However, we have added clarification as the reviewer suggested: "Averaging CO_2 from all sites within a latitude band is consistent with our hypothesis that atmospheric CO_2 may provide constraints on large-scale patterns of heterotrophic respiration, but individual sites may be too heavily influenced by local characteristics not accounted for by the testbed fluxes. As such, averaging simulated and observed CO_2 across latitude zones

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smooths local information while retaining information about regional scale fluxes."

L226-242: This section is not clear.

Text changes in section 2.2 have expanded the explanation of NPP and HR as boundary conditions for the GEOS Chem atmospheric transport model simulations. A figure demonstrating the land masking technique used has been added to the Supplementary information as well as a chart detailing the input fluxes and resulting CO_2 naming convention. Additionally, Line 242 has been moved to section 2.2 to explain the use of NEP as a variable with the accompanying equation defining the term.

L226: "spatial" you mean the lat zones? How exactly do you isolate the imprints? What do you mean by "tag"? I believe "track CO_2 tracers" is confusing since "tracer" relates to flow dynamics, but the analysis is about concentration changes(?). You mention "4 sets of fluxes" but aren't the observations CO_2 concentrations. What is then being compared (you mention CO2NEP variability is comparable to observations)? Are CO2NEP and CO2HR fluxes or concentrations? If these are the fluxes, should they not be introduced in the previous section?

We thank the reviewer for this reminder to remove atmospheric science jargon from the paper. We have replaced the use of the word "tracer" with "species" throughout to indicate that we consider each region to affect only one CO_2 species. The concentration of each species is then directly tied back to a single flux. The updated text reads: "The testbed fluxes from 1980 to 2010 are used for land emissions to simulate the imprint of these different soil model configurations on atmospheric CO_2 (Fig. 1). In our simulations, HR and NPP fluxes were separated into the five regions listed above (NHL, NML, NT, ST, SE) so that the influence of carbon fluxes originating from these individual regions on global atmospheric CO_2 mole fraction could be quantified. We initialized separate species of CO_2 in the atmospheric model, one for each flux (HR or NPP) and region (NHL, NML, etc.). Since we considered four fluxes (CASA NPP and three types of HR) originating in five regions, we simulated a total of 20 species. These species

were tracked throughout the simulation as their spatiotemporal distribution changed due to the combined influence of CO_2 fluxes at the surface and atmospheric weather. Although these species are simulated individually, we can simply sum the regional atmospheric species for a given flux (e.g., CASA HR) to determine the atmospheric CO_2 arising from all fluxes over the globe." All modeled CO_2 is output as concentrations (ppm) and the ESRL CO_2 observations are also reported as mole fraction (ppm). Additional wording was added to section 2 to clarify variable units: Lines 133 to 135: "For this analysis we use reference CO_2 measurements reported in parts per million (ppm) from 34 marine boundary layer sites (MBL, Table S1) within the NOAA Earth System Research Laboratory sampling network (ESRL, Fig. 1; Dlugokencky et al., 2016)." Lines 156 to 157: "All testbed fluxes are output in grams of carbon per meter square at a daily temporal resolution and then converted to petagrams (Pg C, over a region)." Lines 227 to 228: "The model is initialized with globally-uniform atmospheric CO_2 mole fraction equal to 350 ppm."

L245-248: The calculation is not the variability but just the growth rate (or rate of change). Where does the annual IAV time series come from? Is it an annual average of the IAV series? Please clarify. Consider rewording, e.g. "The monthly and yearly rate of change of the the interannual anomaly (i.e. the IAV timeseries) was calculated as ...".

The text was reworded to explain the calculation of the growth rate anomaly (rate of change in the interannual CO_2 values, or residual values after detrending and removing the seasonal cycle) which is a distinct value from the growth rate (rate of change in the unaltered CO_2 values): "Rates of change were derived from monthly and annual timeseries to ultimately calculate the temperature sensitivity of the testbed fluxes, the modeled CO_2, and the observed CO_2 values. The CO_2 growth rate anomaly was calculated as the difference between timestep n and n-1 in both the monthly and annual CO_2 IAV timeseries. As a result of this technique, the monthly CO_2 growth rate anomalies were centered on the first day of the corresponding months. To compare

flux information with CO2 growth rate anomalies, daily testbed flux timeseries were averaged to monthly resolution and then interpolated by averaging between months to center values on the first day of each month."

L248-250: This sentence is not understandable.

This wording was replaced in the response to the previous comment.

L251-253: "fitted to ... against ..." instead of "for .. with ...". Also, where does T IAV come from?

