# Response to Referee Comments on "'Drivers of column-average $CO_2$ variability at Southern Hemispheric total carbon column observing network sites'"

December 2, 2013

We thank the referees for both taking the time to read and review the paper, and appreciate their input. Below we address their comments, which are italicised for clarity.

**Anonymous Referee #1**

*The chief conclusion of this work seems to be that the seasonal cycle of column averaged CO2 (XCO2) in the Southern Hemisphere is mostly determined by the terrestrial biosphere, although biomass burning emissions can be important as well. Specifically, at Darwin, the mean seasonal cycle is mostly determined by the surface CO2 flux from the Australian Tropical region. By itself, the first conclusion is neither new nor puzzling. We already know from existing flux estimates, including CT2011 (which is used by the authors), that the biosphere and biomass burning components together constitute the largest seasonal variation. Therefore it is unsurprising that those components should show up as the majority contributors to the mean seasonal cycle (MSC) of XCO2. The decomposition of the twin-peaked MSC at Darwin as a single-peaked time series from the Northern Hemispheric biosphere added to a double-peaked signature from Tropical Australia is interesting, but to my understanding that is the only interesting result presented in this manuscript. Of the other novel ideas presented, none have been proved concretely, as I will explain below.*

Yes, it is *unsurprising* that the seasonal cycle of $X_{CO_2}$ in the southern hemisphere is dominated by the terrestrial biosphere, with some contribution from biomass burning. We are, however, unaware of anywhere that this has been previously shown, and think that it is important to investigate and publish these results. The southern hemisphere especially has not received very much attention, and from that point of view we consider that the results presented here are in fact interesting, regardless of whether they could be considered to be surprising.

We agree that the double-peaked seasonality observed at Darwin is interesting, but disagree that it is the only interesting result presented in the manuscript. This will be addressed below.

*The authors use posterior CO2 concentrations of CT2011 to distinguish between the MSCs due to the biosphere and biomass burning. However, as they themselves acknowledge (P14341, L1), it is impossible for a data assimilation system such as CT to disentangle terrestrial biosphere fluxes from biomass burning ones, and errors made in prescribing the biomass burning fluxes are aliased into estimates of biosphere fluxes. In areas with frequent fires such as South East Asia (which is relevant for the signal at Darwin), it is quite possible for GFED to underestimate biomass burning emissions. Therefore, the argument the authors make in section 4.1 (and throughout the manuscript) by separating biosphere and biomass burning fluxes stands on very shaky ground at best.*

We are, as mentioned, aware of the difficulties in differentiating between co-located source/sink processes in an inversion or data assimilation system. In particular, we are also aware of the difficulties in estimating biomass burning emissions. One of the strengths of the FTS measurements is the ability

to retrieve multiple gases, allowing for a multiple tracer approach to diagnose particular processes. This is why we use measured $X_{CO}$ as a tracer for biomass burning to assess the BB fluxes used in this study. In particular, in Figure 9 of the discussion manuscript, we show that from the South-East Asian and Northern Australian biomass burning fluxes, the seasonality is well-captured, and the magnitude is of a reasonable order (though as the reviewer points out, this could still be under-estimated). Therefore, we think that the separation between biosphere and biomass burning fluxes is at least reasonable, though naturally not perfect. We have emphasised this by moving Figure 9 and its original discussion to the earlier section on Biomass Burning (4.1.2), and re-stressed that some biomass burning signal could be aliased into the optimised terrestrial biosphere fluxes.

*On pages 14346 and 14347, the authors point to the early onset of the 2005 monsoons as a factor behind enhanced photosynthesis in 2006 (the so-called relief of waterstress), resulting in a shallower XCO2 maximum in the middle of 2006. However, it is not at all clear to me why an early onset of the monsoons in November 2005 should relieve water stress in July/August 2006. In fact, in figure 11 I see a shallower minimum in Feb-Apr 2006; does this mean the earlier onset of the monsoons causes less productivity in the following months?*

It is not clear to us how the referee has reached the conclusion that we are referring to July/August 2006, when we are specifically discussing the period in January-March 2006 (referred to as "'early 2006"' in line 17, p14344). As a result, we have revised the wording of the manuscript to re-iterate that we are referring to the period of model-measurement mismatch seen in early 2006. Note that we are referring to the real world in this discussion, not the fluxes as inferred via CarbonTracker. The hypothesis is that the CarbonTracker data assimilation system fails to increment the tropical Australian flux in a way that may be expected from the IAV in rainfall, so the response of the CT fluxes does not reflect this.

