We would like to thank reviewer #1 for the detailed review of our manuscript and the thoughtful suggestions that will help to improve our manuscript. In the following, we will answer each of the reviewers comment.

Zscheischler et al. pull together a variety of surface to air CO2 flux estimates and ask the question "Do these add up to a globally balanced budget?" This is a worthwhile effort, and the authors are using state of the art estimates. As alluded to in the text, the primary goal of this work is to create a combined data product that can be used as input to future data assimilation efforts. Unfortunately, there are vital errors the analysis. Large annual cycles of CO2 flux are taken into account for land, but entirely ignored for the ocean.

We believe that that this is a misunderstanding. We do consider annual cycles of all variables – where these were available. And these were indeed available for most land and ocean area. State-of-the-art observation-based estimates of shelf areas and inland waters are however still missing the seasonal representation. This kind of gap analysis is exactly what we intend to do here: demonstrate where we currently miss information to achieve a comprehensive and purely data driven description of the surface-atmosphere CO2 exchange. We believe that this is the best way forward to improve our future understanding and fill these knowledge gaps.

The authors suggest that they are looking at the full "background" of natural CO2 fluxes, but only consider the anthropogenic perturbation in the ocean. To be correct and consistent with the statements of a full accounting for natural background fluxes, Table 1 and Figure 1 should have large fluxes in the ocean that are of the order of the GPP and TER on land. Furthermore, just assembling these data-based estimates into one with global coverage is not sufficient for publication.

To the best of our knowledge this is the first critical appraisal of data driven estimates of surface-atmosphere CO2 fluxes, which may be relevant for wide community working in C cycle science. For the ocean we provide estimates of the contemporary carbon fluxes, i.e. a combination of natural and anthropogenic fluxes. Based on surface ocean pCO2 observations the natural and anthropogenic components cannot be separated (see also below).

The analysis here is too thin, and the findings are poorly presented. Based on how the independent products have been produced, no one should expect that they would add up to a balanced budget – this finding is no surprise.

Indeed, we don't expect the readers to be surprised that individual components do not add up – but we assume that e.g. the spatially explicit description of the data uncertainty and budget mismatch is of key importance to guide future research efforts. I.e. while it is no surprise that the numbers don't add up, it is of much more importance where they don't add up and where we have today the largest observational knowledge gaps and uncertainties.

The authors do not do enough to explain what are the major sources of the uncertainty, nor do they do enough to make it clear how they estimate this uncertainty.

We apologize if we didn't achieve a sufficient description of how we estimate uncertainty. To improve the presentation and to make it more clear how we derive our uncertainties, we will adjust the text and include a visual description of the work flow (see illustration at the end of this document).

They need to do a lot more with the products that they have before this manuscript is acceptable for publication.

We agree, that the data we present here would allow much more analysis and we would encourage the community to use the datasets and add additional analysis. Overall, however, we would like to reemphasize that the main aim of this study is not to simply check whether data-driven surface-atmosphere CO_2 fluxes add up to a balanced budget. Given the difficulties of guaranteeing a consistent and contiguous global C monitoring system, this simply cannot be expected. And the current data-driven knowledge about many of the relevant fluxes cannot compensate this. But – and we find this an important contribution – we provide global spatiotemporal estimates of the net carbon flux combining a variety of heterogeneous datasets and consistently propagate their uncertainties. Through our approach we can identify regions of high and low uncertainty, guiding new monitoring campaigns and novel scientific approaches to reduce specific uncertainties. Our NCE estimates specify contemporary fluxes over the whole Earth surface, thus including background and anthropogenic fluxes, as explained below.

Major Comments

1. The authors indicate that their goal is to not just address anthropogenic carbon uptake, but to also address the background carbon fluxes (Page 3). Yet their methodology is inconsistent across land vs ocean in this respect. While on land, they separate GPP uptake of CO2 from TER efflux, they completely ignore the comparable cycle in the ocean. See Figure 6.1 of Ciais et al. 2013 (IPCC WG1, Chapter 6) where it is clear that the naturally occurring cycle in the ocean creates an exchange flux of 80 PgC/yr out of the ocean and 78.4 PgC/yr into the ocean; this is comparable in magnitude to the GPP and TER (+-100 PgC/yr), but the authors here simply ignore these ocean fluxes by only presenting their sum. They also appear to ignore these large fluxes in their assessment of uncertainty (though detail on how uncertainty is accounted for is so thin that it is hard for the reviewer to be sure on this point). The full background cycle in the ocean must be included in this analysis must be remedied in this analysis.

