Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-256-AC3, 2020

Biogeosciences
Discussions
Open Access

Interactive comment on "Leveraging the signature of heterotrophic respiration on atmospheric CO_2 for model benchmarking" *by* Samantha J. Basile et al.

Samantha J. Basile et al.

sjbasile@umich.edu

Received and published: 11 January 2020

Basile and colleagues compare three model formulations of heterotrophic respiration in their predictions of CO2 generation to the atmosphere, and compare the predictions with observations of atmospheric CO2 concentrations from a series of oceanic observations. I found the direct comparison of model formulations to be important and timely, given that the authors compared a CENTURY-like traditional formulation to more recent "mechanistic" models that explicitly simulate microbial processes. In some ways, this is a well-written manuscript. The text has the crisp precision is a hallmark of good scientific writing. However, the manuscript is also challenging to understand and follow, in part because it uses jargon as well as many symbols and acronyms. I suggest that the authors embed summary sentences at the end of some paragraphs throughout the results and discussion section to sum up the meaning of the results for the reader, without using acronyms (e.g., “These results suggest that the preponderance of the CO2 production driving the seasonal cycle of atmospheric CO2 originates in the southern tropical region”).

We thank the reviewer for their recognition of where this work falls in the modeling field and their feedback for more clear connecting statements and explanations throughout the paper. We have added summary sentences without jargon or abbreviations throughout the Discussion section of the revised manuscript.

Specific remarks: Line 60. I agree with the central message of this paragraph, but I would further emphasize that HR is exceptionally challenging to measure, even at the local scale. Separating soil respiration into autotrophic and heterotrophic components is possible, and it has been done well in a few places where isotopic techniques are possible on intact soils, but it has also been done poorly or with significant limitations in other places. This is, in part, because of the intrinsic linkage between microbial decomposition and root activity (i.e., exudation, allocation of carbohydrate to mycorrhizal partners). I encourage the authors to acknowledge the uncertainty in estimating HR from soil respiration fluxes, similar to their statement regarding NEE measurements (âĹijline 64).

The paragraph has been reworded as:

“Ecosystem respiration, or the combination of autotrophic and heterotrophic respiration fluxes, can be backed out from eddy covariance net ecosystem exchange observations at spatial scales around 1 km2, but with substantial uncertainty (Baldocchi 2008; Barba et al., 2018; Lavigne et al., 1997). The bulk of ecosystem respiration fluxes comes from soils, but soil respiration fluxes from chamber measurements can exceed ecosystem respiration measurements from flux towers, highlighting uncertainties in integrating spatial and temporal variability in ecosystem and soil respiration measurements (Barba et al. 2018). Further partitioning of soil respiration measurements into autotrophic and heterotrophic components to derive their appropriate environmental sensitivities remains challenging, but critical to determining net ecosystem exchange of CO2 with the atmosphere (Bond-Lamberty et al 2004, 2011, 2018). Additionally, because fine-scale variations in environmental drivers such as soil type and soil moisture affect rates of soil respiration, it is difficult to scale local respiration observations to regional or global levels (but see Zhao et al. 2017). Specifically, soil heterotrophic respiration (HR), the combination of litter decay and microbial breakdown of organic matter, is the main pathway for CO2 release from soil carbon pools to the atmosphere. Currently, insights on HR rates and controls are mostly derived from local-scale observations. Soil chamber observations can be used to measure soil respiration at spatial scales on the order of 100 cm2 (Davidson et al., 2002; Pumpanen et al., 2004; Ryan and Law, 2005). ”

Line 80. I appreciate this text directly comparing models like CENTURY to the newer, “more mechanistic” models that explicitly simulate microbial processes. Directly comparing these modeling frameworks is timely and important.

We thank the reviewer for this positive note and kept this comment in perspective with the feedback for additional model comparison.

