ADDITIONAL REMARKS

In addition to the changes made to respond to the editor, the referee and the short comments, we made two other changes to the paper.

First, we now refer to a very recent paper by Sterner and Johansson (2017) which is a model-based investigation of the impact of the climate-carbon feedback on emission metrics. Their conclusions are qualitatively the same as ours.

Second, with the aim of proposing the most up-to-date metrics, in addition to the update of the climate IRF, we now also include an update of the radiative efficiencies of CO_2, CH_4 and N_2O (Etminan et al., 2016). Therefore, we have added a new row in table 2 and some new text:

"In table 2 (fifth row), we provide another set of relative metrics, similar to the previous one in that it includes the feedback response calibrated on OSCAR and the updated climate IRF, but it also includes an update of the radiative efficiencies of CO_2, CH_4 and N_2O (Etminan et al., 2016). The new radiative efficiency of CO_2 differs by +2%, that of CH4 by +14%, and that of N_2O by -3%. These changes logically impact the GWPs and the GTPs, since both metrics are function of the φ_x parameters. The change is substantial for CH_4: in most cases more so than the update of the climate IRF. Notably, the update of the radiative efficiency of CO_2 – being the reference gas in relative metrics – implies a change in the metrics' values of all species, even those whose own radiative efficiency are not changed. These results show that the first-order processes (here, the radiative forcing) may have more impact on the metrics than second-order processes such as the climate-carbon feedback."

EDITOR COMMENT

I think that the paper is of great interest and the exchange with the reviewers has also been positive and constructive.

I would like to make a remark regarding some of the comments appeared in the discussion with the reviewers. I would like to emphasise that, even if the climate system has indeed a chaotic dynamics and is far from being anything close to a linear system, it is possible to develop a (rigorous) linear response theory able to predict its response to forcings with a high degree of accuracy. This justifies the possibility of using the kind of approach and formulas you propose in the paper. You can see the following references:

Ragone F., V. Lucarini, F. Lunkeit, A new framework for climate sensitivity and prediction: a modelling perspective, Clim. Dyn. 46, 1459–1471 (2016)

Lucarini V., F. Ragone, F. Lunkeit, Predicting Climate Change Using Response Theory: Global Averages and Spatial Patterns, J Stat Phys 166, 1036-1064 (2017)

We have added a sentence in the first paragraph of the section introducing the IRFs, to refer to those papers, and therefore justify further the linear approach: "Despite these apparent caveats, the use of such a linear-response approach to emulate the behavior of complex systems can be warranted by the theory, especially in the case of the climate system (see e.g. Ragone et al., 2016; Lucarini et al., 2017)."

SHORT COMMENT #1

This paper is a well-written, carefully constructed, and valuable contribution to the metrics literature. This paper improves upon the existing approach of incorporating climate-carbon feedbacks into GWP calculations and will constitute a very useful resource for subsequent assessments that will update these climate metrics. However, we strongly recommend the authors reconsider their recommendation to use the version of the GWP calculated with the climate-carbon feedback as the primary metric.

Thank you for your support.

As noted in the manuscript, AR4 inconsistently calculated GWP's by including climate-carbon feedbacks for CO2 perturbations but not for non-CO2 perturbations. This inconsistency was noted in AR5 which presented climate metrics both with and without climate-carbon feedbacks for the non-CO2 perturbations (based on Arora et al., 2013 and Collins et al., 2013), and we agree with the authors that this inconsistency should be resolved. The use of a climate-carbon feedback that more realistically incorporates an eventual relaxation back to a prior equilibrium for a pulse of climate change, as is presented in this paper, is an improvement to the calculation of a GWP that includes climate-carbon feedbacks. However, one option that was overlooked in AR5, and is presented as a secondary option in this paper, is to remove the climate-carbon feedbacks from both the CO2 and non-CO2 perturbations. We argue that for reasons of simplicity and transparency, that removing the climate-carbon feedback entirely is preferable for calculating GWPs for the use of policymakers.

GWPs have found favor among the metrics community for two primary reasons: ease of computation and simplicity/transparency. Including climate-carbon feedbacks may partially negate both of these benefits of the metric. Without climate-carbon feedbacks, one can calculate the absolute GWP for any given gas (with a known lifetime and radiative forcing) in a simple two-step process. Relative GWP then requires only the use of a previously-calculated 4-exponential function for CO2. However, the calculation of an absolute GWP with climate-carbon feedbacks is apparently a 10 step process (see Figure 4). Including the climate-carbon feedbacks is also shown in this paper to require additional assumptions beyond gas lifetime and radiative efficiency (the only two parameters necessary for the calculation of a traditional GWP). Requiring this choice reduces both the simplicity and transparency inherent in the GWP. Inclusion of climate-carbon feedbacks makes the value of the GWP dependent on attributes of the model chosen – its climate sensitivity, its rate of ocean uptake of heat, and how the carbon cycle changes in response to warming. This kind of additional complexity has been noted as a drawback of the GTP in comparison to the GWP. Incidentally, as the last equation on page 6 shows, the GTP is now effectively a necessary step in calculating the GWP using the methodology in this paper. This approach also requires additional, somewhat arbitrary choices: for example, the authors chose a climate change step of +0.2◦C to be approximately consistent with the peak warming of a 100 GtC pulse of CO2 and the approach of Joos et al. (2013), though Figure 3 shows that for step sizes smaller than 1 degree, this choice does not appear to have a large impact.

Rows 5 and 6 of Table 2 demonstrate that there is little added value in terms of accuracy for the GWP when including the climate-carbon feedback (the 100 year GWP is particularly important as the metric in the most widespread use). The difference between the GWP calculated with and without climate-carbon feedbacks is less than 2% for any the 5 substances considered for any timeframe. This small improvement in accuracy of the 100 year GWP comes at the cost of complexity and lack of transparency as previously discussed. Moreover, despite the good work done by the authors in this paper, it is unclear to what extent use of a different model, parameters, or other choices could lead to changes in this small adjustment to the GWP.

The authors do note that "our results raise the question of whether the climate-carbon feedback should be included in emission metrics", and yet, they "recommend using the metrics shown in this fourth row of Table 2, since they are the most consistent, robust and up-to-date metrics available" (and even raise questions about what other feedbacks should be included, such as climate-wetlands feedbacks). We would strongly recommend that the authors reconsider this recommendation.

The authors should continue to present the most up-to-date metrics based on their carbon-cycle models, but we believe that the authors should in fact endorse the use of the GWP without the climate-carbon feedback (in either the numerator or denominator). The authors show that including the climate-carbon feedback offers a slight improvement in accuracy, but in our opinion, that improvement in accuracy is far outweighed by the double drawbacks of increased difficulty of computation and loss of simplicity and transparency.

Recommending the use of metrics without the feedback would mean highlighting the 3rd equation in Appendix C.1, as it would then be possible to calculate the GWP for any arbitrary non-CO2 gas given lifetime and radiative efficiency values. The authors could encourage other carbon-cycle modelers to similarly calculate carbon dioxide response functions without the inclusion of carbon-climate feedbacks such that a future IPCC assessment could draw from multiple studies to inform its GWP calculations.

We are, however, less opposed to the inclusion of climate-carbon feedbacks in the calculation of GTPs. Including the feedback in GTPs results in larger impacts than when considering the GWPs (almost 30% as the authors note, for the case of 20 year GTPs and either BC or SO2). Additionally, the additional computational cost, increase in complexity, and loss of transparency are much less powerful arguments when applied to the GTP in contrast to the GWP, since many of those drawbacks are inherent in GTP calculations in the first place.

Again, we commend the authors on an extremely interesting, robust, elegant, and useful analysis, but ask the authors to take our comments into account.

This comment makes a very strong case against including the feedback in GWPs. And we do agree with most of the points made. This is actually a debate we had, amongst authors, and in the first submitted version of the paper we settled on recommending including the feedback because we also had the absolute metrics in mind. Sometimes, AGWPs and AGTPs are used as very simple models of climate change. In this case, including the feedback improves the accuracy of the representation of the system.

However, we have decided that in the revised version there will be no recommendation at all. Partly because we cannot clearly recommend one of the two options, but mostly because we now think that making any recommendation would make this paper too controversial, and would weaken its more scientific/methodological aspects.

We initially wrote the paper as a research paper. And though we understand that a part of the community is eagerly waiting for a recommendation and consistently updated metrics, we believe our paper is not the place where this should happen. A commentary or perspective paper could follow, of course, but we are not convinced it is our role to lead such a paper (although we can surely contribute). As the sought recommendations ultimately concern more the user community, maybe this community should be the one making those recommendations.

So in concrete terms, in the text, we have removed all previous recommendations, and rewritten the first paragraph of section 5.2:

"In the case of absolute metrics – both AGWPs and AGTPs – these changes are substantial since we are adding a positive feedback to the model. Therefore, the choice of including or excluding the feedback may ultimately depend on the application of the metrics. On the one hand, for the sake of simplicity and transparency, the feedback could be excluded from the evaluation of GWPs, since it avoids the trouble of the five convolutions shown in figure 4. On the other hand, if absolute (e.g. time-varying) metrics are used as a first-order model of climate change, one may prefer including the climate-carbon feedback to have a better representation of the system."

and a part of the conclusion:

"Ultimately, whether emission metrics should include the climate-carbon feedback is a decision for the user, and we only recommend consistency in the way feedbacks are included or excluded. The trade-off between simplicity and transparency on the one hand, and accuracy of representation on the other hand, has to be weighed by the final user."

Additional technical comments:

We note as a relevant comparison, that Reisinger et al. (2011) calculated the effect on GWPs of using different RCPs to project future concentrations, and found that GWPs could change by 10 to 30% for N2O, -10 to 20% for CH4, and 2 to 36% for the halocarbons. However, the community has to date retained the assumption of constant background concentration, presumably in order to preserve simplicity and to avoid the necessity of choosing a single future emissions scenario (or combination of scenarios). The effect of this simplifying assumption is an order of magnitude larger than that resulting from the inclusion of climate-carbon feedbacks.

Yes, just as we point out that the background strongly affects the calibrated IRF (and subsequent metrics). We've added a sentence to note the larger effect of the background: "[Variation in the metrics' value from including/excluding the feedback] are also less in magnitude than those induced by the choice of the protocol used to calculate the metrics, such as the background conditions (e.g. Reisinger et al., 2011), or by the choice of a given time horizon (see e.g. table 2)."

Page 1, line 14: "the IPCC presented tentative values": The text of AR5 was not clear that the climate-carbon feedback values were to be considered "tentative", nor does this match the way that the AR5 values have been perceived and used by the community.

Removed. (But see next point.)

Page 2, line 16: "The standard metrics provided in the fifth assessment report": Similarly, the text of AR5 was not clear that the version of the metric that did not include climate-carbon feedbacks for non-CO2 gases (and was therefore "inconsistent"), should have been considered "the standard metric".

We disagree with this comment: the huge table at the end of chapter 8 of IPCC is the main product of this section of the report.

However, we agree that the fact that it was published as an appendix (because of its size) made it difficult for the metric-user community to perceive which of the two tables (w/ & w/o feedback) was the main product…

Page 11, line 16: The phrasing of the following sentence could be improved: "which is itself the value chosen by Joos et al. (2013) – and therefore by the IPCC": while the IPCC implicitly endorsed the approach of Joos et al., there was no explicit determination that 100 GtC or +0.2 degrees C is the optimal value to use. The IPCC can be limited by the literature available, and the choice of a given paper to support a parameter choice does not necessarily indicate endorsement of all the choices made within that paper. A preferable phrasing would be, "which is itself the value chosen by Joos et al (2013), which provided the carbon lifetime used by the IPCC" or something along those lines.

Agreed. The end of the sentence is now: "chosen by Joos et al. (2013) and used in the IPCC AR5."

Page 11, line 18: the text refers to "Figure 5" as an illustration of the GTP calculation process, but should be corrected to refer to Figure 4.

Yes, we have corrected the numbering.

Page 14, line 6: the authors do note that the inclusion of the feedback has less than a 10% impact on GWPs and GTPs, but the fact that the impact is less than 2% for other GWPs, even for short-lived species, for any time horizon, is an important distinction that is not sufficiently emphasized in the text. GTPs and GWPs are clearly impacted very differently here.

We have reformulated the discussion to more clearly separate the effect on GWPs and GTPs:

"We have found that including or excluding the climate-carbon feedback in a consistent manner does not greatly change the values of the relative GWPs (only about 2%). In the case of relative GTPs, the change is slightly larger for greenhouse gases (less than 10%) and becomes even larger for very short-lived species and over short time-horizons (greater than 30%)."

SHORT COMMENT #2

Gasser et al. report on findings that the effect of including climate-carbon feedbacks for both the target species and CO2 produce GTP and GWP values that are much closer to their default values than was suggested in IPCC AR5 report. They also call for discussion in the community about the limits of the prevailing linear, impulse response function framework for describing complex feedbacks in the climate system.

I wanted to add some minor comments on the wording and equations:

Thank you for the comments.

Page 3 / L8: 'dynamic' – IRF describes a dynamic system, but not sure it is correct to say that it is dynamic; i.e. the impulse response functions are invariant with regards to initial time

An IRF is the analytical solution of a set of ordinary differential equations with constant coefficients (i.e. linear). Therefore, it describes the exact same (physical) system as the differential equations do. If the system is deemed dynamic, we see no reason why the IRF should not be said to be dynamic as well.

Page 3: "the change in atmospheric concentration of the species (Qx)" - should be Qx(t) - Qx(0), or else equation on next line could simply be Qx(t) = ...
Similar for the line about Tx

Note that the sentence can be read slightly differently: "the change in [atmospheric concentration of the species (Qx)]"; i.e. "Qx" is attached only to "atmospheric concentration of the species" and not to the whole beginning of the sentence.

Moreover, if we assume the sentence is read as the commenter suggests, the equation has to be written with Qx(0) on the right-hand side. But this is completely equivalent to the way we wrote it.

So we don't think any change to the sentence should be made, firstly for the sake of legibility.

Page 4/L7: should be approximate (≈) symbol rather than definition (≡)

This has been changed to: $RF^x(t) = \varphi^x\big(Q^x(t) - Q^x(0)\big)$

Page 4: 'mass' would be more clear than 'size' in describing the emissions

Though 'mass' would be physically correct, we prefer to use the more colloquial and yet widely used word 'size', as Joos et al. (2013) and IPCC did.

The word 'normalized' and 'relative' are used inconsistently (it is not always used to say that GWP is relative while AGWP is absolute and that both are normalized (to a 1 kg pulse) metrics). This is apparent on page 4 / L9-11, but also applies page 2 / L18 and elsewhere

Very true! We checked and corrected all occurrences.