The reviewer is correct that we have only displayed aquatic net fluxes (not only for the open ocean but throughout the whole aquatic system), which was simply an effect of data availability. We also concur that the gross fluxes may have individually larger uncertainties attached to them. We do, however, disagree, that we „ignore“ these fluxes or their uncertainty. The uncertainty of the net flux presented in this study is comparable to other uncertainty estimates such as the IPCC report or the Global Carbon Budget (e.g. Le Quéré et al 2015). The reason for the smaller net uncertainty stems from the correlation between air-sea and sea-air fluxes and their uncertainty. The largest source of uncertainty between individual flux elements (sea-air or air-sea) stem from the gas transfer formulation (see also answer to a more specific comment below), i.e., a systematic source of uncertainty which likewise effects fluxes in both direction leading to a much smaller net flux difference and attached uncertainty. This is also stated in the caption of the IPCC mentioned by the reviewer: "Individual gross [air–sea exchange] fluxes and their changes since the beginning of the Industrial Era have typical uncertainties of more than 20%, while their differences (Net land flux and Net ocean flux in the figure) are determined from independent measurements with a much higher accuracy (see Section 6.3). Therefore, to achieve an overall balance, the values of the more uncertain gross fluxes have been adjusted so that their difference matches the Net land flux and Net ocean flux estimates."

Over land we have used TER and GPP because these fluxes are available at the spatiotemporal grid which we required. This is not the case for the ocean. However, we agree with the reviewer that this is inconsistent. In the revised version, we will therefore only use the directly upscaled NEP product from FLUXCOM, which will reduce the sample size of the NEP estimates from 16 to 8 (the uncertainty related to the flux separation (split of NEP into GPP and TER) will be dropped, as it is not relevant for *the uncertainty estimation of the net fluxes). The uncertainty in the upscaled NEP product is 2.1PgC/yr (compared to 3.4PgC/yr when using TER-GPP), which is still much larger than the uncertainty of the net flux over the ocean (0.15PgC/yr). Using directly upscaled NEP leads to similar global mean estimates than using TER-GPP (the difference is <0.7PgC/yr, i.e., <5%).*

In response to the reviewer's comments we will also add above explanation regarding net uncertainty in the text. We do however avoid the term „background fluxes" as this term can be easily confused with „natural fluxes", whereas we cannot separate natural and anthropogenic components. All our flux estimates are the aggregates of natural fluxes and anthropogenic disturbance. We address this in more detail below, in direct response to an additional comment by this referee.

2. A coherent explanation for the large imbalance in the final "budget" is never presented, instead the reader is left is a laundry list (e.g. page 16) of possibilities and no clarity of what the authors have identified as the likely most important uncertainties. It seems quite likely that the large GPP and TER fluxes, or the comparable ocean fluxes, are biased high or low. Their uncertainties are the only ones on the same order as the NCE uncertainty. This issue is even more obvious at the regional scale. This issue should be more directly addressed.

In the revised version we will more clearly emphasize and discuss the most likely reasons for this imbalance. In our opinion it is most likely a combination of

- *i) a bias in NEP, most probably in the tropics where only very few eddy covariance sites lead to a weak observational constraint.*
- *ii) missing sources (as listed in section 4.4), especially emissions from wetlands and VOCs.*

a. One clear place to do this would be on Page 12, where it is stated that in 13% of their runs, the global C source is consistent with the atmospheric growth rate. If this finding is meaningful, these 13% of runs need to be analyzed and presented clearly so that the reader can understand what is different about them. A simple explanation for the uncertainty in the budget could be that GPP is overestimated by 10%, and if all of these 13% of runs have GPP on the low side, then it would be useful to identify such a pattern.

We have thought about this suggestion but concluded that this would imply a level of insight that is not supported by the data, i.e, the large uncertainty related to missing fluxes (see page 16 and comment above). Alternatively, constraining the NEP ensemble could thus be misleading because the constraint is highly uncertain. Furthermore, if we assume that relevant drivers are missing in the set of predictors in FLUXCOM (e.g. forest age, see Sect. 4.1), all members in FLUXCOM are biased in the same way (all members use the same set of predictors). Constraining the ensemble, even if we had a well-defined constraint, thus cannot provide us with new insights into the processes that need further investigation or drivers that are missing in the set of predictors. For these reasons we decided to omit the part where we state that 13% of the NCE runs are consistent with the CGR.