Regarding the apparent fluxes presented in Figure 11, as CarbonTracker does not seem to take into account interannual variability due to the monsoon onset we cannot say from these fluxes what the effect of an earlier monsoon onset might be on productivity in later months. We do expect that the initial growing after monsoon onset, which will also see regrowth after the regular northern Australia biomass burning season, will be the most productive, and that productivity will reduce after this initial burst. As mentioned by Referee 2, the increased photosynthesis is expected to be somewhat offset by increased respiration after relief of water stress.

*The authors try to pinpoint the factors behind the model-observation mismatch of XCO2 at Darwin, and ultimately pin it on surface fluxes from Tropical Australia. All this is done, however, with a single flux inversion (CT) using a single transport model (TM5). There is no attempt to see whether this problem at Darwin is systematic across different inversion frameworks and different transport models. The authors could have used the inversion products from http://transcom.lsce.ipsl.fr/, for example, to check whether the CT results are typical of other inversions or not. As it stands now, the manuscript*

*reads more like an evaluation of the CT posterior fields and less like a significant statement about our ability to estimate surface CO2 fluxes, or our ability to model column averaged CO2. Moreover, if the Australian Tropical flux in CT2011 is the culprit, how come the surface time series at Cape Ferguson (http://www.esrl.noaa.gov/gmd/ccgg/carbontracker/co2timeseries.php?site=CFA_02D0&year=all#imagetabl does not show big discrepancies as well?*

Firstly, we agree that looking at this with other models and inversion frameworks is indeed interesting, and this is something that we had planned to tackle in the future. It is true that we have solely assessed the CarbonTracker results, however, we have not simply used one transport model, but rather two - TM3 and TM5 - which yield very similar results in the total columns. This has also previously been shown by Houweling et al (2010), including for Darwin. A quick look at the Transcom website and the RECCAP paper by Peylin et al (2013), shows that the CT results are not atypical for inversion products. Given the comments here from both referees, we decided it was most interesting to extend the study to compare results for prognostic (SiB) and analysed (CT2011oi) terrestrial biosphere fluxes, and have therefore included analysis of tagged tracer runs using the Simple Biosphere model to provide the terrestrial biosphere fluxes.

Cape Ferguson is located on the eastern coast of Australia, towards the southern end of the tropics. The sampling strategy there, as for most flask sampling sites, is designed to sample baseline air. As such, samples are collected when there is an ocean breeze, therefore sampling from a fetch that does not include immediate influence from the Australian tropical land region. Therefore we do not expect to necessarily see a discrepancy there, even in the case when "'local"' biosphere fluxes are not well-estimated. Also, being in the tropics, convection plays an important role, especially when measuring air that does not come directly from the source region in which we are interested.

*P14334, L13. "'Vertically integrated CO2 ... than in situ measurements."' This is a common-sense argument, but not necessarily true. While a wrongly modeled boundary layer will cause less of an error in the column averaged CO2, the signature of surface fluxes in the total column is also smaller than at the surface. Thus, compared to the size of the signal one is trying to extract from the measurements, errors in the total column CO2 can be just as important as errors in the boundary layer CO2. This is compounded by the fact that the CO2 signal travels quite fast laterally in the free troposphere, thus wrong boundary layer modeling can introduce errors in surface fluxes far away from the XCO2 measurement location.*

We think that the referee is objecting to the second sentence in this paragraph "Thus surface flux inversions based on column abundances are expected to have reduced sensitivity to errors in modelled vertical transport.", in which case we can understand the objection. We have rephrased the paragraph to emphasise that column measurements are expected to provide complementary information to surface measurements in constraining surface fluxes using inversions. The first sentence is kept in the manuscript, because it only refers to the measurements themselves, not to a model's ability to reproduce them.