Line 239. I am somewhat concerned by the lack of treatment of ocean CO2 fluxes, which are quantitatively large relative to the other fluxes listed here. I appreciate the following sentence, which at least partially addresses my concern. The authors might consider specifically state the assumption they are making by ignore these fluxes, which is that ocean CO2 fluxes are constant at seasonal and interannual timescales. This assumption is challenging to swallow, particularly given that the atmospheric CO2 observations were made in areas surrounded by oceans.

We acknowledge the reviewer’s concern on the assumption surrounding ocean fluxes. We have updated the text with the following statements and have added Supplemen-

Printer-friendly version

Discussion paper

tary Figures (SFig 1-2) that show the magnitude of ocean flux contributions to atmospheric CO2 in comparison with CASA-CNP CO2NEP for the Northern Hemisphere high latitudes.

“We also simulated the fossil and ocean imprint on atmospheric CO2 using boundary conditions from CO2 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 CO2 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 CO2 from ocean fluxes and fossil fuel emissions. Likewise, the IAV from fossil and ocean-derived CO2 was at most 25% that of NEP-derived CO2 at most latitude bands. These results are consistent with previous studies that have demonstrated that NEP drives most of the atmospheric CO2 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 CO2 partially cancel each other and the large uncertainty in ocean fluxes, we choose to omit these CO2 species from our analysis.”

I found the ordering of the results to be challenging to understand. I first wanted to see an assessment of the model simulations relative to the data at the two temporal scales of interest here (seasonal and interannual).

We find this point helpful to improve the readability of the paper. We have restructured the seasonality text in the Results and Discussion sections to further distinguish HR impacts. The following text was moved from the Discussion section 4.1 to section 3.1:

“Our evaluation of CO2 simulated using testbed fluxes revealed that all testbed models overestimated the mean annual cycle amplitude of atmospheric CO2 observations. In the Northern Hemisphere, the bias was largest for MIMICS, as the CO2MIMICS NEP amplitude was overestimated by up to 100% (Fig. 3). The mismatch was smallest in CO2CORPSE NEP, which was within 70% of the observed annual cycle amplitude where CORPSE simulates the largest seasonal HR fluxes (Fig. 3a-c, Table 1)."

The following text was added to the Discussion under section 4.1:

"Our evaluation of CO2 simulated using testbed fluxes revealed that all testbed models overestimated the mean annual cycle amplitude of atmospheric CO2 observations. In the Northern Hemisphere, the bias was largest for MIMICS, which had a CO2 amplitude from net ecosystem production that was overestimated by up to 100% (Fig. 3). The mismatch in the amplitude of the Northern Hemisphere NEP fluxes was smallest from CORPSE, despite CORPSE also simulating the largest seasonal amplitude in HR fluxes (Fig. 3a-c, Table1). By contrast, in the Southern Hemisphere the simulated CO2 annual cycle amplitudes were similar across all three models, with small absolute mismatches (about 1 ppm) compared to observations (Fig. 3)."

I did not find Figure 2 or it's associated text at the beginning of the results section to be useful in aiding my understanding. I am likely missing something. However, I would find the results to be structured more understandably (for me) if the current figures 3 and 4 became the first figures presented as results. That is, the authors may consider omitting figure 2, or moving it down.

We've spent some time clarifying the results associated with Fig. 2, now Fig. 4 after rearranging. But because the integrated effects of the difference between NPP and heterotrophic respiration (Fig. 4) are reflected in the CO2 NEP fluxes (Fig. 3), we still see value in presenting the component fluxes.

I was surprised by the relative lack of direct comparisons across these three models in the discussion section. I was hoping for more explicit "unpacking" of the particular model formulations, with direct recommendations as to which model components are most justifiable given the observed data. I found that much of the discussion amounted to throw-away sentences such as line 457-460, in which little of consequence was said

Printer-friendly version

Discussion paper

regarding how we should model HR.