3. There are many inconsistencies in the data products used here. For example, for the ocean flux the parameterization of gas exchange is Wanninkhof (1992) with ERA-interim winds, but for the shelf it is Wanninkhof et al. (2013) with CCMP winds. These differences could make a significant difference to the ultimate fluxes even though based on the same pCO2 database. On the one hand, this reviewer recognizes that these differences are due to choices made by the providers of these previously-published flux products, and cannot be easily changed by these authors. Nevertheless, some evaluation of these effects should be performed. One possibility for such evaluation could be in the overlap regions of the three products that go into the merged Marine flux field.

We agree that there are inconsistencies between the data products. As noted by the referee, it is not the aim of the study to re-calculate estimates, but rather bring together existing knowledge. We have tried to account for many inconsistencies, i.e., we calculated flux estimates at the same spatial and temporal resolution, we have unified the uncertainty calculation procedure, we have recalculated overlap areas to avoid double accounting, etc, but as rightfully noted by the referee, there are still some other sources we have not accounted for. The referee highlights the gas exchange formulation as an example and we concur that this is a factor that has a significant impact on the air-sea exchange of CO2. However, as explained above, the net effect of the gas flux formulation is of lesser importance when the net flux is considered. We would also like to note that while the open ocean estimates use the formulation of Wanninkhof et al 1992, i.e., a quadratic dependency between gas flux and wind speed at 10m, they use more recent gas transfer coefficients (Rödenbeck et al and Landschützer et al scale their estimates to match a mean transfer velocity of 16cm/hr as suggested by Wanninkhof et al 2013). However, we concur with the reviewer that there is an additional uncertainty related to the transfer. In this way, uncertainty in ocean estimates is probably underestimated. This is, however, also true for land based estimates. E.g. in FLUXOM, all models use the same set as predictors. We welcome the suggestion of the reviewer to use the overlap area for testing, however, this overlap area is very local and is biased towards coastal zones. We will discuss this probable underestimation of uncertainty in the discussion section. Landschützer et al 2014 estimated that the choice of transfer formulation and the pCO2 mapping mismatch (also including other relationships than quadratic) lead to an uncertainty of 37% for the global average over 1998-2011, with the majority of this uncertainty stemming from the gas transfer formulation.

4. The text is difficult to follow, particularly in the discussion and conclusion sections. These sections read as a list of possibilities, without clarity as to what is really important. The authors need to do more to provide this needed clarity.

We will reformulate these sections to emphasize and discuss our main results better. In particular, we will focus the conclusion on these 3 main results:

- *i) Current spatiotemporally explicit observation-driven estimates of surface-atmosphere CO2 exchange are not constrained well enough to close the carbon budget at the global scale.*
- *ii) Regionally, those estimates are partly well constrained and may be used for model-data integration studies and validation of models. These regions include Europe, Russia, South Asia, East Asia, Australia and many oceanic regions. Constraining C fluxes in regions with currently high uncertainties better should be a priority of future research.*
- *iii) The most likely candidate for inducing the mismatch between data-driven surface-atmosphere CO2 exchange and the atmospheric CO2 growth rate is land NEP, in particular tropical NEP estimates appear to be very uncertain. Understanding this bias will help designing better upscaling approaches (e.g. by including currently missing relevant drivers) and pinpointing variables that need to be (better) monitored in the future.*

Minor comments
Page 2
- Line 15 "limitations."

Thanks.

- Line 24 Which regions have the large net sink? The authors specify several regions with flux to the atmosphere, then the sum of all is large and negative, presumably due to the tropics. The reader should be able to better understand where this large negative is coming from geographically based on the abstract.

The large C is over found over most of the tropical land, i.e., Amazon, Congo and Indonesia. We will mention these regions in the revised abstract.

Page 3
- Line 15: "background CO_2 fluxes over land and ocean," This analysis only accounts for background fluxes in the land, not in the ocean. Instead this analysis suggests there is no net background ocean flux, only the anthropogenic residual! In contrast to Figure 6.1 of IPCC WG1 (Ciais et al. 2013), this analysis ignores background, natural ocean exchanges. This is a major error that must be remedied.