There is also a subtle change in notation that is not mentioned that some of the equations on page 5 and 6 give terms that were previously explicit functions of time but are now shown with implicit dependence through the variables Tx, Qx, E0x, RFx and Θ

The change in notation is explicitly introduced with the following (existing) sentence: "To simplify the discussion and avoid quintuple integrals, we introduce the simplified notation $\star$ for the convolution: $a \star b \equiv \int_0^t a(t')\, b(t-t')\, dt'$, and note the commutative property of the convolution: $a \star b = b \star a$."

The disappearance of the time variable comes with the new notation, and happens for the same reason: to keep the notation simple and legible.

Page 5 / L26: a(t) should be a(t')

Yes. Corrected.

Page 6, L18: This seems to be the relation between pulse and continuous emission given in Aamaas et al. 2013, ESD 4: 145-170, but I could not follow the logic here.
Also unclear that the 'definition' (≡) symbol is applicable

We have added a short sentence to make this part clearer: "[Note] that convoluting any function with the Heaviside function is equivalent to integrating it."

Although, we have to say there is no 'logic' here apart from acknowledging that the integral of a function *f* is – by definition – strictly equal to the convolution of *f* by the Heaviside function.

Page 9 / L19: the use of the word 'extended' causes a little confusion since the meaning is not described until L27-L29

Yes. We have moved that sentence to appendix B, where the details of the fit are given.

Constant intensity term (λ) "climate sensitivity": overall there was not much discussion of this parameter, but believe should at least point out that it refers to an equilibrium climate sensitivity.

The parameter is defined in section 2.1. We have added the word "equilibrium" to the text to make it clearer.

REFEREE COMMENT #1

This manuscript presents a methodology to better assess the greenhouse-gases emission metrics, by considering and removing the "climate-carbon" feedback that is implicitly used in previous estimations and in previous IPCC recommendations. The methodology is well exposed and rather straightforward, the scientific discussion is clear and well written. Therefore, I have no comment on the technical content of this paper.

We thank the referee for acknowledging the technical quality of the paper.

In contrast, I have some major comments on the overall presentation, introduction and conclusion: these critical comments must be accounted for by the authors before considering publication. Indeed, greenhouse-gases emission metrics is a very "subjective" tool that should be presented as such. It is possible to build a very accurate subjective methodology, but this certainly does not help to provide an objective one.

Nowhere in this paper do we pretend to create an "objective" metric. The subjective aspects of emission metrics have been largely discussed in the literature (e.g. IPCC AR5 WG1 Chapter 8), and the topic falls out of the scope of our paper. Here, we discuss scientific and technical issues regarding the inclusion of the climate-carbon feedback in metrics.

I therefore strongly disagree with the general tone of the paper, given in the introduction: page 1, line 20: "However, including carbon-climate feedbacks, particularly in absolute metrics or for short time horizons, gives a more realistic representation of the response"

This sentence has been changed: "Including or excluding the climate-carbon feedback ultimately depends on the user's goal, but consistency should be ensured in either case."

It now reflects the fact that we do not recommend a particular approach, between including and removing the feedback. We do recommend, however, a consistent approach, and therefore to update the IPCC metric estimates.

I also strongly disagree with the conclusion that: page 15, line 15: "To avoid potential biases in metric values, we suggest to include the climate-carbon feedback in metric estimates".

This has been changed as well. The concluding paragraph now is:

"Ultimately, whether emission metrics should include the climate-carbon feedback is a decision for the user, and we only recommend consistency in the way feedbacks are included or excluded. The trade-off between simplicity and transparency on the one hand, and accuracy of representation on the other hand, has to be weighed by the final user. But metric users ~~must~~ should also keep in mind that IRFs and emission metrics are extremely simple models of a complex system, and that sometimes it may be beneficial to use more complex models that better capture multiple and interacting feedback processes."

The very concept of a unique simple metric for GHG is both UNREALISTIC and BIASED. Refining this concept will not change this fundamental fact. The purpose of GWPs or GTPs is to provide a unique simple metric to compare the "climatic impact" of the many different anthropogenic greenhouse gases (GHG). Obviously, from a scientific perspective, this amounts to comparing oranges and apples. I understand that such an exercise is necessary from a policy perspective, and that scientists should help and provide numbers. Still, I am not convinced that comparing "very accurately" oranges and apples is either necessary or desirable. At the very least, when comparing them, scientists should keep insisting on the differences.

Our paper is embedded in the existing literature on emissions metrics. The basic premise of the paper was to reassess the way that feedbacks are included in metrics, an issue noted as requiring research (IPCC AR5 WG1 Chapter 8). As the reviewer notes, metrics may be "necessary from a policy perspective, and that scientists should help and provide numbers". We see this paper as fulfilling a request from policy makers, to show and correct a mistake made by the IPCC. We additionally show several metrics (GWP, GTP) and different time horizon, and show the importance of feedbacks. We do not recommend one metric over another; that is not our role.

The most important (and arbitrary) parameter is the chosen time horizon: do we value more the current generation (20 years from now) or future generations (500 years from now)? This is a moral question, not a scientific one. Therefore, in the 2001 IPCC report, we read, for instance concerning methane (CH4), a range of values: GWP20 = 62 ; GWP100 = 23 ; GWP500 = 7 (IPCC 2001, page 388, Table 6.7) Interestingly, the range given in the 2014 IPCC report (AR5) is "narrower": GWP20 = 84 ; GWP100 = 28 (IPCC 2014, page 731, Table 8.A.1) which does not reflect scientific advances or a more accurate assessment of the metric, but simply a different a priori choice, with the 500-year horizon not being discussed anymore in the last AR5 report. Similarly, using the GTP metric (the effect at final time t) instead of GWP (the effect integrated between gas injection and time t) is a rather arbitrary choice. The use of the global mean temperature (in GTPs), or global mean radiative forcing (in GWPs), is also quite arbitrary, since local impacts do not necessary scale linearly to such global averages. Of course, all these points have been discussed in the literature many times and are well known to specialists. Still, I believe they are so critical and so often overlooked by non-specialists (policymakers, BUT also many climate scientists), than they need to be heavily stressed in papers on GHG metrics like the current manuscript. In particular, the reassessment of GWPs (or GTPs) performed in this manuscript, in order to "remove the carbon-cycle feedback in the denominator", does change the numerical values by, typically, a few percent or less, something very much smaller than, for instance, the arbitrary choice of a time horizon. This needs to be explicitly stated and probably strongly emphasized in the manuscript: comparing GHGs is much more a moral and subjective choice (eg. long-term versus short-term) than a scientific question. Providing accurate estimations of a subjective metric does not lead to an objective metric.

We have added a sentence in the discussion/conclusion to recall that the time horizon remains an important choice when calculating emission metrics: "[Variation in the metrics' value from including/excluding the feedback] are also less in magnitude than those induced by the choice of the protocol used to calculate the metrics, such as the background conditions (e.g. Reisinger et al., 2011), or by the choice of a given time horizon (see e.g. table 2)".

The fact that there is a first order uncertainty does not prevent studying a second order one, especially since the first order uncertainty is of political nature whereas the second order one is of scientific nature.

The very concept of GWPs/GTPs is based on a simple linear view of the climate system (impulse response functions, transfer functions, Laplace transforms, …). In order to be physically relevant, it requires the quite strong assumption that there is NO feedback at all in the system (ie. GWPs are fully independent on climate or other GHG levels). Of course, GWPs/GTPs can be diagnosed from complex non-linear systems, but their use as a simple metric is based on the assumption that the climate responds linearly to each individual forcing.

The original purpose of emissions metrics was to compare GHG emissions at the margin (e.g., IPCC FAR). The general concept is to compare one additional kg of different GHGs. In practice, because of the signal-to-noise ratio, large pulses are used to estimate IRFs (e.g. Joos et al., 2013). Though, test are performed to ensure the pulse is not so large as to introduce non-linear responses (e.g. Joos et al., 2013, but also our figure 3).

IRFs and metrics do include some types of feedbacks, with the strong limitation that they are implicitly and linearly accounted for. For instance, the climate IRF implicitly includes the water vapor, lapse rate, cloud-cover and sea-ice feedbacks.

Our paper also demonstrate that it is possible to account for more feedbacks by developing further the IRF framework. And as the work of Joos et al. (2013) shows, despite the feedbacks, it is still possible to look at a linear (marginal) response.

The aim of the paper is therefore to remove the feedbacks in the carbon cycle to better "fit" into the concept of linear systems and therefore provide a more "accurate" quantification of GWPs/GTPs.

This is not the goal of the paper. The first goal is to correct the IPCC mistake by making the GWPs and GTPs consistent in the way they include the climate-carbon feedback. The second goal is to discuss how the metrics are affected by including or excluding the feedback.

But at the same time, climatologists usually insist in describing climate as a complex non-linear system, with many feedbacks (in particular between climate and the carbon cycle, precisely the one discussed in the paper). This is a point that deserves some extended discussion: To what extent GWPs/GTPs are sound concepts for climate? And to what extent are they simply imperfect tools designed to answer the heavy policy requirement for a metric?

The discussion suggested by the referee is way out of the scope of the paper. Our paper is not a review nor a perspective on the topic. It is based on the existing literature and solves one previously identified issue of the emission metrics.

Further, IRFs and metrics are designed to be used at the margin where linearity holds, and they are used here to compare pulses of GHGs (not emission scenarios where linearity becomes a problem).

I have also a more specific problem with the IRF for temperature. The impulse response functions for carbon (Appendix C.1) have all the same structure: a constant term (= percent carbon staying in the atmosphere "forever") and several decreasing exponentials (= capture of carbon by vegetation and ocean). In contrast, the impulse response functions used for temperature (Appendix C.2) have no constant term. In other words, a basic fundamental ASSUMPTION in the GTP computations is that climate change is fully reversible: whatever the size of the initial radiative "spike" forcing at time zero, climate recovers to its initial conditions after a few centuries. I have some major difficulties to admit such a strong HYPOTHESIS, which stands against all my knowledge in climate science... These response functions are obtained from atmosphere-ocean only GCMs simulations (without feedbacks from the surface vegetation changes, land ice cover, deep ocean changes, etc…) by fitting one-way experiments (abrupt or gradual 4xCO_2 experiments with stabilization). Is this supposed to be realistic? Interestingly, there are no reversed experiments, even though the IRF functional form assumes reversibility: is this climate reversibility assumption based on something else than just simple convenience?

Again, I understand the requirement for a metric to compare GHG. Obviously, this implies some arbitrary choices and some drastic simplifications of the climate system. Still, I have difficulties with the logic of "fitting" the climate system into a simple linear fully reversible system. I certainly do not share the scientific concept behind. At the very least, these fundamental assumptions should be explicitly stated and discussed in the manuscript.

It is true that full reversibility is implicitly assumed, so we have added the following in the introductory paragraph of the section dedicated to IRFs, to remind the reader of the implicit reversibility of the model: "[IRFs] represent a fully reversible system […]". With IRFs, however, this reversibility is not instantaneous, and such a model is fully capable of showing the kind of hysteresis one can observe in complex models.

Here, in the specific case of emission metrics, the idea that those are calculated as the contribution of a marginal emission of the considered gas is also important. The marginal emission of the gas is assumed to occur under a given background, but it is not assumed to affect this background. Therefore, under the metric framework there is no issue of irreversibility. The issue only arises if one wants to use the IRFs as first-order models of the climate system, which is not the case in this paper.

Note also that the constant term in the carbon-cycle response is not a proper irreversibility. It is an apparent irreversibility within the time-frame of the experiment used to calibrate the IRF (1000 years). IRFs over a longer time-frame have been proposed, in which case the constant term becomes also a decaying exponential with a time-scale much longer than 1000 years (~80.000 years if we stick to only one exponential to describe this long-term response).

There is a real danger to misrepresent the response of the climatic system, in a "very accurate" BUT certainly not "objective" fashion, as a linear response to the superposition of independent GHG forcings that are not allowed to interact with each other, nor with climate. I am not sure that scientists should blindly misrepresent the real world, only to fit policy requirements of a simple metric. At least, they should be extremely cautious and stress the limitations of the GWPs/GTPs concept.

"Remember that all models are wrong; the practical question is how wrong do they have to be to not be useful." (Box G .E. P. & Draper N. R., 1987, *Empirical Model Building and Response Surfaces*).

Though the quote might be overused, it remains – we believe – a fundamental truth about modeling. We argue that the (political) demand for metrics such as GWPs and GTPs makes those useful *de facto*. We acknowledge the many limitations of those simplistic models. But these have been discussed extensively in the literature, and it is not within the scope of this paper to revisit the question.

I am not a specialist of GHG metrics. I am writing this review just after the interactive comment from M. Sarofim et al. was posted online, and I strongly agree with it. The added value of a more "accurate" assessment of GWPs/GTPs, as presented in this manuscript, comes at the cost of simplicity and reproducibility. Though the scientific methodology presented in this paper is sound and well presented, I am not sure this is the best way to fulfill the requirements of GHG metrics. Fundamentally, GHG metrics are only a "rule of thumb" to decide which GHG is "better" or "worse", from some subjective perspective. Scientists should not try to disguise this "rule of thumb" into an objective, quantified, assessment.

As stated by the reviewer, emission metrics are used to compare GHGs, to decide if one is better or worse than the other from some subjective perspective. Yes, that sums up an emission metric. But, policy makers have a need for such a tool (such as in emission trading). All the authors on this paper are fully aware of the limitations of emission metrics, and have written on the subjective aspects (some extensively). We are in no way trying to "disguise" this subjectivity. This paper is well embedded in the existing literature. The paper is of a technical nature and discusses a technical issue, and therefore readers would go elsewhere for a more detailed discussion of the subjective aspects of metrics (many of which we mention and cite). It is not in the scope of the paper, nor the interests of the readers, to discuss a topic that has been well discussed, reviewed, and assessed by others.

REFEREE COMMENT #2

The authors present a new and elegant approach to including climate-carbon cycle feedbacks consistently in the estimates of emission metrics, and more specifically, absolute metrics for non-CO2 components. The paper makes several important points associated with the treatment of climate-carbon cycle feedbacks in the calculations of emission metrics performed for IPCC AR5. The text requires some editing (although I like the style of writing), but the argument is clear and the results are well presented. I think this paper potentially has a strong impact in the field of emission metrics and may influence the next IPCC report but can also lead to confusion among metric users as I discuss below. The paper requires a revision by reflecting the comments below before being recommended for publication in Earth System Dynamics.

We thank the referee for his review and support.

I start with one broad comment, followed by several minor ones. The paper begins with the issue that the treatment of climate-carbon cycle feedbacks was inconsistent in representative metric values in IPCC AR5 (i.e. Table 8.A). More precisely, such feedbacks are accounted for in the estimates of absolute metrics for CO2 but ignored in those for non-CO2 components, resulting in an inconsistency when they are put together to calculate relative metrics. This inconsistency is, to be sure, clearly indicated in multiple places in IPCC AR5, but my observation is that the inconsistency has created confusion among metric users. Some studies that follow (e.g. (Cherubini et al. 2016; Levasseur et al. 2016)) support a use of alternative metric values taking climate-carbon cycle feedbacks consistently into account (i.e. Table 8.SM.15 in the Supplementary Material of IPCC AR5), even though alternative values are available only for a subset of the components of interest. Now, the paper reveals that the approach to incorporating climate-carbon cycle feedbacks for non-CO2 components adopted in IPCC AR5 was actually wrong because the natural carbon sinks are assumed inactive for the additional CO2 release through climate-carbon cycle feedbacks (e.g. Figure 2). This finding essentially disqualifies all the alternative metric values in IPCC AR5.