We thank the reviewer for this comment to clarify our discussion points and add more detail for comparison between the testbed models. The discussion was extensively modified to add depth to the comparisons being made and avoid jargon and abbreviations. We also added clarifying sentences on the scope of the analysis in the introduction section.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-256/bg-2019-256-AC3-supplement.pdf

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-256, 2019.

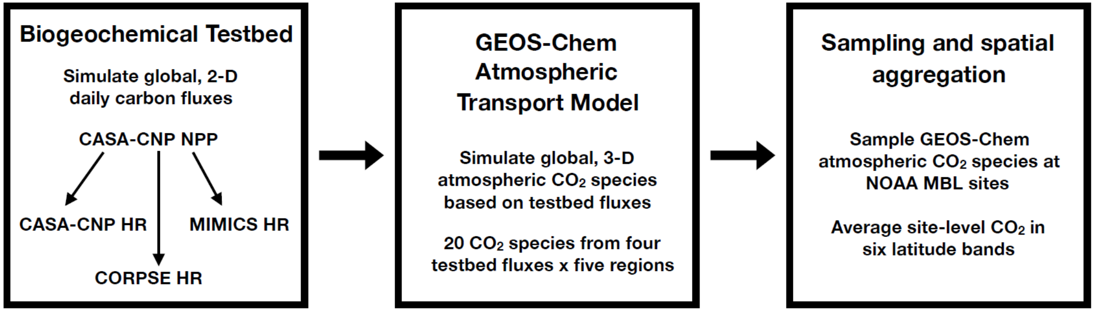

Fig. 1. Figure 1: Flow chart depiction of the analysis process from soil model fluxes to simulated CO2 concentration and comparison with NOAA observations.

Printer-friendly version

Discussion paper

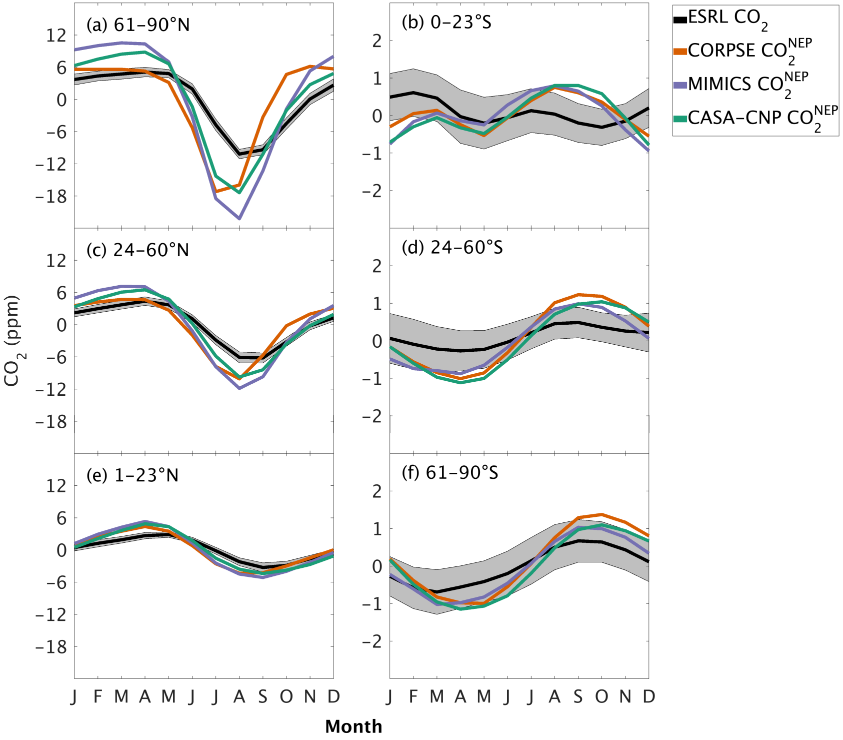

Fig. 2. Figure 2: Tagged flux regions and marine boundary layer CO2 observing sites used in our analysis. The 5 tagged flux regions are shown in color fill: Northern High Latitude (NHL), Northern Mid-Latitude