The text has been reworded as: "Following Arora et al. (2013), we calculate temperature sensitivity (γ) using an ordinary linear regression (OLR). We calculate OLR for the interannual variability timeseries of CASA-CNP soil temperature (T IAV) against 1) atmospheric CO2 growth rate anomalies, and 2) land flux IAV (see section 2.2)."

L255: "global temperature sensitivity"

The text has been updated.

L255-259: Was this reference calculated from IAV data or actual observations? What is this reference for?

The term "reference" was used needlessly in the original manuscript, and the text has been updated as: "The global temperature sensitivity value for the observed CO2 growth rate anomaly was calculated for 1982 to 2010 using ESRL CO2 observations and the Climatic Research Unit's gridded temperature product (CRU TS4; Jones et al., 2012), which is derived from interpolated ground station measurements."

L263-277: Global variability of what? What do you define as SDrel, the ratio or the "IAV magnitude"? (presumably the latter, but be more explicit). In any case, the term IAV magnitude is confusing. Why not simply say IAV relative SD? The calculation should be made clearer. What are "regional values of simulated CO2" (since all simulations made are regional CO2). Why do you use the relative standard deviation (or CV). A

high CV from a small flux can have a smaller impact on global values than a small CV from a large flux. The actual SD may be a better measure. Also, it seems to me that using a ratio between regions and global assumes an additive effect of each region, which might not be the case (what if the regional IAV CV is larger than the global value?). What do you mean by sourced only from a single region? Consider rewriting this paragraph/revising this analysis.

We thank the reviewer for this comment and the opportunity to clarify the methods we used. For the flux analysis, the SDrel is the ratio of the IAV magnitude in a given region relative to the IAV magnitude of the globe (five regions summed). For the CO2 analysis, the SDrel is the ratio of the IAV magnitude of the global-average atmospheric CO2 arising from a single region relative to the IAV magnitude from the global fluxes. Thus, we are not using a coefficient of variability, which would normalize the magnitude of IAV to the baseline flux. As the reviewer notes, we avoid this formulation because it could lead to a high CV when fluxes are small, which is undesirable. We agree with the reviewer that the SDrel from the five regions is not necessarily additive, which is why we have provided correlation coefficients between the regional and global information since not all variations will be coherent with the global signal. The text has been made clearer as to why the values were chosen and how they are defined "We also assess the influence of individual regions on the global mean signal for both component land fluxes (NPP, HR) and simulated atmospheric CO2 (CO2NPP, CO2HR, CO2NEP). We first quantify the magnitude of variability in each region relative to the magnitude of global variability (σREL) as the ratio of regional IAV standard deviation to global IAV standard deviation. This ratio is calculated for monthly flux IAV from each of the five flux regions and for the global-mean CO2 timeseries that arises from fluxes in each of the five flux regions (e.g., the global CO2 response to NHL fluxes, or the global CO2 response to NML fluxes, etc.). The value of σREL has a lower bound of 0, which would indicate that a region contributes no IAV, but has no upper bound, since a value greater than 1 simply indicates that the fluxes in a given region are more variable than global fluxes. We note that the timing of IAV in a given region may be independent of IAV

in other regions, and thus may or may not be temporally in-phase with global IAV. We therefore also calculate correlation coefficients (r) for the timeseries of regional flux IAV and CO2 IAV with the global signal. Thus, if an individual region were responsible for all observed global flux or CO2 variability, it would have both σREL and r values equal to 1 in this comparison. The value for r will be small if a regional signal is not temporally coherent with the global signal, even if the magnitude of variability is high."

L283-285: Still at a loss of what CO2NPP, CO2HR are. Atmospheric values derived from...? Is it in any way logical that peaks in CO2NPP are max in April (early spring) and min August (mid summer) in NHL?

Definitions for CO2NPP, CO2HR are included in section 2.3. Also the peaks relate to the reversal in the sign of the NPP flux, which carries through to CO2NPP. Descriptive wording explaining the sign convention have been added to the text and Figure 2 (now Figure 4) caption. The text has been updated as: "Within the modeled carbon dioxide concentrations resulting from land fluxes, CO2NPP and CO2HR, show largest season-ality in the NHL, with seasonal amplitudes decaying toward the tropics and Southern Hemisphere. In the NHL, the peak-to-trough amplitude of CO2NPP is 39±2 ppm, with a seasonal maximum in April and a seasonal minimum in August (Fig. 4a; note, this CO2NPP peak reflects the sign reversal in the driving NPP flux (section 2.3)."

L299-300: Does CO2NEP not always consider NPP and HR together?