We believe that this is a misunderstanding. The provided flux estimate is neither natural nor anthropogenic but the contemporary flux, i.e., a combination of both natural and anthropogenic. Using surface ocean observations to estimate our flux we cannot distinguish between natural and anthropogenic. To separate the components, we would need to know the pre-industrial state of the ocean. Some budgets, e.g. the global carbon budget solve this e.g. by using an estimate of pre-industrial net ocean outgassing of 0.45PgC/yr (Jacobsen et al 2007) that is derived from riverine carbon input and prohibits CO_2 saturation between the ocean and the atmosphere. However, this number does not account for natural variability of the pre-industrial flux. The study also does not focus on the natural carbon budget, or on the anthropogenic flux budget, but rather tries to quantify contemporary air-sea and air-land fluxes based on available observations. The same holds for the land. To make this clearer we will clarify this aspect in the introduction of the revised manuscript.

Page 5
- Line 2 What is the meaning of "resampled". Is this averaging of all points in a 1x1 grid? If data are at coarser resolution, what is done? Please be more specific. Show that global mean values of the variables considered are conserved by this method.

All datasets have at least 1x1 degree resolution (1x1 degree or finer), such that resampling here means averaging to this coarser resolution. All global means are conserved by this averaging (by taking the land-ocean masks into account). We will explain this in more detail in the methods section of the revised manuscript.

- Line 25 "For NCE estimates, we randomly combined all datasets, using a single realization of each flux, to generate an estimate of NCE." What is the meaning of "randomly combined all datasets"? How is this random if all datasets are used? If the "random combination" applies only to the 2 fluxes (ocean and LUC) that have multiple sources according to Table 1, then the result here is an incomplete estimate of uncertainty. More explanation is needed here so that the reader can have confidence in the uncertainty estimate being made.

The random combination applies always to all fluxes contributing to NCE. That is, we create multiple estimates of NCE by summing up different random combinations of the source datasets following Eq. 1. Uncertainty is then estimated based on the newly generated NCE ensemble. See also the response to the comment below.

- Overall it is hard to understand how the uncertainty is propagated. Bits and pieces are mentioned under each of the flux products below, but a coherent picture is not made clear. Perhaps this lack of clarity could be partially remedied with a schematic figure that clarifies how many different realizations of each flux and how the sampling across them is performed.

*We will introduce a schematic figure to better explain how the uncertainties are propagated, as the reviewer suggested (see Figure R1 below). Each of the 200 NCE ensemble members consists of the sum of randomly selected members of the fluxes contributing to NCE (see Eq. 1). In principle, we could create 10*10*50*16*10*2=160000 different NCE estimates by combining all the available members (see the #Runs shown in Table 1). We limit ourselves to 200 NCE estimates as a representative sample for the whole distribution due to the prohibitive computational expense of running all 160000 combinations. All NCE uncertainty estimates are then derived from these 200 runs. This approach implicitly contains information on the spatiotemporal uncertainty structure of the NCE estimates (i.e., the error covariance matrix). In this way regional or continental NCE uncertainties can be computed by aggregating each of the 200 NCE estimates over the desired region, automatically taking correlated errors into account. This was for example done for Figure 4.*

Page 6
- Line 16 “schused” is a typo

Thanks, should be “used” and will be changed in the revised version.

Page 8
- Line 15 If the same FLUXCOM product is being used to separate the gpp and ter, the ocean fluxes out vs in should also be separated (Figure 6.1, Ciais et al. 2013). It is inconsistent to take different approaches with land vs ocean, and skews the reader impression of the magnitude of local fluxes and their uncertainty.

As explained above, in the revised version we will only use the directly upscaled NEP product over land to be consistent and to focus the attention on the uncertainties that are related to the net fluxes only.

Page 10
- Line 8: A reference for EDGAR is likely warranted.

Thank you, we will insert the reference in the revised version.

- Line 24 "Not all inversions were available till 2010." What is done if this is the case?

The mean and uncertainty for each year is taken over all available inversions for that year. We will specify this in the revised version.

Page 11
- Line 6: That this imbalance is not real, but an artefact of the uncertainty of the data should be made explicitly clear here; not just left for section 4.