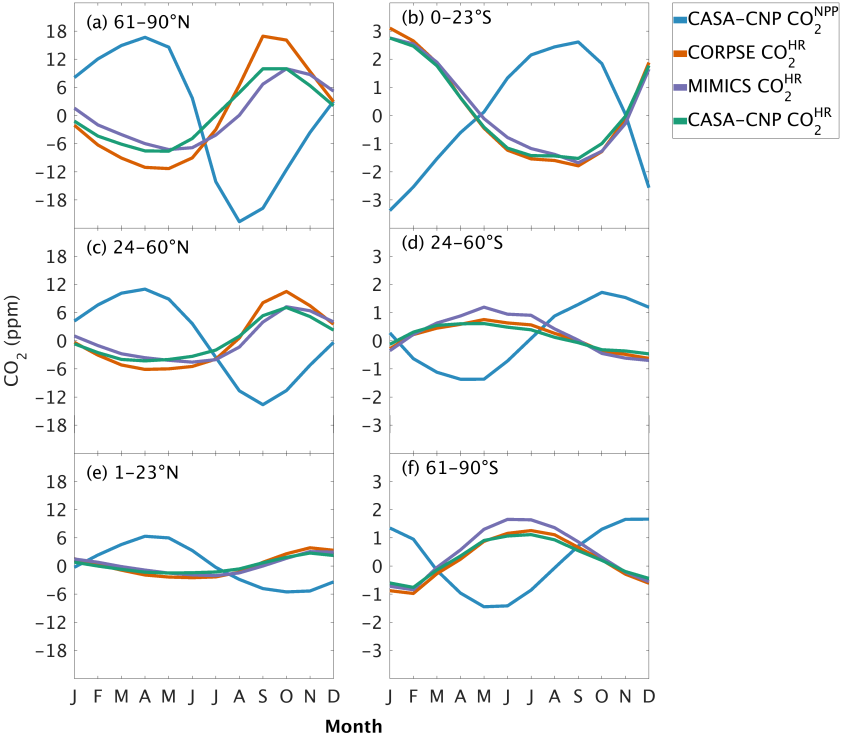

Fig. 3. Figure 3: Climatological annual cycle (median) of CO2 for observations (black) and global net ecosystem productivity flux (CO2NEP, colors) between 1982 and 2010. Monthly climatology values were create