*P14335, L6. Why is the underestimation only of the strength of the boreal seasonal cycle and not of the temperate as well?*

This is based on a summary of the conclusions drawn in previous studies (Keppel-Aleks et al, 2011, 2012; Yang et al, 2007), which postulated that boreal seasonality is underestimated, and also that this is indeed the most important driver of the seasonality in column measurements. This is not to rule out the effect of temperate regions, though the previous work suggests that an under-estimate in those regions is less likely to be a problem, and they impart a less significant signal to the XCO2. The work of Keppel-Aleks et al (2011), and the fact that seasonal cycle amplitude scales linearly with the total Northern Hemisphere fluxes, confirms that hemispheric-scale fluxes determine the seasonal cycle amplitude in XCO2.

*P14337, L18. Why is the model sampled at 00:00 UT and not co-sampled with TCCON measurements?*

The model was sampled at 00:00UT because this is during the daytime (hence measurement time) for these sites, as well as to limit the volume of model output necessary. In addition, when looking at monthly averages in column data, adding model output more frequently than daily does not result in significant differences. We assessed these differences using the full (90 minute) temporal resolution CT model CO2 fields, and found them to be less than 0.1 ppm in $X_{CO_2}$. We have added a sentence to the manuscript emphasising this.

*Section 4.2 (and 5.2). I do not follow how the "'fossil fuel"' tracer can diagnose interhemispheric transport (IHT). The experiment performed by the authors only shows the impact of IHT on XCO2 at the sites given the source patterns corresponding to fossil fuel emissions. Biosphere and biomass burning fluxes have very different source patterns, so the impact of IHT on XCO2 could be different. If the goal was instead to evaluate whether the TM5 IHT was close to the "'truth"', then why not use an SF6 simulation?*

The intention here was to take an existing piece of information that we had at hand to inform us about this. In this case, the fossil fuel tracer is a good approximation of an $SF_6$ simulation, because the seasonality in these prescribed fluxes is small (seasonal cycle amplitude of approximately $\pm 5\%$). In addition, following the work of Keppel-Aleks et al (2011), we expect the column to be sensitive to hemispheric scale signals, and therefore the geographic distribution in the opposite hemisphere to be a second order effect. The referee is, however, correct that our diagnosis is dependent on the spatial and temporal patterns in the fossil fuel fluxes, even though we expect this dependence to be small. Taking this into account, we have removed the figure and discussion regarding the northern hemispheric fossil fuel tracer and at the request of Referee 2, add some additional discussion regarding dynamics. We also cite papers that used SF6 simulations to show that the TM family of models have a realistic IHT.

*P14348, L11. Why is a 40% underestimation in the strength of the uptake unrealistic? Isn't this within the ballpark of the underestimation found by Yang*
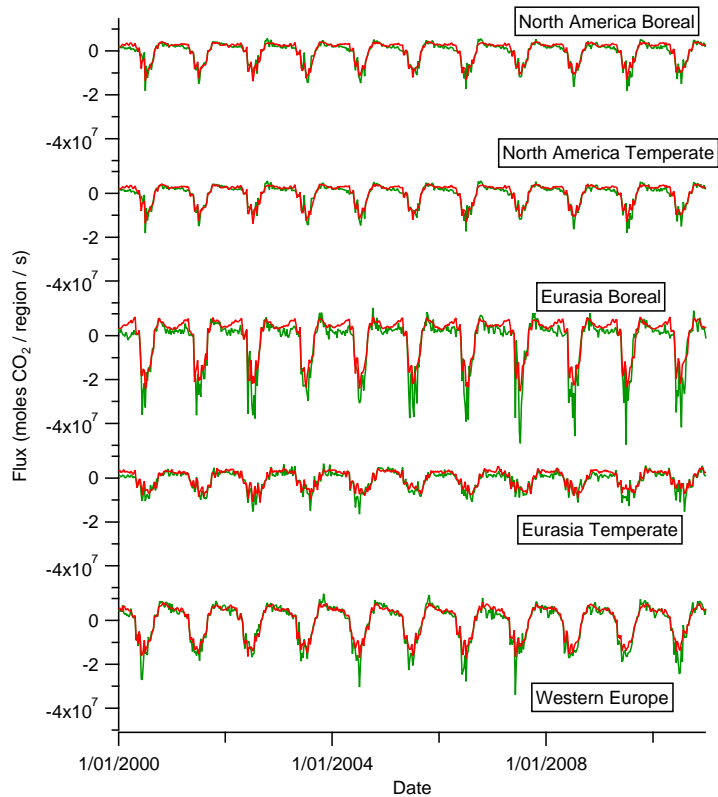
Figure 1: Prior (red) and optimised (green) CarbonTracker terrestrial biosphere fluxes for the northern hemisphere boreal and temperate regions.