We agree with the reviewer that this statement may be misunderstood. We report the mismatch which is obtained when combining all currently available spatiotemporal data-driven surface-atmosphere fluxes. This statement thus highlights that our current knowledge on C fluxes is not sufficient to close the C budget in this way (i.e., leaving out process-based models). We will add a comment on this in the revised version.

- Line 12: “whereas in fact many errors might be correlated as this is clearly the case for GPP and TER.” The same statement will almost certainly be appropriate in the case of uncertainty in ocean fluxes, once they are appropriately accounted for.

As stated above, the uncertainties in the gross fluxes over the ocean are highly correlated, leading to much smaller uncertainties for the net fluxes (see also the caption of the IPCC figure mentioned by the reviewer). By using the ocean net flux estimates for NCE, these correlated uncertainties are automatically accounted for. As stated before, in the revised version we will only use directly upscaled NEP over land. Hence the comparison of the uncertainties will be more consistent.

- Line 22: “Due to the small contribution of the oceans, absolute uncertainties are barely discernible." Comment: This will probably be different once out vs in is considered separately.

As outlined in our response to the reviewer’s major comment above, the net sink uncertainty is in fact much smaller than air-sea and sea-air flux uncertainty, due to the correlation between the uncertainties between the individual components. Using NEP over land (instead of TER-GPP) will decrease this difference slightly.

Page 12
- Line 14 "We use the land cover map of 2005 from the European Space Agency (http://www.esa-landcover-cci.org/) to identify tropical forests (all pixels where broadleaved evergreen trees dominate). " Why use satellite product, when FLUXCOM model is what your estimate is based on. The FLUXCOM land cover product should be used.

Thank you for this comment, we will use the map of plant functional types used in FLUXCOM in the revised version.

Page 13
- Line 3-11: This section overstates the level of agreement with Ciais et al. (in revision). It suggests that Figure 3 illustrates “good agreement” except for a few regions, but when one looks closely, only 5 land regions agree, including Australia that is basically zero, while 4 do not. Overstatement is exemplified by this sentence “Given that Ciais et al. (revision) rely on an independent method, this demonstrates that a good understanding of net C fluxes exists for non-tropical areas, North America excluded.” This section should be written more carefully to acknowledge that lack of agreement is as common as agreement.

We agree with the reviewer and will formulate this more carefully in the revised version, better highlighting the large uncertainties.

Page 14
- Line 6: NEP should be defined again as its not been defined for many pages.

Thanks, will be done in the revised version.

- Line 23-end: This reads as it may be one hypothesis of many. Or is it a leading one? The reader needs the authors to be more clear.

This is the leading one. We will emphasize this more in the revised version. To test this hypothesis rigorously, the complete upscaling procedure needs to be redone with including a forest age map as a *predictor, which should be done in future research.*

Page 15
- Line 8: "often not too far off but given that different top-down studies using different atmospheric models provide conflicting information on the adjustments needed to align modelled concentrations with measured ones, this information cannot be used to provide clear uncertainty ranges." This is not understandable to someone doesn't work with these models or know this jargon.

We will rephrase the section regarding uncertainties in fire emission referring to a recent paper on global fire emissions by van der Werf et al, currently in discussion for Earth System Science Data (van der Werf et al., 2017). Here it is stated that assuming 50% uncertainty overall is a best guess assessment, and better quantifying this uncertainty requires an assessment of the burned area estimates as well as new field data on fuel consumption and emission factors. We cannot, however, propagate this uncertainty to the NCE estimates as this would require spatiotemporal error covariance matrices.

- Line 13 "To better constrain C exchange on a monthly basis, however, the seasonal cycles of those fluxes would be necessary." Comment: Awkward phrasing.

We will rephrase this statement as "Estimates of the seasonal variation in these fluxes are necessary to better constrain seasonal variations in NCE."

- Line 23 "at similar latitudes."

Thanks.

Page 16
- The degree to which the unaccounted fluxes (list) could be large enough to impact the global "budget" should be discussed. Each of these fluxes should be quantified to the best degree possible so as to put in context with the overall budget. The laundry list approach is not helpful to the reader, particularly when so many of the proposed fluxes are left entirely unquantified, and the authors do not discuss the list at all after it is presented. What is the reader to meant to conclude?