Printer-friendly version

Discussion paper

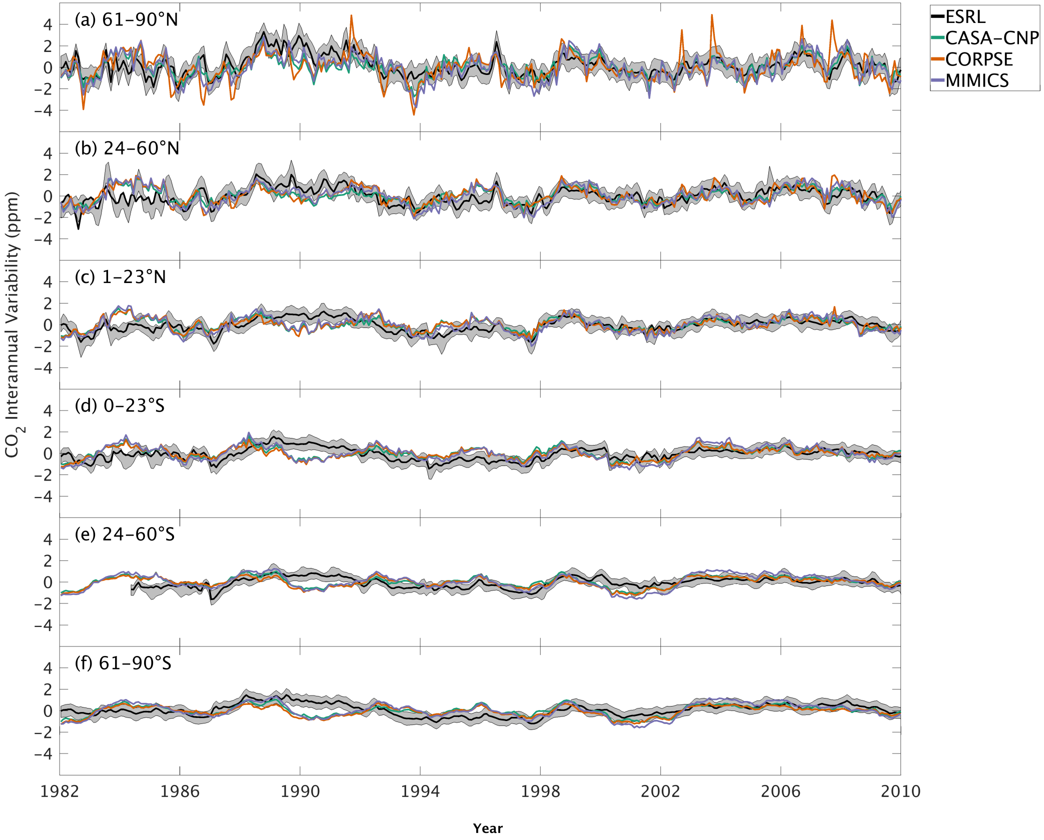

Fig. 4. Figure 4: Climatological annual cycle (median) of atmospheric CO2 simulated from land fluxes (CO2NPP, CO2HR) between 1982 and 2010. Monthly climatology values were created after detrending the CO2 tim