The reviewer is correct that CO2NEP always considers NPP and HR together, and equations have been added to section 2 to make this relationship clear.

L338: Not if NPP increases more than HR.

The text has been updated for clarification as: "Both global NPP and HR fluxes are sensitive to temperature variations at interannual timescales, with increased build-up of CO2 in the atmosphere at higher temperatures, in part because the rate of HR increases at higher temperature and in part because most latitude bands show a re-

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duction in NPP at above-average temperatures."

Figure 2 and 3: If I understood correctly, values here are differences with the long term trend. To make this clearer, it would be good to note this in the caption.

Clarification that the seasonal cycles shown are detrended values has been added to the figure captions.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-256/bg-2019-256-AC1-supplement.pdf
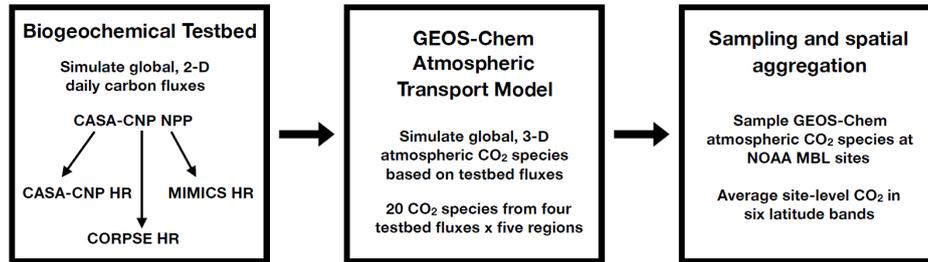
Fig. 1. Figure 1: Flow chart depiction of the analysis process from soil model fluxes to simulated CO2 concentration and comparison with NOAA observations.
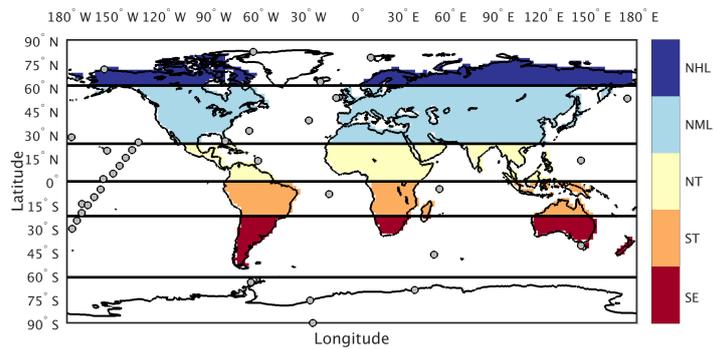
Fig. 2. Figure 2: Tagged flux regions and marine boundary layer CO2 observing sites used in our analysis. The 5 tagged flux regions are shown in color fill: Northern High Latitude (NHL), Northern Mid-Latitude
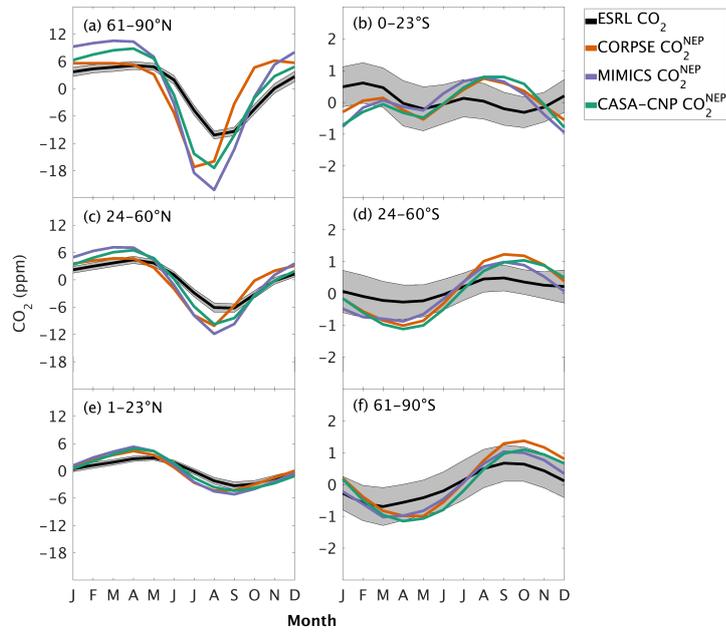
Fig. 3. Figure 3: Climatological annual cycle (median) of CO2 for observations (black) and global net ecosystem productivity flux (CO2NEP, colors) between 1982 and 2010. Monthly climatology values were create