Given the situation above in the recent past, this paper may create a new confusion among metric users dealing with climate and environmental policies and assessments. I would therefore request a more detailed clarification of what has happened and what should be done for the metric values in IPCC AR5 in their view. I think that this paper is a right place to do so because some of the authors have been closely involved in the writing of the metric section of IPCC AR5.

Hopefully this comment can be taken in a constructive way, but the paper can be more explicit about why the treatment of climate-carbon cycle feedbacks ended up with being inconsistent in IPCC AR5. The paper describes how it is inconsistent in sufficient details (e.g. Page 5, Lines 3-9), but it is unclear to me why this has happened. For instance, why was it not possible to estimate an IRF for CO2 response without climate-carbon cycle feedbacks? If this were available, this might have allowed one to estimate metrics ‘consistently’ without climate-carbon cycle feedbacks. This might have been an alternative solution, if not a best one, in light of the inherent linear limitation in the IRF approach that is discussed in Section 5.2. In practice, it is probably not feasible to re-do an experiment requiring many models. But, looking back, was there a lack of coordination at the beginning? Furthermore, what about the method to account for climate-carbon cycle feedbacks for non-CO2 components in AR5? This method has not been sufficiently tested before the adoption and is based just on a section of one peer-reviewed paper (Collins et al. 2013), whose main contributions lie elsewhere. How was the ad-hoc decision process leading to the adoption of this approach made? Is there anything useful that can be learned from for the next IPCC report? What are the recommendations for metric values? I noticed that the paper does contain some text recommending the new estimates (page 12, lines 15-17), but it is buried in the middle of the paper and I am not sure what are the intentions. I raised some of the questions that may arise if the paper is officially published, although not all of them may not have to be answered in this paper. Clarifications suggested here should be helpful for metric applications, and ultimately the IPCC AR6.

The reviewer raises a fascinating and important point, but we fear, well beyond the scope of this paper. Essentially, the reviewer is passing comment on IPCC processes. The IPCC assesses the literature, and by doing so, place appropriate confidence on different findings. This comment is far broader than just the feedback value from Collins et al 2013. The GWPs have been update in all five ARs, sometimes due to shifting background concentrations and sometimes due to improved scientific understanding. Thus, users of metrics should have an expectation that GWPs (and GTPs) will change in the future, due to both shifting background concentrations and improved scientific understanding. The inconsistency with the feedbacks has occurred in all ARs, and neither the literature (including reviewers) nor the IPCC has elevated this issue sufficiently for new analysis to be performed (until Collins et al 2013). Yes, one could question the IPCC and scientific community processes, a worthy endeavor but well beyond the scope of this paper. But also, the IPCC AR5 WG1 Chapter 8 has clearly noted that values change and will continue to change.

As to making recommendations, following the same logic that our paper should remain a research paper, we have decided not to make any. We now believe that our initial recommendation, although motivated by being the most up-to-date and comprehensive in terms of modeling, may not be the best – as shown by the comment by M. Sarofim and colleagues. Now we limit our paper to a short discussion as to why one would want to exclude or include the feedback, considering this is not our choice to make. We understand this won't please the metric-user community, but it appears that the choice is ultimately more a political choice than a scientific one (just as for the time horizon).

Relevant text added: "Therefore, the choice of including or excluding the feedback ultimately depends on the user's needs. On the one hand, for the sake of simplicity and transparency, the feedback could be excluded from the evaluation of GWPs, since it avoids the trouble of the five convolutions shown in figure 4. On the other hand, if absolute (e.g. time-varying) metrics are used as a first-order model of climate change, one may prefer including the climate-carbon feedback to have a better representation of the system."

Minor comments

Page 1, Lines 11-14 The argument concerns only a set of works using IRF to estimate emission metrics. There are also a body of relevant works based on other approaches like simple climate models (e.g. (Tanaka et al. 2009)) and more complicated ones (e.g. (Gillett and Matthews 2010)). These particular studies do consider climate-carbon cycle feedbacks to calculate emission metrics. The statement can be revised to be more restrictive.

We have added the following sentence to the first paragraph of the 'mathematical framework' section (in which IRFs are presented as a means to calculate metrics): "Note that emission metrics can also be estimated thanks to complex model simulations (e.g. Tanaka et al., 2009; Sterner and Johansson, 2017), with the strong caveat that the approach lacks the simplicity and transparency of the IRFs."

We also note that part of the discussion (section 5.2) is dedicated to the interest of model-based metric estimates (that can include feedbacks in a much easier way than IRFs).

Page 2, Line 2 Another area that I could think of is the ecosystem community (e.g. (Neubauer and Megonigal 2015)).

Yes. Added.

Page 2, Line 13 If there is any reference to support this statement for the last century, please add.

It was unclear that the references to support this statement were the same as for the next sentence. So we have slightly altered the two sentences to put the references at the right place.

Page 2, Line 21 “whose” instead of “which”?

Changed.

Page 3, Line 9 I don’t think the underlying models exhibit a hysteresis within the range of IRF calibrations.

Yes they do! The inertia of the simplest IRF (one decaying exponential) is enough to exhibit hysteresis. If a symmetric forcing is applied to any of the IRFs used in this paper, the resulting response will show hysteresis if looked at in the (forcing, response) plane.

Page 4, Line 6 In practice, this pulse emission is large. As in Appendix A, it is 100 GtC in the case of CO_2.

Agreed. A sentence has been added: “Note that the assumption of a very small pulse may be inconsistent with the way the IRFs are actually derived, as it is currently the case for CO_2 (see appendix A).”

Page 5, Line 26 Should it be a(t’) instead of a(t) in the integral?

Yes. Corrected.

Page 7, Line 16 It would be helpful if the authors provide a few sentences on how climate-carbon cycle feedbacks are modeled in OSCAR, rather than just a reference. Do the feedbacks act only on soil carbon? What about NPP? Do they directly affect the ocean carbon uptake?

Done:

“OSCAR includes the following climate-carbon feedbacks: the effect of temperature and precipitation change on net primary productivity of land ecosystems, their heterotrophic respiration, and the rate of occurrence of wildfires; and the effect of temperature change on the carbonate chemistry and the stratification of the surface ocean.”

Page 10, Line 3 Please elaborate on how this equation was derived.

The way we derive this equation is explained in the three previous paragraphs. We find difficult to elaborate further. But we have extended one sentence in the third paragraph to make clearer where the Dirac-δ function comes from, and we have put the final equation in a separated paragraph starting with “based on the above” to improve clarity.

Page 10, Lines 9-10 There are many earth system processes that are nonlinear. As something that has been discussed intensively before, I would point out the buffering of ocean CO_2 uptake under rising atmospheric CO_2 concentration. But this nonlinearity can be modeled by a revised IRF approach that treats the atmosphere and the mixed layer as one box (Hooss et al. 2001).

True, but our point was about reversibility. We have added some clarification: "This is however likely unrealistic, given all the existing ~~non-linear~~ processes, such as vegetation migration (e.g. Jones et al., 2009) or permafrost thawing (e.g. Koven et al., 2011), that can produce some degree of irreversibility in the system but are ignored here."

Page 11, Lines 4-5 Related to the comment above, the authors should refer to the relevant debate on the linear limitation of IRF (Joos et al. 1996; Hooss et al. 2001). A detailed biogeochemical discussion is given in Section 2.1.2 of (Tanaka et al. 2007).

We disagree that this section is the place where to mention this 'debate'. We demonstrate that the IRF we derived is only an approximation and therefore has a limited domain of validity, and later (in the conclusion) we remind the reader that more complex models should be used in some cases. We find this sufficient as our paper is not a review on IRFs. Note also that we do discuss the interest of more complex model in sections 5.2.

Page 12, Line 2 This should be "figures 5 and 6" because there is no figure 7 in the current manuscript.

Corrected.

Page 12, Lines 15-17 Related to my major comment, if this is really a recommendation for metric users, this needs to be more highlighted in the text. Metric users would otherwise be left wonder what are the values that should be used for applications.

As explained above, we have decided to not recommend any particular metric. This is not the goal of this paper.

Page 13, Line 20 This is just a minor note, but TOTEM (Ver et al. 1999; Mackenzie et al. 2011), which is one of the models used to derive the IPCC AR5 IRF (Joos et al. 2013), accounts for nitrogen and phosphorus limitations.

Yes. Although this does not play any role in the experimental setup of Joos et al. (2013) – or in ours – since there is no N or P deposition during the establishment of the IRF; just as there is no land-use change. And so we argue that these three drivers (and others) are therefore not accounted for in the IRF, even if the response has been calibrated on a model that in principle includes the drivers.

Page 14, Lines 29-30 I fully agree with this statement.

Thank you.

Page 15, Lines 16-17 I am coming back to the first minor comment. Although I somewhat hesitate to repeat this point because of the conflict of interest, the paper should discuss studies that estimate emission metrics based on models other than IRFs at least at some length. Examples are (Manne and Richels 2001; Tanaka et al. 2009; Gillett and Matthews 2010; Reisinger et al. 2010; Johansson 2012; Smith et al. 2012; Tanaka et al. 2013; Sterner et al. 2014), and there are many more. The current manuscript narrowly focuses on IRF-based studies. I believe that adding more relevant studies should enrich the discussion in this paper and make the argument more convincing.

We have added a reference to the very recent and only paper we know of that is a model-based study of the climate-carbon feedback in emission metrics (Sterner and Johansson, 2017).

Page 27 Figure 4 is not discussed in the paper

It is now. It was just a numbering issue.

Accounting for the climate-carbon feedback in emission metrics

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Abstract. Most emission metrics have previously been inconsistently estimated by including the climate-carbon feedback for the reference gas (i.e. CO_2) but not the other species (e.g. CH_4). In the fifth assessment report of the IPCC, a first attempt was made to consistently account for the climate-carbon feedback in emission metrics. This attempt was based on only one study, and therefore the IPCC ~~presented tentative values and~~ concluded that more research was needed. Here, we carry out this research. First, using the simple carbon-climate model OSCAR v2.2, we establish a new impulse response function for the climate-carbon feedback. Second, we use this impulse response function to provide new estimates for the two most common~~usual~~ metrics: Global Warming Potential (GWP) and Global Temperature change Potential (GTP). We find that, when the climate-carbon feedback is correctly accounted for, the emission metrics of non-CO_2 species increase, but in most cases not as much as initially indicated by IPCC. We also find that, when the feedback is removed for both the reference and studied species, these relative metric values only have modest changes~~,~~ compared to when the feedback is included (absolute metrics change more markedly). ~~However, including carbon-climate feedbacks, particularly in absolute metrics or for short time horizons, gives a more realistic representation of the response.~~ Including or excluding the climate-carbon feedback ultimately depends on the user's goal, but consistency should be ensured in either case.

1 Introduction

Emission metrics are a tool to compare or combine the climate impact of the emission of different greenhouse gases and other climate forcing agents, typically putting them on a so-called CO_2-equivalent scale. The physical meaning of this scale depends on the climate parameter chosen to calculate the metric (e.g. radiative forcing or temperature change), but also on the time-horizon and on whether it is an instantaneous or integrative metric. Emission metrics can be given in absolute terms or in relative terms, the latter being ~~a normalization of~~ the absolute metric ~~to~~taken relatively to that of a reference gas which is usually CO_2. For instance, GWP100 – the most widely used metric – is a ~~normalized~~ relative metric defined as the ratio of the cumulative radiative forcing induced after 100 years by 1 kg of a given species over that induced by 1 kg of CO_2. The GWP100 is currently used in UNFCCC emission inventories, climate agreements (e.g. the Kyoto Protocol), and climate policies (e.g. emissions trading systems). Emission metrics are also used to evaluate multi-gas policies, to compare emissions and sinks from countries and/or economic sectors, or simply as zeroth-order models of the climate system. They are used in areas such as life cycle assessment (e.g. Levasseur et al., 2016), ecosystem service study (e.g. Neubauer and Megonigal, 2015) and integrated assessment modelling (e.g. Clarke et al., 2014). More about emission metrics can be found elsewhere (e.g. Cherubini et al.; 2016; Myhre et al., 2013; Shine et al., 2015).

Since emission metrics are based on simple representations of more complex models, there are choices in how components of complex models are incorporated in the metrics. One such component is the climate-carbon feedback. The "climate-carbon feedback" refers to the effect that a changing climate has onto the carbon-cycle, which impacts atmospheric CO_2, which in turn changes further the climate. In concrete terms: when CO_2 is emitted, the atmospheric CO_2 pool increases. A fraction of this excess atmospheric CO_2 is taken up by the ocean and the terrestrial biosphere (the "carbon sinks"), but as long as a part of the excess CO_2 stays in the atmosphere, it warms the climate. In turn, this warming climate slows down the uptake of the atmospheric CO_2 by the sinks. This slowing down constitutes a positive feedback, i.e. a warming climate is warmed further through the feedback (Ciais et al., 2013). Rather than a slowing down of the carbon sinks, it is also possible to view the feedback as a reduction of the carbon sinks uptake efficiency (Raupach et al., 2014). According to models of the coupled carbon-cycle – climate system, the climate-carbon feedback has contributed to the observed warming over the last century and will have a large impact in warmer future scenarios (e.g. Ciais et al., 2013; Friedlingstein et al., 2006; Raupach et al., 2014),~~.~~ ~~Yet,~~although there are large uncertainties about the magnitude of this feedback and underlying mechanisms~~(e.g. Ciais et al., 2013; Friedlingstein et al., 2006; Raupach et al., 2014)~~.

The standard metrics provided in the fifth assessment report (AR5) of the IPCC (Myhre et al., 2013; table 8.A.1) are inconsistent in their treatment of the climate-carbon feedback. While absolute metrics for CO_2 itself do account for the feedback, the absolute metrics for all other species do not. As a result, the ~~normalized~~ relative metrics, defined as the ratio of the absolute metric of a non-CO_2 species over that of CO_2, are inconsistently calculated. Aware of this limitation, the IPCC made a first attempt at including the climate-carbon feedback into metrics in a consistent manner. This attempt was based on an earlier study by Collins et al. (2013) ~~which~~ whose main object was not the climate-carbon feedback (but regionalized metrics). This study is therefore an attempt to assess the robustness of these alternative but tentative metrics proposed by the IPCC (Myhre et al., 2013; table 8.7).