Printer-friendly version

Discussion paper

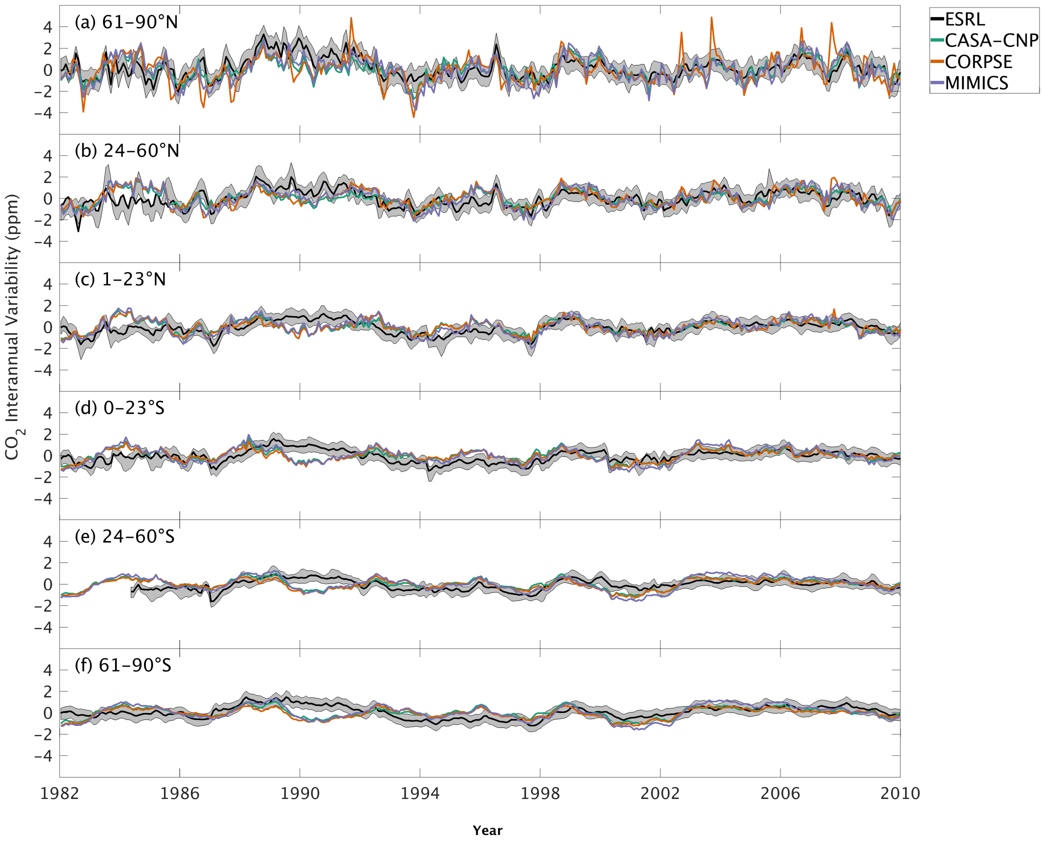

Fig. 5. Figure 5: Interannual variability of CO2 from global net ecosystem productivity (CO2NEP IAV) for testbed models (colors) and marine boundary layer observations from the NOAA ESRL network (black). Gra

Printer-friendly version

Discussion paper

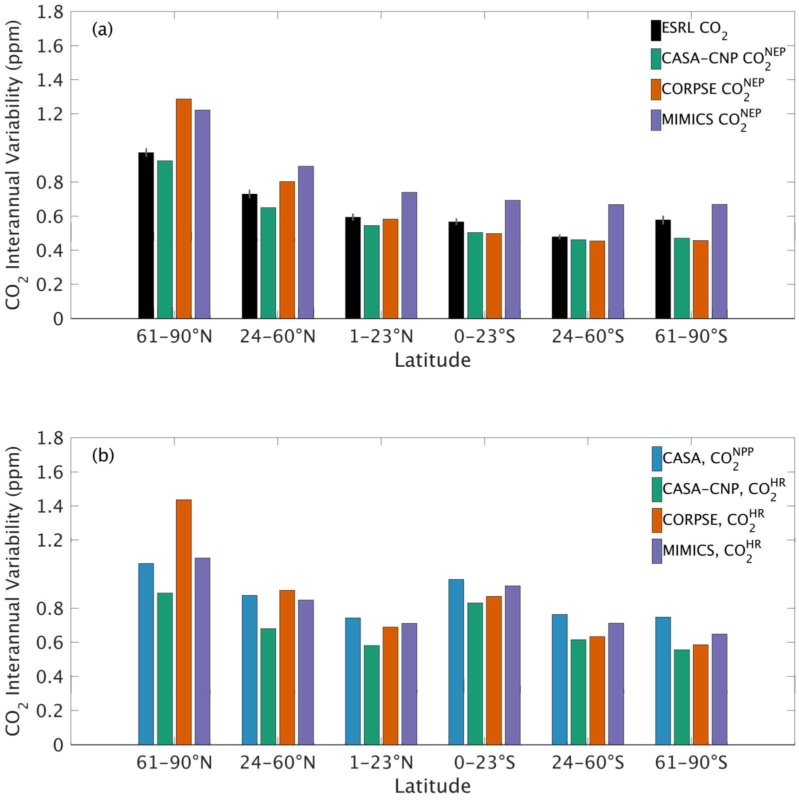

Fig. 6. Figure 6: Magnitude of CO2 interannual variability resulting from (a) individual flux components (CO2NPP IAV, CO2HR IAV) and (b) global net ecosystem productivity (CO2NEP IAV). Observed CO2 IAV from N