*et al (2007)? CT does not assimilate any measurements from boreal Eurasia, so I can imagine such an underestimation in the uptake by that region.*

Yang et al (2007), they postulate an underestimation of only 25% of the seasonal cycle strength. Also, their work is based on unoptimised CASA fluxes, and therefore could be considered something of an upper limit (assuming that any incrementing of the fluxes causes the seasonal cycle to be increased). The figure of 40% comes from the work of Keppel-Aleks (2012), also based on unoptimized fluxes.

Figure 1 shows the prior and optimised fluxes for the northern hemisphere boreal and temperate regions. One can quite clearly see that the boreal Eurasian region especially is incremented. Of course this is no guarantee that the optimised fluxes are correct, but given that these are large compared to the other regions, one would expect that any errors of 40% magnitude would show up strongly in column observing sites that are exposed to a fetch from this region. This is not the case (see e.g. Wunch et al, 2013) at Park Falls. To note also, CarbonTracker agrees well with other RECCAP models for this region (see transcom.lsce.ipsl.fr).

**Anonymous Referee #2**

*The paper is clear and well-written, and is an important step in understanding how to utilize column CO2 observations for carbon cycle science. I would suggest that more analysis on how the column observations provide different information from southern hemisphere surface CO2 observations would make this more clear. The authors state that this will be part of a future analysis, but from the manuscript it is unclear what are the new findings that may allow total column CO2 to be used differently than previous data streams.*

In accordance with this we have added some analysis comparing the surface and co-located column output. Unfortunately the in situ surface observation time series at the SH TCCON sites are not sufficient to significantly overlap with the current CT model output, and also require further quality control, which we think is definitely outside the scope of this manuscript. Instead, we have specifically commented on differences in contributing factors to variability between the surface and column measurements. We do this firstly with reference to a paper from Nevison et al (2008), which looks at process contributions to surface CO2 variability, but also with a supplement showing and discussing the surface process drivers at the SH TCCON sites. The TM3 and TM5 surface level simulations, unlike the columns, do not agree well, and we therefore resist making too many quantitative conclusions.

*A major problem with this paper is that it is quite tied to CarbonTracker results, which result from assimilating surface CO2 observations. Because surface observations are sparse in the southern hemisphere, the assimilation of atmospheric observations does not yield much added information about fluxes, as the authors acknowledge. Therefore, given the large inherent uncertainty in how CarbonTracker partitions fluxes, it would have been nice to see additional model runs using biospheric fluxes from other biogeochemical models, particularly those that do not use atmospheric CO2 observations in the flux estimate. To a certain extent, the authors address the lack of generalizability of Carbon-Tracker fluxes by using the monthly pulse fluxes to represent biospheric fluxes in the tropical Australian region, but I don't think the analysis has gone far enough.*

We are aware of the limitations of using only CarbonTracker results, and it is certainly interesting to look at additional model runs. The aim of the paper was to show that the southern hemisphere column observations yield information that is not necessarily included in data assimilation systems like CarbonTracker. Given the lack of observational information in the southern hemisphere, this effectively means that the underlying biosphere model (in this case GFED-CASA) also does not correctly capture this. We have, however, also performed simulations using the SiB biosphere model NEE fluxes with TM3, and included analysis of this alternative model simulation in the revised manuscript.