C17
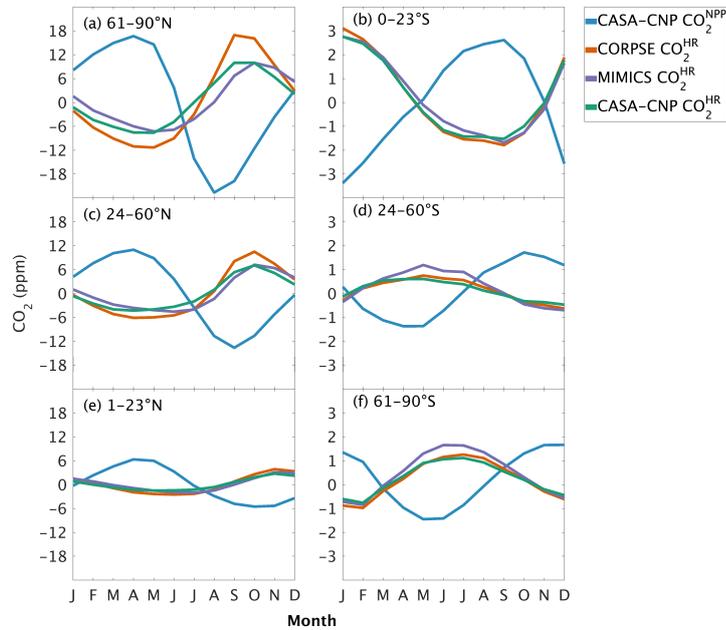
Fig. 4. Figure 4: Climatological annual cycle (median) of atmospheric CO2 simulated from land fluxes (CO2NPP, CO2HR) between 1982 and 2010. Monthly climatology values were created after detrending the CO2 tim

C18
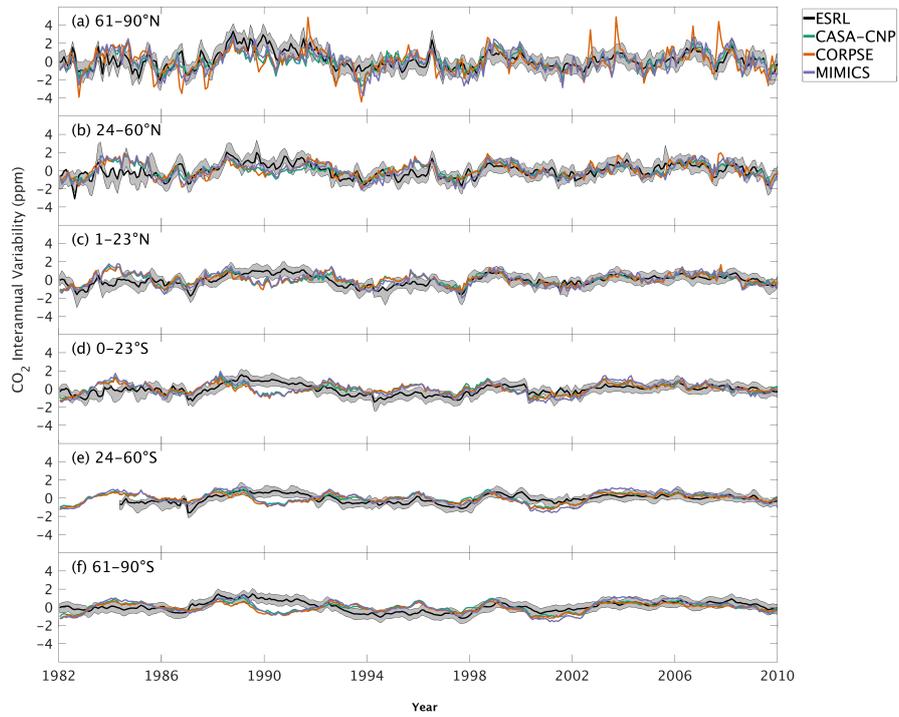
Fig. 5. Figure 5: Interannual variability of CO2 from global net ecosystem productivity (CO2NEP IAV) for testbed models (colors) and marine boundary layer observations from the NOAA ESRL network (black). Gra