Here, we carry out an analysis of the climate-carbon feedback and how it can be included in the emission metrics framework. To do so, in section 2, we recall the mathematical framework used to derive emission metrics, and we extend it with a specific term representing the response of the carbon sinks to climate change. In section 3, we use the simple Earth system model OSCAR v2.2 to derive a functional form for this response, and to quantify its numerical parameters. In section 4, we use the extended framework and our new response function to establish new values of metrics that include the climate-carbon feedback, and we compare those with the values otherwise available.

2 Mathematical framework

2.1 Impulse Response Functions

Emission metrics are usually formulated by means of Impulse Response Functions (IRFs), as it is done in the fifth IPCC report (Myhre et al., 2013). These IRFs are simple models which describe the dynamical response of a subsystem of the Earth system (e.g. the biogeochemical cycle of a given species, or the climate system) to a pulse of perturbation of this subsystem. The response of the subsystem to a more general continuous and time-varying perturbation can be obtained by convolution of the IRF with the time-series of the perturbation. The various IRFs used are generally estimated on the basis of idealised simulations made with complex models (e.g. Geoffroy et al., 2013; Joos et al., 1996; 2013). Per construction, ~~the~~ IRFs are dynamical models which feature e.g. inertia and hysteresis, but they are linear in nature with respect to the intensity of the perturbation, they represent a fully reversible system, and they can only include feedbacks in an implicit manner. Despite these apparent caveats, the use of such a linear-response approach to emulate the behaviour of complex systems can be warranted by the theory, especially in the case of the climate system (see e.g. Ragone et al., 2016; Lucarini et al., 2017). Note that emission metrics can also be estimated with more complex model simulations (e.g. Tanaka et al., 2009; Sterner and Johansson, 2017), with the strong caveat that the approach lacks the simplicity and transparency of the IRFs.

Now let us illustrate the typical formulation of the~~is~~ simple IRF-based model ~~in the case~~ of the climate change induced by ~~of~~ a given species (x). The change in atmospheric concentration of the species (Q^x) can be calculated with a convolution between the time-series of anthropogenic emission of this species (E^x) and the IRF for the species' atmospheric concentration (r_Q^x):

$$Q^x(t) - Q^x(0) = \int_{t'=0}^{t} E^x(t')\, r_Q^x(t-t')\, dt'$$

In the most general case, the radiative forcing induced by this species (RF^x) is taken as a function ($\mathcal{F}^x$) of its change in atmospheric concentration (e.g. Myhre et al., 1998):

$$RF^x(t) = \mathcal{F}^x\big(Q^x(t) - Q^x(0)\big)$$

And finally, the change in global mean surface temperature induced by this species (T^x) is again deduced by a convolution of the radiative forcing with the IRF for the climate system. This IRF is broken down into a dynamical term (r_T) and a constant intensity term (λ) that corresponds to the equilibrium climate sensitivity. This gives:

$$T^x(t) - T^x(0) = \lambda \int_{t'=0}^{t} RF^x(t')\, r_T(t-t')\, dt'$$

Typically, the IRF for atmospheric CO_2 is taken from Joos et al. (2013), those for other greenhouse gases are exponential decay functions with a constant e-folding time taken as the "perturbation lifetime" given by Myhre et al. (2013), the radiative forcing functions come from Ramaswamy et al. (2001) with updated radiative efficiencies from Myhre et al. (2013), and the climate

IRF is taken from Boucher and Reddy (2008). Note, however, that updates of the climate IRF based on CMIP5 models are available in the literature (Geoffroy et al., 2013; Olivié et al, 2013) but they have not been widely used so far.

2.2 Formulation of emission metrics

To produce emission metrics IRFs are used, albeit with two important additional assumptions. First, the initial anthropogenic perturbation is actually taken as a pulse of emission at time $t = 0$, which we can write formally with the Dirac-δ function and the size of the pulse (E_0) as follows: $E^x(t) = E_0^x\,\delta(t)$. Strictly speaking, the Dirac-δ is a distribution, and it is the (approximated) identity of the convolution algebra so that the convolution of any function by the Dirac-δ gives back the initial function. Second, since in the metrics framework this pulse is assumed to be very small, the radiative forcing function is approximated to be linear so that we have: $RF^x(t) = \varphi^x\big(Q^x(t) - Q^x(0)\big)\cancel{F^x \equiv \varphi^x}$, where φ^x is the constant marginal radiative efficiency of the considered species. Note that the assumption of a very small pulse may be inconsistent with the way the IRFs are actually derived, as it is currently the case for CO_2 (see appendix A).

From there, we can formulate the Absolute Global Warming Potential (AGWP) and the Absolute Global Temperature-change Potential (AGTP)~~, which are absolute (i.e. non-normalized) metrics~~. Per definition, the AGWP of a species *x* is the cumulative radiative forcing induced by a pulse of emission of the species, normalized by the size of the pulse, and taken up to a chosen time horizon (*H*):

$$
\begin{aligned}
AGWP^x(H) &= \frac{1}{E_0^x} \int_{t=0}^{H} RF^x(t)\,dt \\
&= \frac{1}{E_0^x} \int_{t=0}^{H} \varphi^x \int_{t'=0}^{t} E_0^x\,\delta(t')\,r_Q^x(t-t')\,dt'\,dt \\
&= \varphi^x \int_{t=0}^{H} r_Q^x(t)\,dt
\end{aligned}
$$

Per definition, the AGTP of a species *x* is the instantaneous temperature change induced by a pulse of emission of the species, normalized by the size of the pulse, and taken at a chosen time horizon:

$$
\begin{aligned}
AGTP^x(H) &= \frac{1}{E_0^x}\,[T^x(H) - T^x(0)] \\
&= \frac{1}{E_0^x}\,\lambda \int_{t=0}^{H} \varphi^x\,r_T(H-t) \int_{t'=0}^{t} E_0^x\,\delta(t')\,r_Q^x(t-t')\,dt'\,dt \\
&= \varphi^x\,\lambda \int_{t=0}^{H} r_Q^x(t)\,r_T(H-t)\,dt
\end{aligned}
$$

The Global Warming Potential (GWP) and the Global Temperature-change Potential (GTP) are metrics ~~normalized~~ calculated relatively to the reference gas CO_2. Therefore, any of these two metrics is defined as the ratio of its absolute counterpart for the species x over that for CO_2:

$$GWP^x(H) = \frac{AGWP^x(H)}{AGWP^{CO_2}(H)}$$

and:

$$GTP^x(H) = \frac{AGTP^x(H)}{AGTP^{CO_2}(H)}$$

We can now detail the inconsistency mentioned in introduction, regarding the way the default GWPs and GTPs are estimated by the IPCC (Myhre et al., 2013; table 8.A.1). To estimate the absolute metrics for CO_2, the IRF derived by Joos et al. (2013) is used, and one feature of this IRF is that it implicitly includes any feedback between the climate system and the carbon-cycle that is also included in the complex carbon-climate models it is calibrated upon. However, the absolute metrics for non-CO_2 species do not include the effect of the warming climate onto the carbon-cycle that is induced by the non-CO_2 species. In other words, the climate-carbon feedback is included in the denominator of the GWP and GTP, but *not* in their numerator. The resulting metric values should therefore be regarded as inconsistent.

2.3 Addition of the climate-carbon feedback

To include the climate-carbon feedback in the metric framework, we choose to model the decrease in the carbon sinks efficiency induced by climate change as an additional flux of carbon to the atmosphere, but without changing the atmospheric lifetime of carbon dioxide. Another approach, mathematically equivalent, would be to change the atmospheric lifetime of the gas. However, the latter approach cannot be used with the IRF framework since, per construction, the atmospheric lifetimes of all the species are fixed.

We define the change in the global carbon sinks ΔF. It is positive if the flux goes into the atmosphere, i.e. if the sinks efficiency is actually reduced. By analogy with previous IRF-based equations, we propose the following formulation:

$$\Delta F^x(t) = \gamma \int_{t'=0}^{t} [T^x(t') - T(0)]\, r_F(t - t')\, dt'$$

In this equation, the forcing term is the global mean temperature change induced by the species x. The IRF for the carbon sinks is broken down into two terms: a dynamical term that is r_F, expressed in yr^{-1}; and an intensity term that is γ, expressed in GtC yr^{-1} K^{-1}. There are two implicit assumptions with this formulation which are discussed hereafter. First, we assume that the carbon sinks response is the same, at global scale and for a given temperature change, whatever the forcing species. Second, we assume that the global mean temperature is a proxy of all the changes in the climate variables that drive a change in the carbon sinks, such as local temperature itself but also precipitation.

To simplify the discussion and avoid quintuple integrals, we introduce the simplified notation $\star$ for the convolution: $a \star b \equiv \int_0^t a(t')\, b(t-t')\, dt'$, and note the commutative property of the convolution: $a \star b = b \star a$.

Since the change in carbon sinks is expressed as a new source of CO_2, one can calculate the additional radiative forcing (ΔRF) induced by a species x through the climate-carbon feedback:

$$\begin{aligned}
\Delta RF^x &= (\varphi^{CO2})\, \Delta F^x \star r_Q^{CO2} \\
&= (\varphi^{CO2}\, \gamma)\, [T^x - T(0)] \star r_F \star r_Q^{CO2} \\
&= (\varphi^{CO2}\, \gamma\, \lambda)\, RF^x \star r_T \star r_F \star r_Q^{CO2} \\
&= (\varphi^{CO2}\, \gamma\, \lambda\, \varphi^x)\, [Q^x - Q^x(0)] \star r_T \star r_F \star r_Q^{CO2} \\
&= (\varphi^{CO2}\, \gamma\, \lambda\, \varphi^x)\, E^x \star r_Q^x \star r_T \star r_F \star r_Q^{CO2}
\end{aligned}$$

and similarly with the additional temperature change (ΔT):

$$\begin{aligned}
\Delta T^x &= (\lambda)\, \Delta RF^x \star r_T \\
&= (\varphi^{CO2}\, \gamma\, \lambda^2\, \varphi^x)\, E^x \star r_Q^x \star r_T \star r_F \star r_Q^{CO2} \star r_T
\end{aligned}$$

We do not need to worry about the endless feedback loop CO_2–climate–CO_2 and add more terms to these equations, because the carbon dioxide IRF (r_Q^{CO2}) already accounts for the effect of climate change on the CO_2 concentration.

It is possible to formulate the additional absolute GTP (ΔAGTP) – which is later added to the AGTP without feedback – for the species x:

$$\begin{aligned}
\Delta AGTP^x &= \frac{1}{E_0^x}\, \Delta T^x \\
&= \left(\frac{1}{E_0^x}\, \varphi^{CO2}\, \gamma\, \lambda^2\, \varphi^x\, E_0^x\right) \delta \star r_Q^x \star r_T \star r_F \star r_Q^{CO2} \star r_T \\
&= (\gamma)\, r_F \star \underbrace{(\varphi^x\, \lambda)\, r_Q^x \star r_T}_{AGTP^x} \star \underbrace{(\varphi^{CO2}\, \lambda)\, r_Q^{CO2} \star r_T}_{AGTP^{CO2}}
\end{aligned}$$

that is:

$$\Delta AGTP^x(H) = \gamma \int_{t=0}^{H} r_F(H-t) \int_{t'=0}^{t} AGTP^x(t')\, AGTP^{CO2}(t-t')\, dt'\ dt$$

To formulate ΔAGWP, it is easier to do the same demonstration if one introduces the Heaviside step function (i.e. the function equal to 1 for $t \geq 0$, and 0 otherwise; noted Θ) and notes that convoluting any function with the Heaviside function is equivalent to integrating it. ~~;~~ T~~t~~he definition of AGWP then is:

$$AGWP^x(H) = \frac{1}{E_0^x} \int_{t=0}^{H} RF^x(t)\, dt \equiv \frac{1}{E_0^x}\, RF^x \star \Theta$$

Hence, similarly to the case of ΔAGTP, we have:

$$\Delta AGWP^x = \frac{1}{E_0^x}\, \Delta RF^x \star \Theta$$

$$= (\gamma)\, r_F \star \underbrace{(\varphi^x\, \lambda)\, r_Q^x \star r_T}_{AGTP^x} \star \underbrace{\varphi^{CO2}\, r_Q^{CO2} \star \Theta}_{AGWP^{CO2}}$$

that is:

$$\Delta AGWP^x(H) = \gamma \int_{t=0}^{H} r_F(H-t) \int_{t'=0}^{t} AGTP^x(t')\, AGWP^{CO2}(t-t')\, dt'\, dt$$

The above discussion holds in the case of species-dependent climate sensitivity parameters, i.e. if we have λ^x instead of λ to account for climate efficacies (e.g. Hansen et al., 2005). These two formulas, for ΔAGWP and ΔAGTP, are similar to those given by Collins et al. (2013) in their section 5.5, where they implicitly assume that: $\gamma\, r_F(t) = \Gamma\, \delta(t)$, where Γ is a constant. Collins et al. (2013) therefore assumes that the carbon sinks response to a pulse of global temperature change was a pulse of size Γ of CO_2 outgassing by the ocean and the terrestrial biosphere, but did not justify this assumption. The next section investigates whether this assumption holds, and what functional form can be chosen for the dynamical function r_F.

3 Estimating the climate-carbon feedback response

3.1 Experimental setup

We use the compact Earth system model OSCAR v2.2 to establish the IRF of the climate-carbon feedback (Gasser et al., 2016). It embeds several modules dedicated to simulating the response of many subsystems of the Earth system; and more specifically to our case, it embeds modules for the oceanic carbon-cycle, the terrestrial carbon-cycle and the climate system. Each of these modules is designed to emulate the sensitivity of more complex – usually spatially explicit – models. In the version used here, the complex models used to calibrate OSCAR were used for the IPCC AR5 via the Coupled Model Intercomparison Project phase 5 (CMIP5). OSCAR includes the following climate-carbon feedbacks: the effect of temperature and precipitation change on net primary productivity of land ecosystems, their heterotrophic respiration, and the rate of occurrence of wildfires; and the effect of temperature change on the carbonate chemistry and the stratification of the surface ocean. OSCAR is used in a probabilistic setup, which means that ensembles of simulations are made so as to be able to derive an uncertainty distribution for our results. These Monte Carlo ensembles contain 1200 elements; each element being the outputs of a simulation done with a set of parameters drawn with equiprobability from the pool of available parameterizations of OSCAR (Gasser et al., 2016). The configuration used here is similar to the one called “offline” by Gasser et al. (2016), and more information as to the basic performance of the model is also provided therein.