Printer-friendly version

Discussion paper

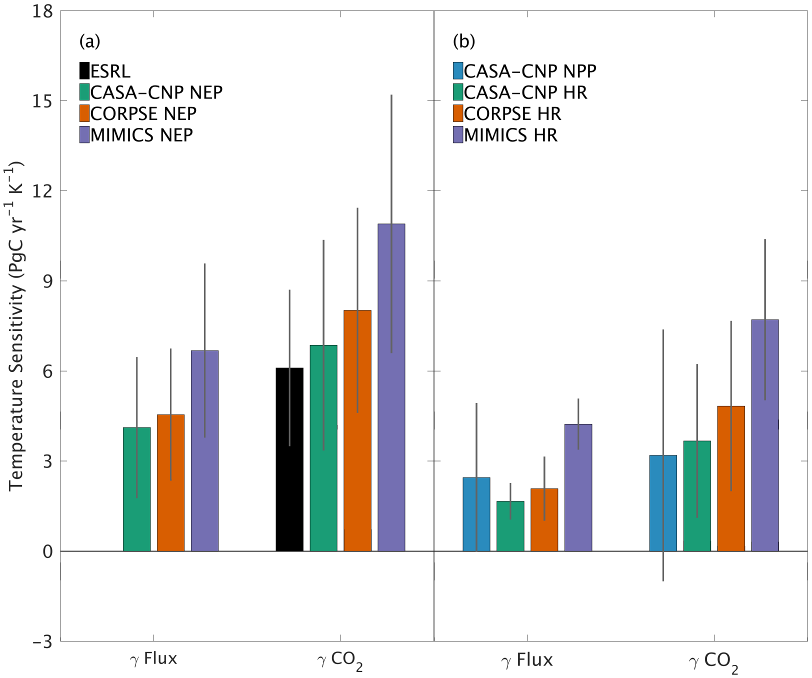

Fig. 7. Figure 7: Temperature sensitivity (γ) calculated for interannual variability (IAV) of CASA-CNP air temperature and (a) flux IAV and corresponding CO2 growth rate anomalies, (b) NEP IAV and CO2NEP grow

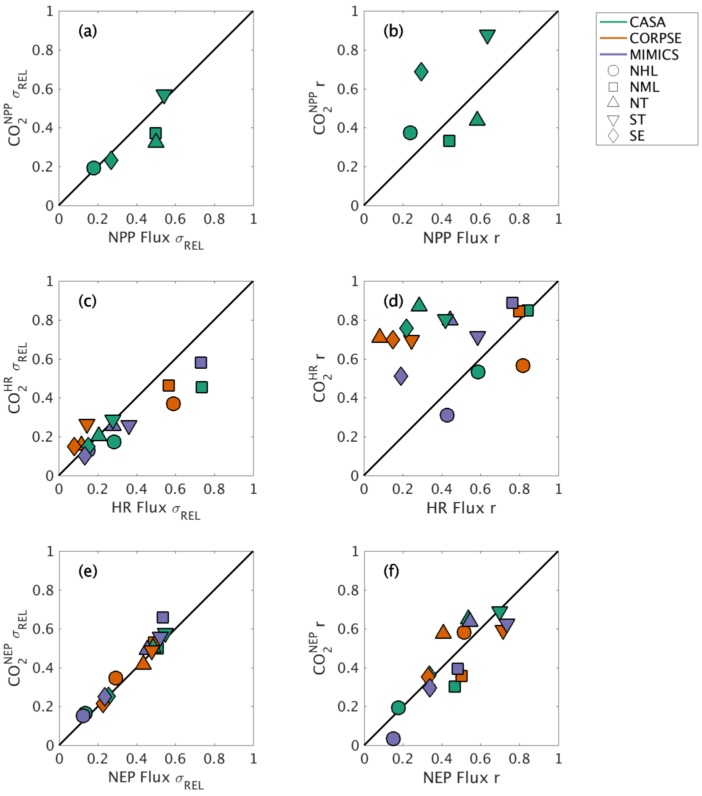

Fig. 8. Figure 8: Comparison of regional and global interannual variability (IAV) from land fluxes and resulting atmospheric CO2 between 1982 and 2010. (a, c, e) Normalized ratio taken between regional IAV an

Printer-friendly version

Discussion paper