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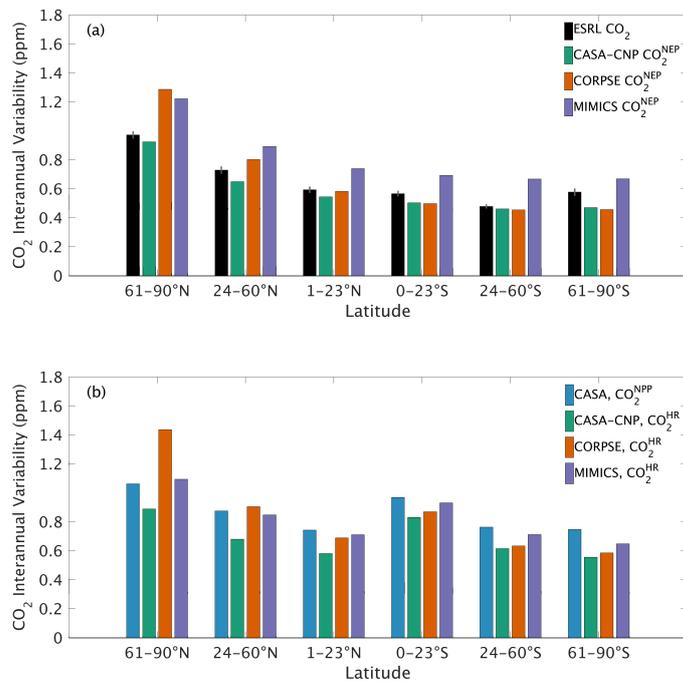
Fig. 6. Figure 6: Magnitude of CO2 interannual variability resulting from (a) individual flux components (CO2NPP IAV, CO2HR IAV) and (b) global net ecosystem productivity (CO2NEP IAV). Observed CO2 IAV from N

C20
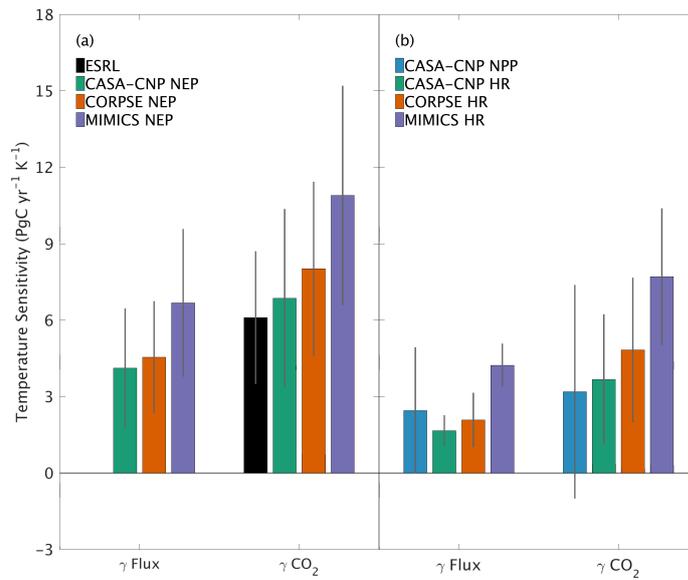
Fig. 7. Figure 7: Temperature sensitivity (γ) calculated for interannual variability (IAV) of CASA-CNP air temperature and (a) flux IAV and corresponding CO2 growth rate anomalies, (b) NEP IAV and CO2NEP grow
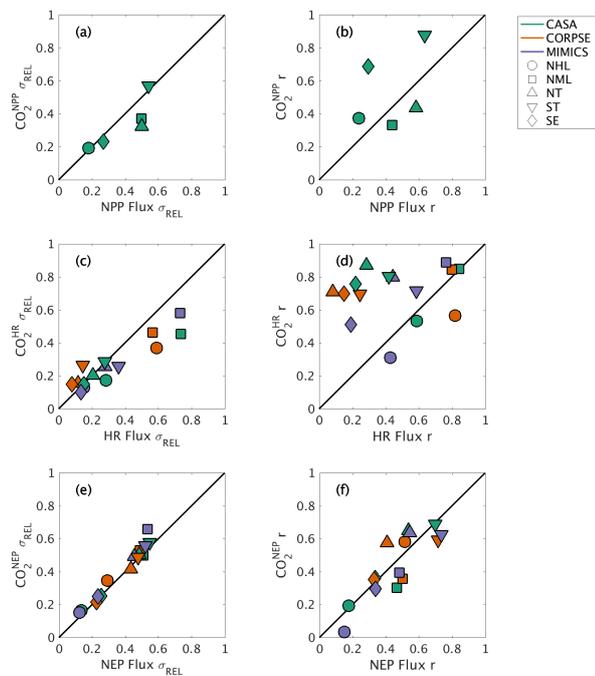
Fig. 8. Figure 8: Comparison of regional and global interannual variability (IAV) from land fluxes and resulting atmospheric CO2 between 1982 and 2010. (a, c, e) Normalized ratio taken between regional IAV an