Before estimating the IRF for the climate-carbon feedback, we benchmark OSCAR’s IRFs of the carbon-cycle and climate system separately against commonly used IRFs. For the carbon-cycle, we follow the protocol by Joos et al. (2013), reproduced in appendix A, and we repeat it a second time while turning off all the climate-carbon feedbacks of the model. The two carbon dioxide IRFs obtained are shown in figure 1a. The IRF obtained when the feedbacks are turned on is very close to the one derived by Joos et al. (2013) and used by the IPCC. When the feedbacks are turned off, the IRF decays faster than when they are on, which means that the carbon sinks are more efficient – as expected. Regarding the climate response, since OSCAR's climate module is a two-box model with constant coefficients, it is equivalent to an IRF, shown in figure 1b. The model's response is close to the average of sixteen CMIP5 models as calculated by Geoffroy et al. (2013), but it differs from the one used in the IPCC AR5 (Boucher and Reddy, 2008). Together the ability of the OSCAR model to reproduce the carbon-cycle and climate IRFs derived from up-to-date and complex models suggests that it is also capable of establishing a reasonable IRF for the climate-carbon feedback.

To estimate this climate-carbon feedback IRF, we adopt a protocol largely inspired by that of Joos et al. (2013) for the carbon dioxide IRF. A first simulation is made to calculate the background conditions, in which atmospheric CO_2 and non-CO_2 radiative forcings are prescribed up to 2010 exactly as it is done with the first simulation of the protocol for the carbon dioxide IRF (see appendix A). These prescribed forcings are then maintained for another 1000 years of simulation. The climate variables simulated in this first experiment are saved to be used in the second simulation. In OSCAR, these variables are the air surface temperature (global and regional over land), the sea surface temperature (global), and precipitation (global and regional over land). A second simulation is made in which the same atmospheric CO_2 and non-CO_2 radiative forcings are prescribed, along with the climate variables saved previously. In this second experiment, in the year 2015 and afterwards, a constant climate perturbation is added on top of the prescribed climate from the first experiment. This perturbation has a global average surface temperature change of +0.2°C, but the local temperature and precipitation perturbations do vary spatially, following the response patterns used in OSCAR and calibrated on complex models (Gasser et al., 2016). In our model, these regional response patterns are easy to obtain, since they are proportional to the global average temperature change, but for more complex models the protocol might have to be adapted (see discussion). Finally, the climate-carbon feedback response (not yet the IRF of section 2.3) is calculated as the difference between the global CO_2 flux from the oceanic and terrestrial carbon reservoirs to the atmosphere simulated in the second and first experiments, normalized by the size of the global temperature step, and setting the time origin ($t = 0$) as the starting year of the step (i.e. 2015).

3.2 Results

Figure 2 shows the carbon sinks' response to the temperature step change simulated by OSCAR v2.2. Panel (b) shows the model change in surface flux due to decreased carbon sinks, panel (a) shows the cumulative response from summing the flux and panel (c) shows the differentiated response from taking the year-to-year difference in flux. If the yearly response is the "speed" of outgassing of the carbon sinks, the differentiated response is its "acceleration". It is important to note that the analytical time-step of OSCAR is one year, and that it is not a process-based model. It is thus impossible to specifically distinguish the very short-term response of the carbon sinks to the step of climate change. Despite this limitation, over the period of time we can study, the response simulated by OSCAR is very different from that assumed by Collins et al. (2013).

In OSCAR, the response of the carbon sinks to a step of climate change is an instantaneous burst of outgassing followed by more outgassing that is however decreasing in intensity with time, despite the constant intensity of the forcing (figure 2b). We also find the land carbon flux response is about double that from the ocean (not shown). This response is physically very different from Collins et al. (2013) and thus the IPCC, where it is assumed that the carbon sinks response to a pulse of climate change is a pulse of outgassing, or equivalently that their response to a step of climate change is a step of outgassing. This would imply that under a stabilized but changed climate (e.g. at +2°C on global average) the carbon sinks would endlessly release CO_2 to the atmosphere. This is unrealistic, since the total emitted CO_2 is limited by the size of the natural reservoirs. Our simulations show the carbon sinks behaving in a more reasonable and expected way. Under a step of climate change, the sinks do release CO_2 – which is consistent with the positive sign of the climate-carbon feedback – but the release of CO_2 slows down with time (figure 2b), until the sinks reach a new equilibrium under a new climate. This behaviour implies that the total amount of released CO_2 is capped (figure 2a) and is given by the difference in the natural carbon pools between the two equilibria under the two different climatic backgrounds. The response to a pulse of climate change is indeed a burst of outgassing; however, after the pulse, the atmospheric CO_2 is now raised above the equilibrium level so the sinks increase, eventually recapturing the lost carbon (figure 2c). The latter part of the response was missing from Collins et al. (2013).

3.3 Estimating the IRF

In this section, we estimate a functional form for the climate-carbon feedback IRF that will then be used to estimate new emission metrics. We look only at the time period covered by our simulations with OSCAR, therefore ignoring the discontinuity around $t = 0$. Let us call f the function of the time variable that will fit the simulated cumulative response (figure 2a). The yearly response (figure 2b) is thus fitted by f' – its first derivative – and the differentiated response (figure 2c) by f'' – its second derivative. The functional form of f is chosen to be a sum of three saturating exponential functions, consequently:

$$f(t) = \gamma\left(\alpha_1\tau_1\left(1-\exp\left(-\frac{t}{\tau_1}\right)\right)+\alpha_2\tau_2\left(1-\exp\left(-\frac{t}{\tau_2}\right)\right)+\alpha_3\tau_3\left(1-\exp\left(-\frac{t}{\tau_3}\right)\right)\right)$$

$$f'(t) = \gamma\left(\alpha_1\exp\left(-\frac{t}{\tau_1}\right)+\alpha_2\exp\left(-\frac{t}{\tau_2}\right)+\alpha_3\exp\left(-\frac{t}{\tau_3}\right)\right)$$

$$f''(t) = -\gamma\left(\frac{\alpha_1}{\tau_1}\exp\left(-\frac{t}{\tau_1}\right)+\frac{\alpha_2}{\tau_2}\exp\left(-\frac{t}{\tau_2}\right)+\frac{\alpha_3}{\tau_3}\exp\left(-\frac{t}{\tau_3}\right)\right)$$

Each of the three exponentials is parameterized by a time constant τ_i and a weight α_i, and the overall function is also parameterized by its intensity γ. The γ parameter is introduced here by choice, and it is the same as in section 2.3. Since we introduce a seventh parameter while only six were needed (we could have defined three γ_i as $\gamma_i = \gamma\,\alpha_i$), we also add the constraint that $\alpha_1 + \alpha_2 + \alpha_3 = 1$. The choice of an exponential-based functional form is motivated by the fact that all other IRFs typically used for emission metrics are also formulated with exponentials, because it allows closed-form analytical solutions of all the convolutions. Another interest of exponential-based IRFs is the possibility to use Laplace transforms to study the carbon-climate system (Enting, 2007).

To deduce numerical values for the parameters, we fit the f function and its first and second derivatives over the three response curves simulated by OSCAR and shown in figure 2. ~~To do so, we use only the actual outputs of OSCAR, i.e. the fit is made~~

~~only over the simulated curves and not over the extended ones.~~ To determine the six freely-varying parameters, we proceed in four steps that are detailed in appendix B. Table 1 shows the parameters obtained by repeating the procedure for the average, upper and lower responses of the ensemble. The intensity parameter of the response (γ) is ~3.0 GtC yr^{-1} K^{-1}. The three time constants of the carbon sinks response are consistent with the atmospheric CO_2 response of OSCAR, but there is more weight placed on the faster modes so that the carbon response to a temperature pulse is faster than the carbon response to a CO_2 pulse. However, it is extremely difficult to relate any of the physical processes to these parameters (Li et al., 2009). We also tried other functional forms for this fit, specifically forms with fewer exponentials, but it was not possible to capture both the dynamics of the first few years and that of the last hundreds of years.

The response obtained with OSCAR exhibits a discontinuity around $t = 0$ (figure 2) as the model cannot simulate the response of the carbon sinks over short time-scales (<1 yr). We assume nonetheless that the flux perturbation can be extrapolated back to $t = 0^{+}$, neglecting any processes faster than a year that we cannot represent. Thus the discontinuity at $t = 0$ is modelled with a Dirac-δ function whose intensity is equal to the value of the flux at $t = 0^{+}$. The resulting extension of the simulated response is schematically shown in figure 2. ~~and~~

Based on the above, we can finally propose a mathematical expression of the climate-carbon feedback IRF defined in section 2.3:

$$\gamma\, r_F(t) = f'(0^{+})\delta(t) + f''(t)$$
$$= \gamma\delta(t) - \gamma\left(\frac{\alpha_1}{\tau_1}\exp\left(-\frac{t}{\tau_1}\right) + \frac{\alpha_2}{\tau_2}\exp\left(-\frac{t}{\tau_2}\right) + \frac{\alpha_3}{\tau_3}\exp\left(-\frac{t}{\tau_3}\right)\right)$$

The constraint $\alpha_1 + \alpha_2 + \alpha_3 = 1$ implies that $\int_{0^-}^{+\infty} r_F(t) = 0$. This means that, in our framework, a pulse of climate change has no effect on the natural carbon pools on the very long-term. In other words, in the response shown in figure 2c, the (infinite) recovery period fully compensates for the initial pulse of CO_2 emission. This idealised feature of reversibility is to be expected from the simple and linear modelling framework that the impulse response functions are, since no multiple equilibria is permitted. This is however likely unrealistic, given all the existing ~~non-linear~~ processes, such as vegetation migration (e.g. Jones et al., 2009) or permafrost thawing (e.g. Koven et al., 2011), that can produce some degree of irreversibility in the system but are ignored here.

3.4 Influence of step size and background conditions

To assess the robustness of our IRF, as well as its domain of validity, we repeat the simulations with different steps of temperature. We derive IRFs for climate change steps corresponding to a global mean temperature increase of: +0.01, +0.1, +0.2, +0.5, +1, +2, +3, +4, +5 and +10 °C. We note however that for the latter values, and especially for +10°C, we are pushing the model into a domain where its performance is questionable. The parameters we obtain for each experiment are shown in figure 3. The climate-carbon feedback intensity (γ) decreases when the step size increases. Since the intensity is normalized by the step size, this does not mean the feedback is weaker when climate change is stronger. This rather means the carbon sinks response is non-linear in intensity: a doubled step of climate change induces less than a doubled outgassing of the natural reservoirs. This saturation effect can be explained by the limited size of the reservoirs: the fewer carbon remains, the harder it is to get it out (i.e. the more energy is required). The climate-carbon feedback effective time-scale (τ_{eff}; calculated as $\tau_{\mathrm{eff}} = \sum_i \alpha_i \tau_i$) also decreases when the step size increases, indicating that under a stronger climate change perturbation the carbon sinks outgassing occurs faster. These two non-linear behaviours appear small for the very small perturbations (i.e. below +1°C). We also repeat the simulations with different background conditions, though only for climate change steps corresponding to a global mean temperature increase of +0.2 and +1 °C. Four different background conditions are obtained with a slight alteration of our protocol: the background-setting part of the simulation – i.e. before the step of climate change – is extended to follow each Representative Concentration Pathway (RCP) atmospheric CO_2 and radiative forcing data (Meinshausen et al., 2011) from 2005 to 2100, and the step occurs in 2105 instead of 2015. Figure 3 shows that the higher the atmospheric CO_2 and global warming of the background, the more intense and faster the climate-carbon feedback, with a doubling of the intensity parameter (γ) and a decrease by one-third of the time-scale parameter (τ_{eff}) under RCP8.5. These results can be explained by the increased amount of carbon stored in the natural reservoirs at the time of the climate change step, as in the model the carbon sinks keep removing CO_2 from the atmosphere during the RCP simulation while atmospheric CO_2 is higher than today. These results are also consistent with those regarding the atmospheric CO_2 IRF (Joos et al., 2013): under a higher CO_2 and temperature background, it is harder for the carbon sinks to remove CO_2 from the atmosphere (slower carbon dioxide IRF) and it is easier for them to release the carbon they are already storing (stronger and faster feedback IRF). Both studies – that of Joos et al. (2013) and ours – therefore show that the carbon-cycle is a non-linear system that can be only approximatively emulated by impulse response functions.

4 New estimates of emission metrics

Using the estimated IRF for the climate-carbon feedback, we now provide new estimates of the two most common emission metrics, GWP and GTP, for five species spanning a broad range of atmospheric lifetimes and climate impacts: methane (CH_4), nitrous oxide (N_2O), sulphur hexafluoride (SF_6), black carbon (BC) and sulphur dioxide (SO_2). We follow the methodology used by the IPCC in the AR5 (Myhre et al., 2013): we use the perturbation lifetimes for non-CO_2 species and the radiative efficiencies they provide (their table 8.A.1), the carbon dioxide IRF from Joos et al. (2013), and the climate IRF from Boucher and Reddy (2008). For BC and SO_2, because the IPCC does not provide a unique set of parameters for these short-lived species, we choose the globally averaged ones from Fuglestvedt et al. (2010). We also have to settle on one of our climate-carbon feedback IRFs: we choose the one corresponding to present-day background conditions and a global climate change step of +0.2°C. This choice is motivated by the fact that +0.2°C is approximatively the globally averaged peak warming induced by a pulse of CO_2 emission of 100 GtC which is itself the value chosen by Joos et al. (2013)~~—~~ and ~~therefore by~~ used in the IPCC AR5. We then use the equations given in section 2.3, solving the convolutions numerically with a time-step of one tenth of a year. Figure 4~~5~~ is provided as an illustration of this process whereby we calculate the ΔAGTP of methane, starting from the initial pulse of CH_4 and going through the five successive convolutions described earlier.

The metrics values are shown in figures 5~~6~~ (AGWPs and AGTPs) and 6~~7~~ (GWPs and GTPs). In these figures, we show separately the default IPCC metrics (Myhre et al., 2013; table 8.A.1) and the additional effect of the climate-carbon feedback (i.e. the Δ-term that will then be added to the metrics) obtained with both the Collins et al. (2013) formulation and ours. The Δ-terms always act to increase the magnitude of both the absolute and relative climate metrics. Although the Δ-terms from Collins et al. (2013) are of similar orders of magnitude, their function forms are very different. Since Collins et al. (2013) did not include the re-uptake of carbon following the initial pulse, their Δ-terms keep increasing with the time horizon, while ours peak and decline. Eventually, the Collins et al. Δ-term is even larger than the default metric on long timescales, which is never the case with our formulation. Note that there is no Δ-term for CO_2 as the climate-carbon feedback is already included in the default metrics, hence including it in the metrics for non-CO_2 species restores consistency.

In table 2 (first three rows) we show the climate metrics, including and excluding Δ-term, for three chosen time horizons: 20, 50 and 100 years. There, one can see again that the metrics are systematically higher (in absolute value) than in the default IPCC case, when the climate-carbon feedback induced by non-CO_2 species is accounted for, whatever the chosen formulation. Quantitatively, however, for long time horizons, the IPCC (Myhre et al., 2013; table 8.7), based on Collins et al. (2013), overestimates the effect of the climate-carbon feedback, whereas this effect is underestimated for short time horizons. This can also be seen in figures 5~~6~~ and 6~~7~~ where the dotted lines are below the dashed ones during the first decades, and over afterwards.