*Analysis of how transport patterns, particularly tropical transport patterns, affect XCO2 would have been particularly useful and would make the results from this paper more broadly applicable for interpretation of XCO2 observations from satellites as well as other upcoming tropical measurements. For example, South Africa and South America are grouped together as "other" remote tropical fluxes, but understanding seasonal and interannual variations in tropical forest*
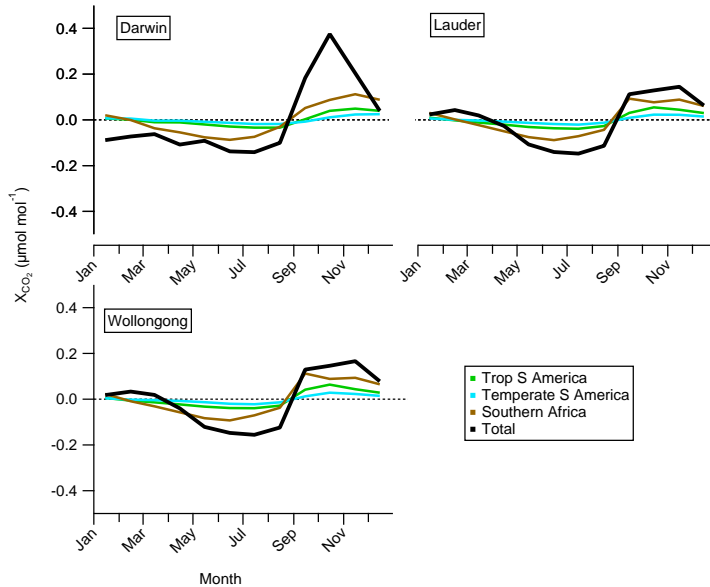
Figure 2: Mean seasonal cycles in South American and South African biomass burning signals as seen at the TCCON sites.

*and savannah fluxes on these two continents is an important goal. If the authors could demonstrate whether tropical fluxes from different continents leave unique imprints on the XCO2 at Darwin site, or in the other southern hemisphere TC-CON sites, which are much "quieter" than the northern hemisphere sites, this would be a very compelling and unique use for column observations. Such analysis would likely require using a transport model outside the TM3/TM5 family since convection patterns in an individual model would likely be a significant determinant of how remote tropical fluxes impact the XCO2 at TCCON sites.*

We now include figures showing the relative contributions from each of the individual regions that comprise Southern Africa and South America. Individually, the signals are small (peak-to-peak seasonal cycle amplitude of 0.3 - 0.4 ppm), however, tropical South America terrestrial biosphere fluxes impart a signal out of phase with temperate South America and southern Africa. The main reason that we combined these regions is because of the similarity in the phase of the biomass burning signals imparted at the TCCON sites. Figure 2 illustrates this in the mean seasonal cycles. We agree, however, that the separation of these regions is useful. Nonetheless, is remains very difficult to distinguish errors in the fluxes because of the interplay of the phasing from the different regions.

Previous studies, such as Houweling et al, 2009, show that TM3 and TM5 transport differences from other models are small, including at Darwin. We could conceivably include other models providing these runs were done for us because we only currently have the knowledge and ability to run the TM3 model.

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However, to the best of our knowledge the runs within TRANSCOM with identical fluxes do not save full column output at Darwin, compromising their use for this study. They are also not resolved by region. In our opinion, extending the tagged tracer aspect to another model is unrealistic for this publication, as much as it would be interesting to analyse the dynamics.

To address the question of dynamics, we also perform some comparative analysis of a model simulation with identical fluxes but fixed 2001 meteorology to disentangle the relative influence of fluxes and large-scale dynamics on interannual variability. A paragraph of discussion is included regarding dynamics and addressing this. In brief, the influence of dynamics is largest at Darwin in the monsoon months, largely depending on the relatively southern oscillation index and the subsequent influence on the location of the ITCZ. This makes Darwin different from the two extra-tropical sites. Outside these months (Jan - Mar), the influence of dynamics is relatively small. The IAV due to transport during monsoon periods at Darwin does mean that dynamics could play a role in the 2006 anomaly.

*The authors provide a justification for considering only variations greater than 0.4 umol/mol "detectable" based on a 0.1 umol/mol precision when averaging across a day and a 0.3 umol/mol error from the airmass correction. This threshold is not so much used in this paper as introduced conceptually, but I wonder what its actual implications are. For instance, the authors suggest that the biomass burning signals are generally undetectable, but in Fig. 6: the seasonal amplitude (peak - trough) for biomass burning at Darwin exceeds 0.4 ppm. Since most of the error (0.3/0.4 umol/mol) is systematic error from the airmass correction, the data could be analyzed and interpreted to minimize the influence of this correction. For instance, at Wollongong, taking the difference in XCO2_bb between months with similar solar zenith (say, March and October) angles/airmasses would yield a signal greater than 0.1 ppm (and by eye it looks like this is even the case at Lauder). Additionally, interannual variability would be detectable at XCO2 variations of 0.1 umol/mol because the airmass correction should be the same for a given month from year to year.*

It is true that one can consider only data acquired at similar solar zenith angles to remove the uncertainties due to residual airmass-dependent biases. However, this has other implications for our "detectable" signal, most obviously that it reduces the amount of data available. Purely applying a time-based filter does not ensure that identical solar zenith angles are sampled in the same fashion. We prefer to work towards a means of accounting for uncertainties in using as much available data as possible from all sites.