Thank you for this comment. We have provided estimates for those fluxes which have been quantified in the past. The remaining fluxes can be assumed to be rather minor, though little is currently known. We will discuss how much of the obtained global mismatch could be remedied by those fluxes in the revised version. Due to this additional uncertainty, we cannot exclude FLUXCOM runs with particularly high carbon uptake.

Page 17
- Line 14: Regions such as the North Atlantic (Schuster et al. 2013, Biogeosciences) should also be noted as having large uncertainty at seasonal timescales and beyond.

The RECCAP initiative has shown that the largest uncertainties are in the southern hemisphere, i.e., in Schuster et al 2013, the South Atlantic has shown much less agreement between methods than the North Atlantic when low frequency signals – such as IAV and trends – are considered, whereas, methods generally agree seasonally where the seasonal cycle is dominated by the temperature variability, i.e., subtropics. In general, the ocean RECCAP studies (Sarma et al, Ishii et al, Schuster et al, Lenton et al and Wanninkhof et al) have shown regionally substantial differences between, however, few of these papers provide estimates based on observations beyond the seasonal cycle (mainly derived *from the Takahashi et al 2009 climatology). Therefore, we do believe that our observation based estimate provides new insight beyond the results from the RECCAP project, yet in turn we agree that our new estimates needs to be put in perspective with previous findings. Therefore, we will add a comparison between the ocean RECCAP results and the results from this study in the text.*

Section 5 overall:
- This section is also poorly organized. It reads as a listing of issues largely already mentioned prior. It needs to be rewritten to focus on the key findings of this work – What are the take-home messages that the reader should be getting?

We will reformulate the conclusions by focusing only on topics that we believe warrant the most attention in future research. These are listed in the response to the major comment #4

Table 2: - should note that negative is from the atmosphere.

Thanks, clarification will be added.

- If the label is –GPP then GPP should be 108.29 not -108.29

We agree, thanks.

- A consistent number of significant figures should be used, unless the authors can justify the greater precision of the numbers with 5 significant figures (GPP) as opposed to those with only 2 or 3. This is important because it is uncertainty in GPP that drives most of the NCE uncertainty. The GPP numbers is clearly not actually known to 5 significant figures.

We use two digits after the comma for all estimates (except CV because of the very small numbers). The uncertainties reported here are not used to calculate NCE uncertainties. Rather, the ensemble of NCE estimates (see response above explaining the uncertainty propagation) is used to estimate this uncertainty. In this way rounding errors do not propagate through the uncertainty estimation.

- All numbers should have the same fontsize, or if the different sizes have a meaning, it should be noted

Thanks, we will correct this. All font sizes should be equal.

- The full decomposition of the "marine" should be noted in this table, so as to be consistent with Figure 1

We list here only those fluxes that are used to estimate NCE (see also Eq. 1). Adding Estuaries and Shelves would be confusing because they don't enter the NCE calculation individually. We added them in Figure 1 for completeness. We will make this explicit in the title of the revised table.

- The natural fluxes of the ocean need to be accounted for in a manner comparable to GPP and TER.

As outlined above, we believe this is a misunderstanding. Our estimate comprises a combination of natural and anthropogenic fluxes, hence we do already account for natural „background" fluxes. Furthermore, as discussed above, we will only use directly upscaled NEP in the revised version to be more consistent between land and ocean.

Figure 1
- The units on the 815 are presumably PgC. This should be noted explicitly on the figure or in the caption.

Thank you, we will add the unit in the revised figure.

- The ocean should have two arrows, one in and one out. The picture from this figure should be consistent with Figure 6.1 of IPCC in that both the ocean and the land have a large background, natural cycle on top of which the anthropogenic is superimposed.

See above, all our estimates account for natural and background fluxes. Over land we will only use NEP in the revised version. Hence we will delete the arrows related to the gross fluxes over land.

Figure 2
- The colorbar in panel a is mislabeled as "%"

Thank you, the label should be gC m-2 yr-1. We will change this in the revised version.

Figure 3
- The x-axis needs a label

Thank you, the label should be latitude, we will change this in the revised version.

Figure 4
- What are the circles? Presumably outliers? Clarify in caption.
- The regions indicated by each acronym should be noted in the caption.

Thank you, yes, the circles are outliers, we will clarify this in the revised version and explain the acronyms.