In table 2 (fourth row), we also provide new estimates of the metrics including the climate-carbon feedback as calculated with OSCAR, but also ~~and~~ with the climate IRF updated from that of Boucher and Reddy (2008) to that of Geoffroy et al. (2013). The latter is calibrated on several climate models of the latest generation, while the former appears to be an outlier of the CMIP5 ensemble – see our figure 1b and results for "HadGEM2-ES" provided by Geoffroy et al. (2013). In concrete terms, the IRF of Boucher and Reddy (2008), used by the IPCC, is slower but has a higher climate sensitivity than the one calibrated on the CMIP5 multi-model mean. The effect of this update can be seen by comparing the third and fourth rows of our table 2. Updating the climate IRF has more effect on the GTPs than on the GWPs, which is logically due to the fact that GTP is defined as a function of the temperature (see section 2.2) while GWP is a function of the radiative forcing and is therefore affected by the temperature only through the climate-carbon feedback. Changing the climate IRF impacts the GTPs for all species, but for short-lived species (BC and SO_2, and to a lesser extent CH_4) a revised climate IRF has an effect as large as correcting the climate-carbon feedback term. This is a reminder of the sensitivity of the GTPs to the representation of the climate time-scales (in r_T), and that these are at least as important as including or neglecting the climate-carbon feedback.

In table 2 (fifth row), we provide another set of relative metrics, similar to the previous one in that it includes the feedback response calibrated on OSCAR and the updated climate IRF, but it also includes an update of the radiative efficiencies of CO_2, CH_4 and N_2O (Etminan et al., 2016). The new radiative efficiency of CO_2 differs by +2%, that of CH_4 by +14%, and that of N_2O by -3%. These changes logically impact the GWPs and the GTPs, since both metrics are function of the φ^x parameters. The change is substantial for CH_4; in most cases more so than the update of the climate IRF. Notably, the update of the radiative efficiency of CO_2 – being the reference gas in relative metrics – implies a change in the metrics' values of all species, even those whose own radiative efficiency are not changed. These results show that the first-order processes (here, the radiative forcing) may have more impact on the metrics than second-order processes such as the climate-carbon feedback.

~~We recommend using the metrics shown in this fourth row of table 2, since they are the most consistent, robust and up-to-date metrics available. Analytical expressions of the IRFs, to be used to calculate metrics for other time horizons and/or other species, are given in appendix C.~~

In table 2 (last two rows), to fully understand the effect of including or not the climate-carbon feedback in emission metrics, we provide two other sets of metrics: the two are based only on IRFs derived from OSCAR (i.e. the responses shown in figures 1 and 2), with one including the feedback for both CO_2 and non-CO_2 while the other does not for either. In both cases (i.e. when the climate-carbon feedback is consistently included or excluded) the metrics are very close. For greenhouse gases (here: CH_4, N_2O and SF_6) the difference remains below 10%, with only very small changes for the GWPs. Only in the case of the GTP of short-lived species (BC and SO_2) and for short time horizons is the difference larger than that, reaching about 30%.

Finally, we show in table 3 that the relative uncertainties associated with these OSCAR-based metrics – calculated using our Monte Carlo ensembles and uncertainty ranges from Myhre et al. (2013; table 8.SM.12) – remain close, no matter whether the climate-carbon feedback is included or not, as long as it is consistent. This can be explained by the fact that the climate-carbon feedback only makes a small contribution to the climate metrics. Therefore, despite being highly uncertain, it does not contribute much to the overall uncertainty.

5 Discussion and conclusion

We have developed a theoretical framework to consistently include the climate-carbon feedback in emission metrics, we have used the simple model OSCAR v2.2 to establish an IRF for the feedback, and finally, we have used the framework and the new IRF to propose new estimates of the GWP and GTP. The overarching goal of our study was to correct and complement the work initiated by Collins et al. (2013) and reflected by the IPCC, and to provide a framework that could be used in future IPCC assessment reports. To this end, we see two technical points that must be discussed: one regarding the underlying assumptions made when we extend the IRF framework to include the climate-carbon feedback; and one regarding the possibility of applying our protocol to more complex models. And to conclude, we open up the discussion to more general considerations about the IRF framework and the interest (or lack thereof) of accounting for the climate-carbon feedback in emission metrics, and about the role of non-CO_2 species in the global climate system.

5.1 Technical aspects

In our extended metrics framework, to account for the climate-carbon feedback, we link the global mean temperature change to the global total change in carbon removal by the natural sinks. This global approach averages over differing regional responses. Consequently, the causal links between i) global climate change and local climate changes, and ii) local climate changes and local responses of the ocean or land sinks are accounted for only implicitly with our modelling approach. Regarding the first causal link, since we apply the same IRF ($\gamma\ r_F$) whatever the forcing species x, we implicitly assume that the local pattern of climate change is always the same~~ whatever~~. This is certainly not the case in reality for temperature (e.g. Hansen et al., 2005) or precipitation (e.g. Shine et al., 2015); note that~~and~~ the latter affects the land sink. This could be addressed by repeating our experiment with different patterns of temperature and precipitation corresponding to various forcers so as to deduce species-dependent IRFs in the form, for instance, of a set of γ^x parameters. Regionally varying climate responses have been explored by e.g. Shindell and Faluvegi (2009) and Collins et al. (2013) and could in principle be used to generate species dependent r_F, although they are very uncertain. Regarding the second causal link, i.e. from local climate change to local carbon sinks response, the local response to climate change can be of a sign different from the global one, and further altered if nutrients such as nitrogen are accounted for (Ciais et al., 2013). Therefore, if IRFs were established at the regional scale, they would not likely resemble the one shown in figure 2.

We have established an analytical expression for the climate-carbon feedback IRF with a simple carbon-climate model and following a specific protocol. Although OSCAR performs well in simulating historical changes in the global Earth system (Gasser et al., 2016) and in calculating carbon dioxide and climate IRFs (see figure 1), our simulations should be reproduced with other – more complex – carbon-climate models, to check whether our results hold qualitatively and quantitatively. Ideally, a multi-model modelling exercise such as the one that led to the carbon dioxide IRF (Joos et al., 2013) should also include the simulations required to establish the climate-carbon feedback IRF. For a complex carbon-cycle model, the step climate change could be defined as the difference between the end of a CMIP 4×CO_2 experiment and the control experiment (simulated by the same model). Note that step changes rather than gradual changes such as +1%/yr CO_2 increase (e.g. Arora et al., 2013) are needed in order to derive the IRFs.

5.2 Conceptual aspects

In a more general perspective, our results raise the question of whether the climate-carbon feedback should be included in emission metrics. Accounting for the feedback implies more simulations in a multi-model exercise similar to that of Joos et al. (2013) for calibration purposes, whereas not accounting for it requires a new set of CO_2 IRFs with the feedback turned off. ~~For most greenhouse gases (e.g. CH_4, N_2O, SF_6), we found that inclusion of the climate-carbon feedback does not greatly change (less than 10%) the values of the normalized GWPs and GTPs provided the feedback is included consistently for CO_2 and non-CO_2 species. For very short-lived species (e.g. SO_2, BC) the feedback does have a significant effect over short time-horizons (greater than 30%). The absolute metrics do change substantially when including climate-carbon feedback. Including the climate-carbon feedback therefore gives consistency and accuracy across a wide range of species and time horizons.~~ We have found that including or excluding the climate-carbon feedback in a consistent manner does not greatly change the values of the relative GWPs (only about 2%). In the case of relative GTPs, the change is slightly larger for greenhouse gases (less than 10%) and becomes even larger for very short-lived species and over short time-horizons (greater than 30%). In the case of absolute metrics – both AGWPs and AGTPs – these changes are substantial since we are adding a positive feedback to the model. Therefore, the choice of including or excluding the feedback ultimately depends on the user's needs. On the one hand, for the sake of simplicity and transparency, the feedback could be excluded from the evaluation of GWPs, since it avoids the trouble of the five convolutions shown in figure 4. On the other hand, if absolute (e.g. time-varying) metrics are used as a first-order model of climate change, one may prefer including the climate-carbon feedback to have a better representation of the system. We provide in appendices C and D all the analytical expressions needed to calculate the metrics with or without the feedback.

It is also important to note that the above changes in the metrics' value are of the same order of magnitude (and sometimes less) as the change induced by the update of the climate IRF and the radiative efficiencies of greenhouse gases, as shown in section 4. Hence multiple types of physical properties need to be correctly accounted for. They are also less in magnitude than those induced by the choice of the protocol used to calculate the metrics, such as the background conditions (e.g. Reisinger et al., 2011), or by the choice of a given time horizon (see e.g. table 2). Although these factors reflect choices about temporal applicability of the metrics rather than refined understanding of physical behaviour.

If the choice is made that this feedback be included in emission metrics, it then raises another question as to what other feedbacks, if any, should also be included. Let us take the climate-wetlands feedback as an example. When climate changes, so does the amount of CH_4 emitted by natural wetlands (e.g. Ciais et al., 2013). This could be included in a manner similar to what we did with the climate-carbon feedback: the atmospheric CH_4 IRF should be re-calculated with interactive wetlands, and a new IRF for the climate-wetlands feedback induced by non-CH_4 forcers should be established. This is feasible; but now one must consider that wetlands emissions are also directly affected by atmospheric CO_2 through CO_2-fertilisation and altered stomatal closure that alters the local hydrological cycle (Ciais et al., 2013). Therefore, accounting for the carbon-climate-wetlands nexus requires a much more complex experimental setup. And this is just one example: feedbacks involving biogeochemical cycles in the Earth system are numerous (Ciais et al., 2013). It can be rightfully argued that some of these feedbacks can be neglected, and that others can be safely linearized (such as the CH_4-OH feedback that is included in emission metrics in the AR5). Nevertheless, it appears that we are reaching the limits of the IRF framework which is linear by essence. The alternative, to include all the possible feedbacks in emission metrics, is actually to develop model-based estimates similarly to what is done for atmospheric chemistry, for instance to look at species-species interactions (e.g. Shindell et al., 2009), regional specificities (e.g. Collins et al., 2013) or the seasonality of processes and drivers (e.g. Aamaas et al., 2016). However, this ~~would be~~is at the expense of the simplicity and transparency that are characteristic of the impulse response functions. For the climate-carbon feedback, Sterner and Johansson (2017) recently proposed a first model-based estimate. Their results show the same difference in physical behaviour when compared to Collins et al. (2013) as ours, therefore strengthening our conclusions as to the need to update the IPCC metrics' estimates.

It could also be argued that, rather than concentrating on improving the level of detail in representing the typical climate impacts associated with GWP and GTP (i.e. radiative forcing and global temperature change, respectively), it would be more useful if metrics were instead expanded to more comprehensively capture the full range of environmental impacts associated with emissions, such as extreme events, crop yields or air pollution (e.g. Shindell, 2015).

~~But u~~Ultimately, the new IRF we established also sheds some light on the climate-carbon feedback and on the role of non-CO_2 species in the global climate system. Using a simple model, a robust framework and idealised experiments, we complement earlier studies on the climate-carbon feedback (e.g. Friedlingstein et al., 2006; Arora et al., 2013) with new qualitative insights as to the dynamics of the feedback. This complex dynamics – summed up in our figure 4~~5~~ – has the peculiar effect of giving a long-term impact to short-lived species. Therefore, our work shows that non-CO_2 species have an additional impact on the global climate system through this feedback loop, as others showed before (e.g. Gillet and Matthews, 2010; Mahowald, 2011; MacDougall and Knutti, 2016). It must be understood, however, that this "enhancement" of the non-CO_2 species' impact – as called by MacDougall and Knutti (2016) – does not actually imply that non-CO_2 species are comparatively more important, in the context of climate change mitigation, than initially though. In fact, while it is true that the climate impact of non-CO_2 species is increased via the climate-carbon feedback (i.e.~~–~~ their absolute metrics are increased)~~–~~ so is the climate impact of CO_2 alone; so that the relative importance of non-CO_2 species *versus* CO_2 when the feedback is included for both remains surprisingly close to the case in which the feedback is not included (i.e.~~–~~ their ~~normalized~~ relative metrics remain similar).

5.3 Concluding remarks

As pointed out in the IPCC AR5, the metric calculations should consistently include the same processes for both CO_2 (denominator) and non-CO_2 emissions (numerator). We have explored including the climate-carbon feedback in both, and revised the preliminary calculations presented in the AR5. Given the complexities of the climate-carbon feedback, it would be beneficial to have more studies, with models of varying complexity, to verify our conclusions. Given that inclusion of the climate-carbon feedback has the greatest impact on metrics with short-lived climate forcers, it would be especially interesting to examine the impact of their inhomogeneous distributions on the spatial pattern of the climate-carbon response.

~~To avoid potential biases in metric values, we suggest to include the climate-carbon feedback in metric estimates.~~ Ultimately, whether emission metrics should include the climate-carbon feedback is a decision for the user, and we only recommend consistency in the way feedbacks are included or excluded. The trade-off between simplicity and transparency on the one hand, and accuracy of representation on the other hand, has to be weighed by the final user. But metric users ~~must~~ should also keep in mind that IRFs and emission metrics are extremely simple models of a complex system, and that sometimes it may be beneficial to use more complex models that better capture multiple and interacting feedback processes.

Appendix A: Protocol to simulate the carbon dioxide IRF

The protocol is exactly that of Joos et al. (2013), reproduced here for clarity.

A first simulation is made in a concentration-driven fashion, with prescribed atmospheric CO_2 and prescribed non-CO_2 radiative forcings that follow the estimates by Meinshausen et al. (2011) for the historical period up to 2005, and then those for the RCP4.5 between 2005 and 2010. These prescribed forcings are then maintained constant to their value of the year 2010 during another 1000 years of simulation. In the case of OSCAR, as recommended by Joos et al. (2013), land-use and land-cover change is also prescribed following the historical and then RCP4.5 data of Hurtt et al. (2011), and then stopped after 2010. The outputs from this first simulation are used to deduce the anthropogenic emissions of CO_2 that are compatible with the prescribed atmospheric CO_2, through simple mass balance of the carbon element (see e.g. Gasser et al., 2015).

A second simulation is made in an emission-driven fashion with the same prescribed non-CO_2 radiative forcings and with the compatible CO_2 emissions deduced from the first simulation, with the only purpose of checking that the atmospheric CO_2 concentration simulated is the same as the one prescribed in the first simulation.

A third and final simulation is made, similar to the second one except that in 2015, on top of the compatible emissions, a pulse of 100 giga-tonnes of carbon is added to the atmosphere. The carbon dioxide IRF seen in figure 1a is simply deduced as the difference between the atmospheric CO_2 simulated in the third and second experiments, normalized by the size of the pulse, and setting the time origin ($t = 0$) as the year of the pulse (i.e. 2015).