Nevertheless, it is indeed interesting to note that with a proper knowledge of the characteristics and limitations of a dataset, it can be potentially more useful than when simply used in a blind fashion.

There are two parts to the argument of whether something is "detectable", about which we maybe were not completely clear. Firstly, there is the measurement repeatability factor, as discussed by the referee. Secondly one needs to consider the detectability of real world departures from the a priori surface flux

estimates, rather than the a priori flux signatures themselves.

*In Section 5.0, the authors state that the IAV during May-September is quite small at Darwin, corresponding to the dry season. Is this flux-driven or also transport-driven?*

A combination. The interannual variability in the change due to the northern hemisphere fossil fuel tracer (Figure 8 of the original manuscript), where the fluxes are reasonably stable, suggests that the interhemispheric transport differences at this time are small. The transport time from the northern hemisphere, which has the largest flux signal, means that this also corresponds to fluxes occuring there during the winter. From Figure 1 above, one can see that the interannual variability in these fluxes is small during the winter, and indeed the IAV is dominated by the uptake. The additional discussion and figures in the revised manuscript regarding dynamics also suggest that this plays a role that is relatively invariant from year to year in these months.

*In Section 5.2, it is worth noting that earlier rainfall may also lead to enhanced and earlier increases in heterotrophic respiration, partially counteracting the effect of rainfall on stimulating photosynthesis.*

Noted.

*In the discussion of biomass burning (Section 5.4), the authors use the term "remote" and "local", but don't define what they mean. It would seem that Indonesian fires are much more "local" to Darwin than to the SH midlatitude site and would potentially leave a larger and differently phased signal at Darwin than at Lauder or Wollongong, in contrast to what is stated on 14349.*

True. We implicitly considered that Indonesia (specifically South-east Asia) was a "local" signal, and referred to the northern hemisphere and south Africa and South America as remote. We have clarified this.

*In the conclusions, the authors could be a bit more quantitative as to the influence of the local, remote, and NH TB on the mean annual cycle amplitude at each site.*

We have attempted to be more quantitative regarding the relative influence of the different tracers.

*Fig. 9: Why the residual BB enhancement in XCO2 in 2007 but not 2006?*

This occurs because of the long lifetime of $CO_2$ in the atmosphere. That means that in a year with a high anomaly compared to other years, this persists for some time afterwards. The effect of the large late-2006 fires is also visible to some extent in 2008.

*Fig. 1: the green circles for the TCCON sites does not show up well against the gray background. Perhaps use a different color or larger symbols.*

Thanks. We have used yellow symbols instead of green.

*Fig. 2: Vertical gridlines, extending up from each 1 January tick mark on the x-axis would be helpful.*

Done

*Fig. 3: Nice figure – maybe consider in the Darwin plot adding a thin trace for the 2006 anomalous year that becomes a large focus later on in the paper, so the reader can easily see how this year differs from the observed mean annual cycle and the simulated mean annual cycle?*

Thanks. We have added the 2006 trace for Darwin.

*Fig. 4: The dashes in the interannual variability subpanels make it harder on the reader – use solid lines or try shading around the lines in the main subplots?*

Shading around the lines makes the main figure too complicated (and my plotting software doesn't handle this!). We have reverted to solid lines in the upper panels

*Fig. 5: It would be nice if the local Australian fluxes stood out. Consider using warm colors for local fluxes and cool colors for remote fluxes, or making the local fluxes a slightly thicker line.*

Done.

*Fig. 7: Panels are reversed relative to the figure caption.*

We have changed the order of the references to the panels in the caption, as well as including parenthesised comments stating that they are the upper and lower panels.

*Fig. 8: It might be nice to subtract off (or add a dashed line indicating) the NH mean dXCO2/dt trend to more clearly see when the growth rate is above/below average.*

We have added a dashed line so as not to lose information regarding the magnitude.

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