Additional References:

Jacobson et al. A joint atmosphere-ocean inversion for surface fluxes of carbon dioxide: 1. methods and global-scale fluxes. Global Biogeochemical Cycles 21, 1, 2007.

van der Werf et al.: Global fire emissions estimates during 1997–2015, Earth Syst. Sci. Data Discuss., doi:10.5194/essd-2016-62, in review, 2017.

Figures:

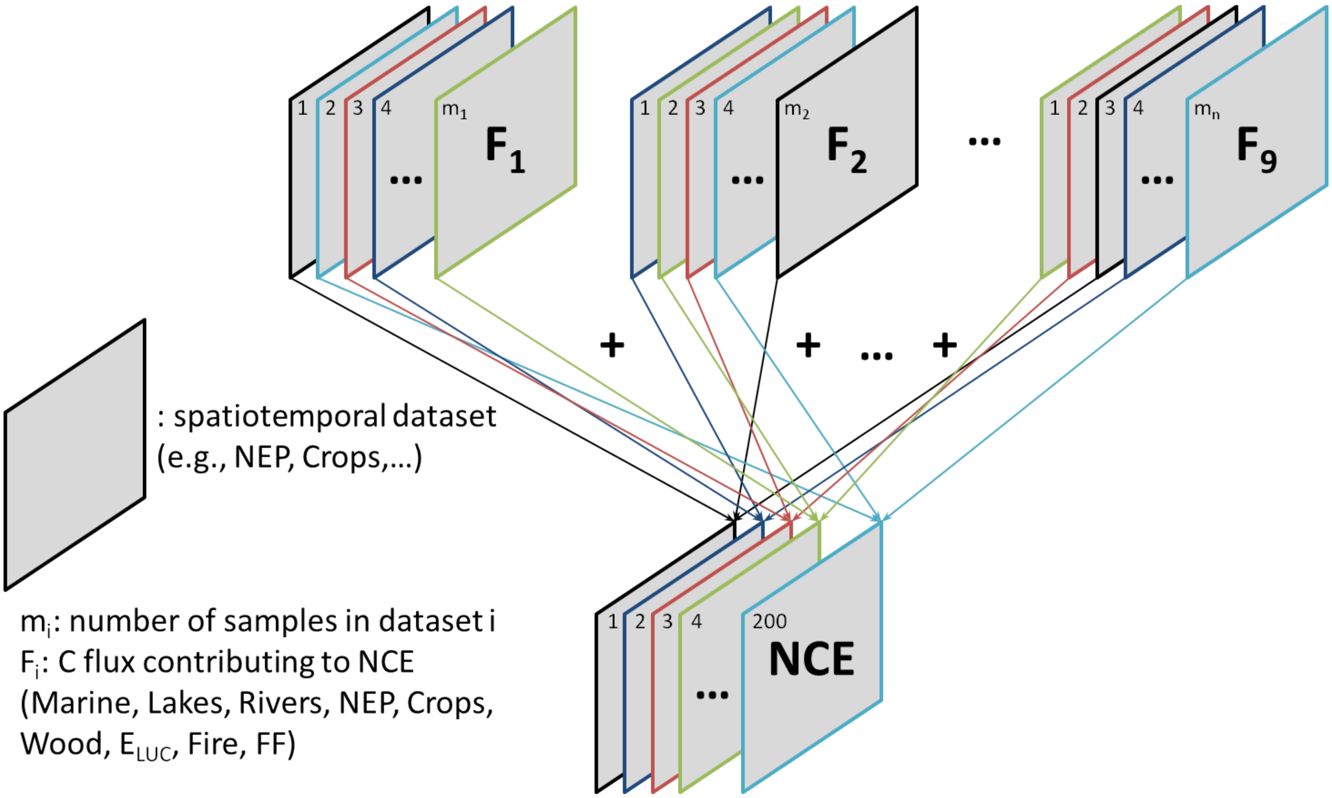

Figure R1 (will be added in the revised manuscript)
Schematic explanation of the uncertainty propagation. Each spatiotemporal estimate of NCE is computed as the sum of randomly selected estimates of the 9 fluxes contributing to NCE (see Eq. 1, here denoted by F_i). For this study we compute 200 estimates of NCE. Uncertainties can now be assessed at different spatial scales by first aggregating all NCE estimates to the desired scale and then using the 200 members for uncertainty estimation.