Specific to our study, we also make simulations following this protocol but with the climate-carbon feedbacks "turned off". This is achieved by prescribing the climate simulated by the second experiment to the third one.

Appendix B: Protocol to fit the climate-carbon feedback IRF

First, we fit a first-guess value for τ_1, using the differentiated response (figure 2c) only over the first 5 (annual) time-steps, and assuming that f'' can be approximated by a one-exponential function over this short period of time. Second, we fit a first-guess value for γ and α_1, using the yearly response (figure 2b) also over the first 5 time-steps, and assuming that f' can also be approximated by a one-exponential function whose time constant τ_1 is the one estimated during the first step. Third, we fit a first-guess value for the remaining parameters, i.e. τ_2, τ_3 and α_2, using the cumulative response (figure 2a) over the whole simulation, and using the parameters determined in the first and second steps for f. Fourth, we fit the final values of the six parameters, using the yearly response (figure 2b) but this time over the whole simulation, and using the six parameters previously estimated as first guesses of the parameters of f'. All fits follow a least squares method, with the additional constraints that: $0 < \alpha_i < 1$ and $\alpha_3 = 1 - \alpha_1 - \alpha_2$. Only the actual outputs of OSCAR are used to fit, i.e. the 'extended' part shown in figure 2 is not used.

Appendix C: Analytical expressions of the IRFs used in this study

C.1 Carbon dioxide response

Joos et al. (2013):

$$r_Q^{CO_2}(t) = 0.2173 + 0.2763 \exp\left(-\frac{t}{4.304}\right) + 0.2824 \exp\left(-\frac{t}{36.54}\right) + 0.2240 \exp\left(-\frac{t}{394.4}\right)$$

OSCAR v2.2, with climate-carbon feedback (average of ensemble):

$$r_Q^{CO_2}(t) = 0.2366 + 0.2673 \exp\left(-\frac{t}{4.272}\right) + 0.2712 \exp\left(-\frac{t}{33.10}\right) + 0.2249 \exp\left(-\frac{t}{302.4}\right)$$

OSCAR v2.2, without climate-carbon feedback (average of ensemble):

$$r_Q^{CO_2}(t) = 0.2033 + 0.3016 \exp\left(-\frac{t}{4.736}\right) + 0.2836 \exp\left(-\frac{t}{34.09}\right) + 0.2115 \exp\left(-\frac{t}{288.4}\right)$$

C.2 Climate response

Boucher and Reddy (2008):

$$\lambda\, r_T(t) = 1.06 \left(\frac{0.595}{8.4} \exp\left(-\frac{t}{8.4}\right) + \frac{0.405}{409.5} \exp\left(-\frac{t}{409.5}\right)\right)$$

Geoffroy et al. (2013):

$$\lambda\, r_T(t) = 0.885 \left(\frac{0.587}{4.1} \exp\left(-\frac{t}{4.1}\right) + \frac{0.413}{249} \exp\left(-\frac{t}{249}\right)\right)$$

OSCAR v2.2 (average of ensemble):

$$\lambda\, r_T(t) = 0.852 \left(\frac{0.572}{3.50} \exp\left(-\frac{t}{3.50}\right) + \frac{0.428}{166} \exp\left(-\frac{t}{166}\right)\right)$$

C.3 Climate-carbon feedback response

Collins et al. (2013):

$$\gamma\, r_F(t) = 1.0\, \delta(t)$$

OSCAR v2.2 (average of ensemble):

$$\gamma\, r_F(t) = 3.015\, \delta(t) - 3.015 \left(\frac{0.6368}{2.376} \exp\left(-\frac{t}{2.376}\right) + \frac{0.3322}{30.14} \exp\left(-\frac{t}{30.14}\right) + \frac{0.0310}{490.1} \exp\left(-\frac{t}{490.1}\right)\right)$$

Appendix D: Other parameters used in this study

D.1 Radiative efficiencies

The following values include the effect of any overlap between the absorption bands of CO_2, CH_4 and N_2O (Myhre et al., 1998; Etminan et al., 2016). They also include some indirect effects: increase in stratospheric water vapor and tropospheric ozone for CH_4, and enhancement of the methane atmospheric sinks for N_2O (Myhre et al., 2013; sections 8.SM.11.3.2 and 8.SM.11.3.3). Note that these indirect effects are not affected by the update of the direct radiative efficiency by Etminan et al. (2016). The background concentration is kept to that of 2011, as in IPCC AR5 (Myhre et al., 2013; section 8.SM.11.1).

Myhre et al. (2013):

$$\varphi^{CO2} = 1.76 \times 10^{-15}\ \mathrm{W\ m^{-2}\ kgCO2^{-1}}$$
$$\varphi^{CH4} = 2.11 \times 10^{-13}\ \mathrm{W\ m^{-2}\ kgCH4^{-1}}$$
$$\varphi^{N2O} = 3.57 \times 10^{-13}\ \mathrm{W\ m^{-2}\ kgN2O^{-1}}$$
$$\varphi^{SF6} = 2.20 \times 10^{-11}\ \mathrm{W\ m^{-2}\ kgSF6^{-1}}$$

Etminan et al. (2016):

$$\varphi^{CO2} = 1.79 \times 10^{-15}\ \mathrm{W\ m^{-2}\ kgCO2^{-1}}$$
$$\varphi^{CH4} = 2.39 \times 10^{-13}\ \mathrm{W\ m^{-2}\ kgCH4^{-1}}$$
$$\varphi^{N2O} = 3.46 \times 10^{-13}\ \mathrm{W\ m^{-2}\ kgN2O^{-1}}$$

Fuglestvedt et al. (2010):

$$\varphi^{SO2} = -3.2 \times 10^{-10}\ \mathrm{W\ m^{-2}\ kgSO2^{-1}}$$
$$\varphi^{BC} = 1.96 \times 10^{-9}\ \mathrm{W\ m^{-2}\ kg^{-1}}$$

D.2 Perturbation lifetimes

These are used to define the non-CO_2 atmospheric concentration IRFs: $r_Q^x(t) = \exp(-t/\tau^x)$.

Myhre et al. (2013):

$$\tau^{CH4} = 12.4\ \mathrm{yr}$$
$$\tau^{N2O} = 121\ \mathrm{yr}$$
$$\tau^{SF6} = 3200\ \mathrm{yr}$$

Fuglestvedt et al. (2010):

$$\tau^{SO2} = 0.011\ \mathrm{yr}$$
$$\tau^{BC} = 0.020\ \mathrm{yr}$$

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	γ (GtC yr^{-1} K^{-1})	τ_{eff} (yr)	α_1 (–)	α_2 (–)	α_3 (–)	τ_1 (yr)	τ_2 (yr)	τ_3 (yr)
mean	**3.015**	**28.69**	**0.6368**	**0.3322**	**0.0310**	**2.376**	**30.14**	**490.1**
upper	4.264	32.06	0.5916	0.3679	0.0405	3.333	37.12	404.3
lower	1.964	27.28	0.8139	0.1761	0.0100 [§]	1.181	38.02	1962. [§]

Table 1: Values of the parameters of the IRF for the climate-carbon feedback (i.e. $\gamma\ r_F$). The parameters calibrated on OSCAR for the mean response are given, as well as those for the upper response (i.e. mean +1 standard deviation) and the lower response (i.e. mean –1 standard deviation). The latter two responses correspond to the two envelope curves in figure 2. The effective time-scale τ_{eff} is calculated as $\boldsymbol{\tau_{\mathrm{eff}} = \sum_i \alpha_i \tau_i}$. [§] The low weight and high value of the slow time-scale indicate that the lower response could be fitted by a two-exponential functional form.

(part 1)	CH4†						N2O						SF6		
	GWP			GTP			GWP			GTP			GWP		
Time horizon (in years)	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**
AR5 (default) §	84	48	28	67	14	4	*263*	275	*264*	*276*	*281*	234	17500	20500	*23600*
AR5 + Collins §	*85*	52	34	70	20	11	*267*	290	*297*	*283*	311	297	17800	21600	26200
AR5 + OSCAR	86	52	31	70	18	5	269	289	283	285	304	258	17900	21500	25200
AR5 + OSCAR + climate IRF update	86	51	31	60	14	7	270	288	281	294	300	253	18000	21500	25000
AR5 + OSCAR + IRF & REs updates	96	57	34	67	16	7	256	274	267	279	285	240	17600	21100	24500
All OSCAR	96	57	34	66	18	9	255	273	267	279	283	241	17600	21000	24500
All OSCAR (no CC-fdbk)	96	57	34	65	16	8	257	275	269	282	286	244	17700	21200	24800

(part 2)	SF6			BC‡						SO2‡					
	GTP			GWP			GTP			GWP			GTP		
Time horizon (in years)	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**
AR5 (default) §	*19000*	*23900*	*28300*	1560	736	426	451	71	58	-140	-66	-38	-40	-6	-5
AR5 + Collins §	19400	26000	33700	1620	818	519	528	172	165	-145	-73	-47	-47	-15	-15
AR5 + OSCAR	19500	25500	30800	1630	794	465	525	110	69	-146	-71	-42	-47	-10	-6
AR5 + OSCAR + climate IRF update	20500	25900	30400	1630	787	460	210	116	90	-146	-71	-41	-19	-10	-8
AR5 + OSCAR + IRF & REs updates	20100	25400	29800	1600	772	451	206	114	88	-143	-69	-41	-18	-10	-8
All OSCAR	20100	25200	29400	1590	769	450	213	147	105	-143	-69	-40	-19	-13	-9
All OSCAR (no CC-fdbk)	20400	25600	30200	1570	760	448	165	128	101	-141	-68	-40	-15	-11	-9

Table 2: GWPs and GTPs at a time horizon of 20, 50 and 100 years, in the case of CH_4, N_2O, SF_6, BC and SO_2. The first row (“AR5 default”) shows the base metrics as calculated by the IPCC AR5 (Myhre et al., 2013; table 8.A.1). The second row (“AR5 + Collins”) shows the metrics proposed in the IPCC AR5 as a first attempt to account for the climate-carbon feedback (their table 8.7), in which case the climate-carbon feedback IRF ($\boldsymbol{\gamma}\, \boldsymbol{r_F}$) is the one of Collins et al. (2013). The third row (“AR5 + OSCAR”) shows the metrics when using our climate-carbon feedback IRF. The fourth row (“AR5 ~~updated~~ + OSCAR + climate IRF update”) shows the same metrics as the third row, except that the climate IRF ($\boldsymbol{\lambda}\, \boldsymbol{r_T}$) is updated ~~from that of Boucher and Reddy (2008)~~ to one based on an ensemble of CMIP5 models~~that of~~ (Geoffroy et al., ~~(~~2013). ~~This fourth row, in bold font, shows our recommended values.~~ The fifth row (“AR5 + OSCAR + IRF & REs updates”) is the same as the fourth one, except that we also update the radiative efficiencies (REs) of CO_2, CH_4 and N_2O (Etminan et al., 2016). The sixth~~fifth~~ row (“all OSCAR”) shows the metrics obtained when all IRFs used are based on OSCAR and the radiative efficiencies are also updated, with inclusion of the climate-carbon feedback. The seventh~~sixth~~ and last row (“all OSCAR no CC-fdbk”) shows the same as the sixth~~fifth~~ row, but this time without including the climate-carbon feedback: neither for CO_2 nor for non-CO_2 species. § Because we use a numerical resolution method while the IPCC used an analytical one, some values in these rows may differ from the IPCC values by 1 because of the rounding (by 100 in the case of SF_6); these differing values are shown in italic font. † This does not account for the oxidation of CH_4 into CO_2 (see e.g. Boucher et al., 2009). ‡ Metrics for BC and SO_2 are not directly provided by the IPCC, rather we use here the IPCC method with lifetimes and radiative efficiencies from Fuglestvedt et al. (2010).

	CH_4[†]						N_2O					
	GWP			**GTP**			**GWP**			**GTP**		
Time horizon (in years)	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**	**20**	**50**	**100**
All OSCAR	17%	20%	22%	23%	32%	32%	10%	13%	15%	14%	17%	19%
All OSCAR (no CC-fdbk)	19%	22%	24%	24%	34%	34%	13%	16%	18%	15%	19%	21%

Table 3: Uncertainty of GWP and GTP at a time horizon of 20, 50 and 100 years, in the case of CH_4 and N_2O. The relative uncertainties for ±1 standard deviation are shown. They are calculated on the basis of: i) the Monte Carlo ensembles of simulations made with OSCAR, shown in figures 1 and 2 and described in main text, and ii) the uncertainty ranges given by Myhre et al. (2013; table 8.SM.12) for radiative efficiencies and perturbation~~s~~ lifetimes. [†] This does not account for the oxidation of CH_4 into CO_2.

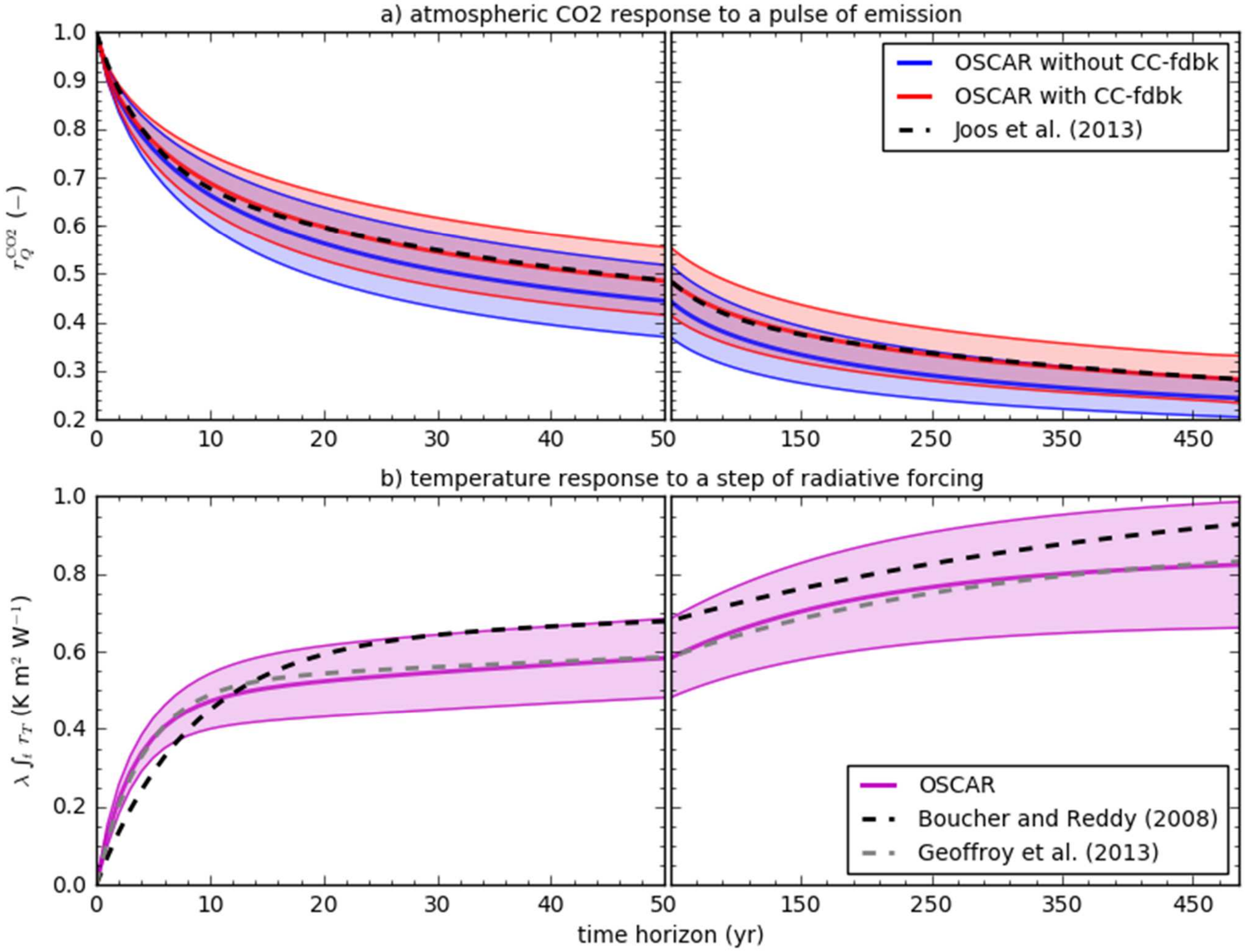

Figure 1: Impulse response functions estimated with OSCAR. (a) Response of the atmospheric CO_2 to a pulse of emission, in the case where the climate-carbon feedbacks ("CC-fdbk") are turned off (in blue), and in the normal case (in red). The responses by OSCAR are compared to that of Joos et al. (2013) used by the IPCC AR5 (dashed black). (b) Response of the global mean surface temperature to a step of radiative forcing. The response by OSCAR is compared to that of Boucher and Reddy (2008) used by the IPCC AR5 (dashed black) and to that of Geoffroy et al. (2013) that is based on CMIP5 models (dashed grey). The actual climate IRF (i.e. the response to a pulse) is obtained by taking the derivative of the curve shown in (b). Plain and thick lines show the mean response of OSCAR, while shaded and coloured areas show the ±1 standard deviation around the mean.

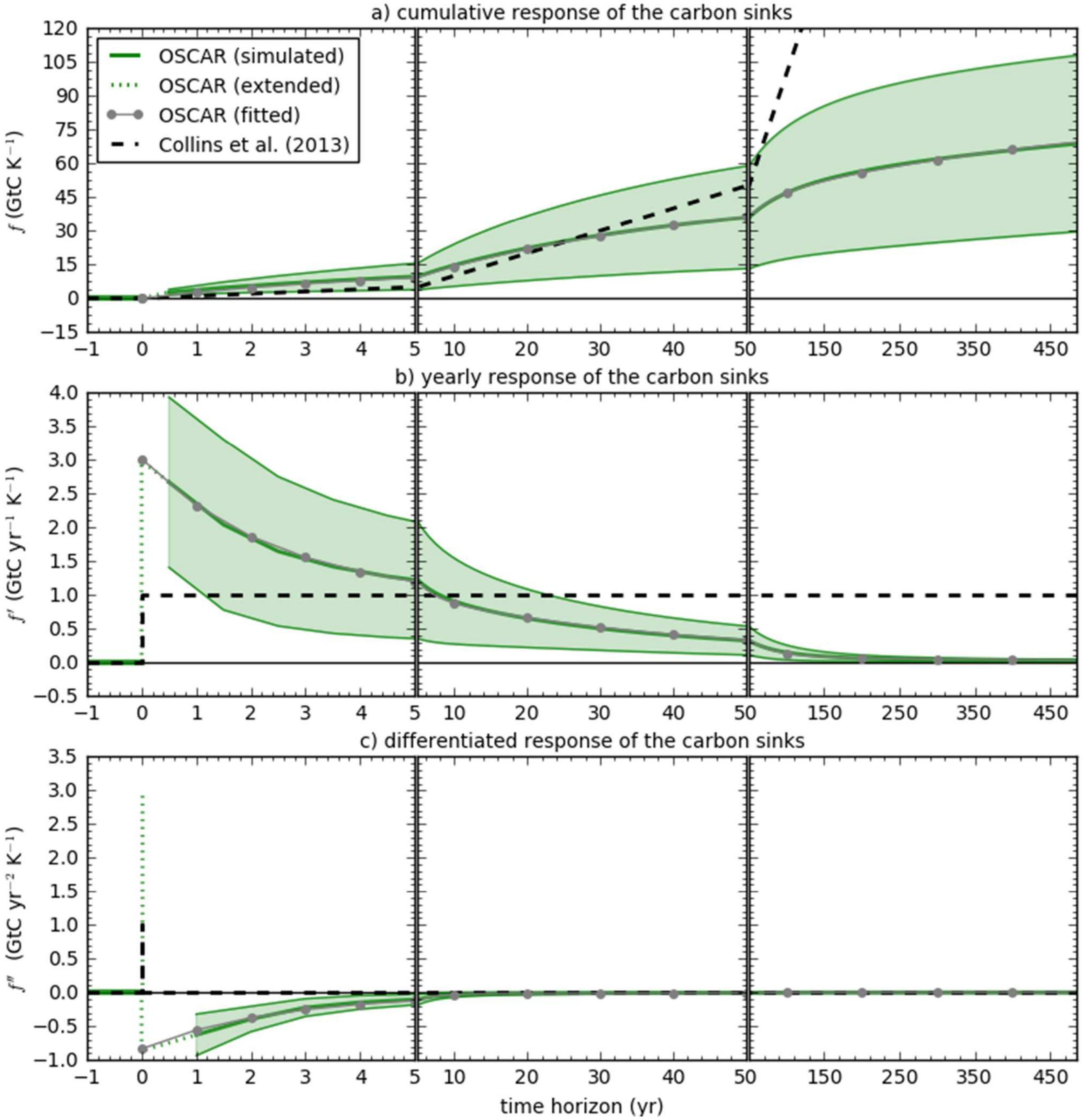

Figure 2: IRF for the carbon sinks response estimated with OSCAR. The response of the carbon sinks to a step of climate change is shown in three different ways: (a) as the cumulative amount of CO_2 outgassed by the sinks; (b) as the annual amount of CO_2 outgassed by the sinks; (c) as the derivative of the annual response to the step of climate change, which is equivalent to the annual response to a pulse of climate change. As in figure 1, the plain and thick (green) lines show the mean response from the Monte Carlo ensemble, while the shaded areas show the ±1 standard deviation. The dotted (green) lines illustrate our arbitrary extension of the response simulated by OSCAR when around t = 0 (see section 3.3). The grey lines with round markers are the results of our fit. For comparison, we also show the response assumed by Collins et al. (2013) as dashed black lines.

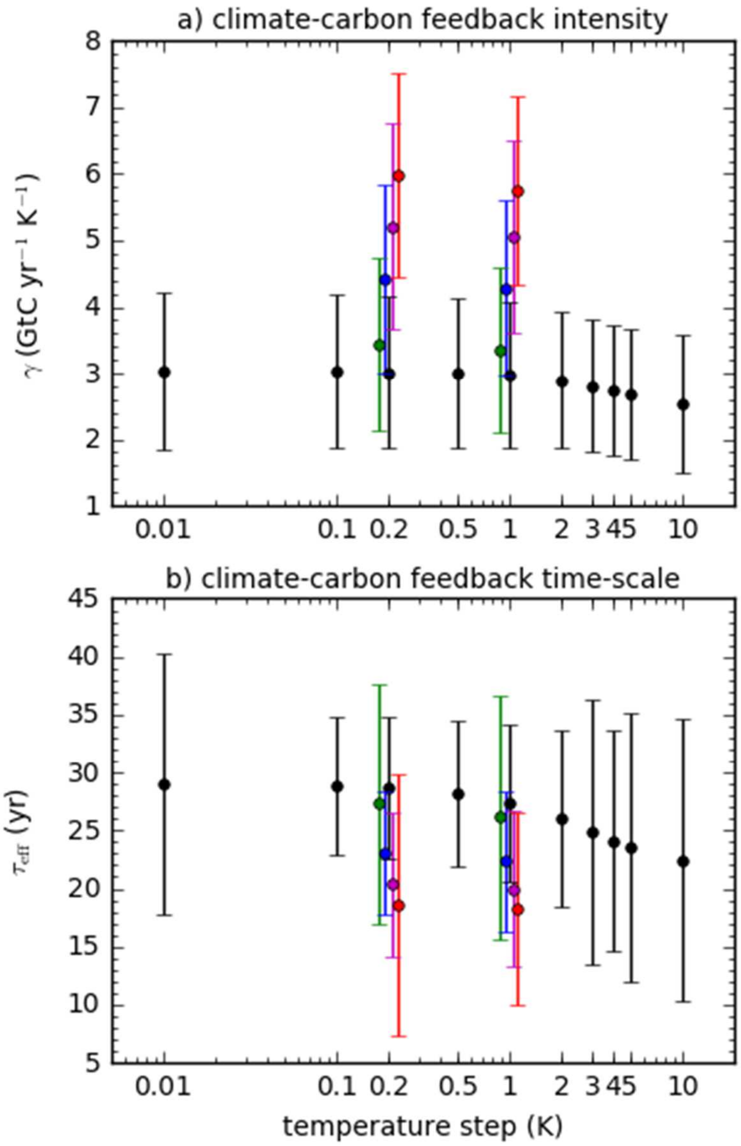

Figure 3: Influence of step size and background on the climate-carbon feedback IRF: (a) on the climate-carbon feedback intensity γ; and (b) on the climate-carbon feedback effective time-scale τ_{eff} (calculated as $\boldsymbol{\tau_{eff}} = \sum_i \boldsymbol{\alpha_i \tau_i}$). The effect of the amplitude of the step of climate change (in black) and of the atmospheric CO_2 and climate background following the four RCPs (in colour; green for RCP2.6, blue for RCP4.5, magenta for RCP6.0 and red for RCP8.5) are shown. The uncertainty ranges shown is the ±1 standard deviation range, corresponding to the "upper" and "lower" responses in table 1.

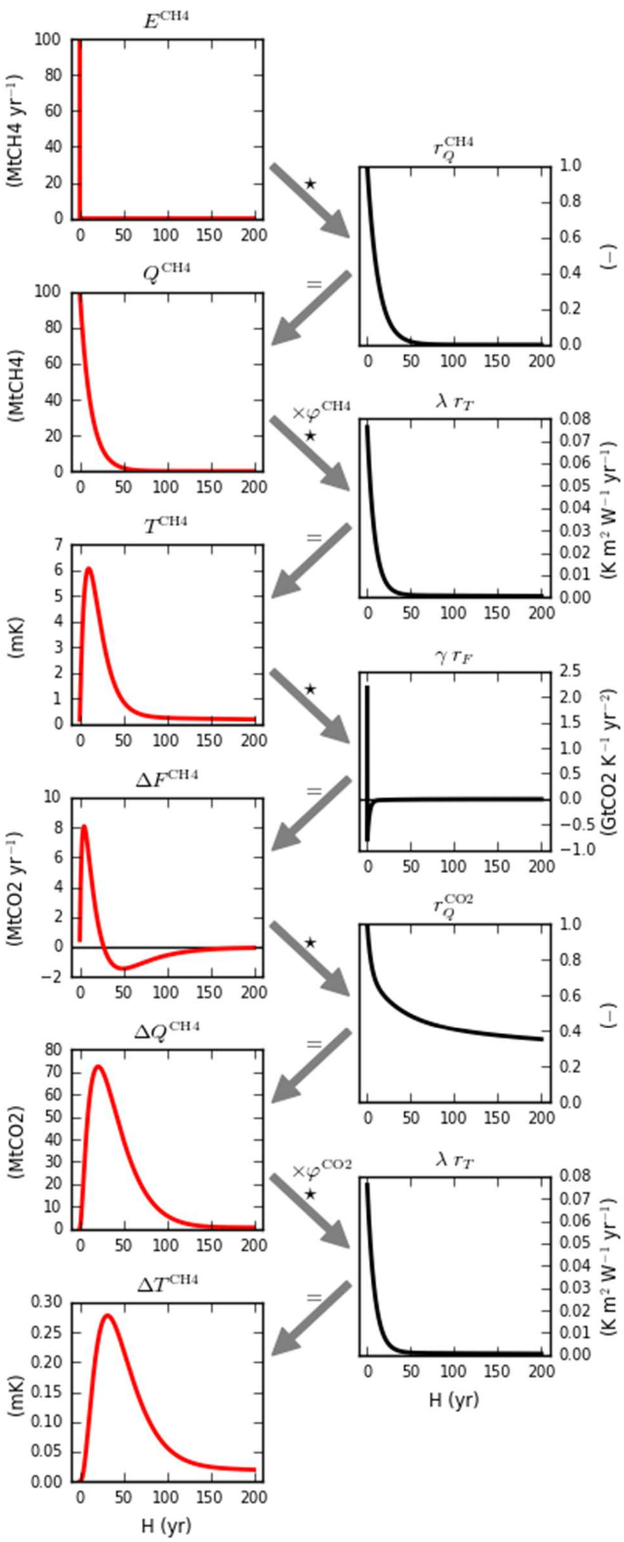

Figure 4: Example of the step-by-step convolutions leading to the ΔAGTP of CH_4. The figure is read panel by panel, following the arrows and starting in the upper-left corner. The left-hand side panels show the actual physical variables, whereas the right-hand side panels show the IRFs used for the convolutions. We start with a pulse of CH_4 emitted at t = 0, of an arbitrary size of 100 MtCH4. This pulse ($\boldsymbol{E^{CH4}}$) is then convoluted (symbol ⋆) with the atmospheric CH_4 IRF ($\boldsymbol{r_Q^{CH4}}$) to give the induced change in atmospheric CH_4 ($\boldsymbol{Q^{CH4}}$). This atmospheric CH_4 is then multiplied by the CH_4 radiative efficiency ($\boldsymbol{\varphi^{CH4}}$~~; units: W m^{-2} GtCH4^{-1}~~) and convoluted with the climate IRF ($\boldsymbol{\lambda\, r_T}$) to give the induced change in global surface temperature ($\boldsymbol{T^{CH4}}$). One would stop here to deduce the AGTP by normalizing the obtained temperature change by the size of the initial pulse. In our case, the temperature change is then convoluted with the climate-carbon feedback IRF ($\boldsymbol{\gamma\, r_F}$) to give the induced flux of CO_2 released by the sinks ($\boldsymbol{\Delta F^{CH4}}$). This flux of CO_2 is then convoluted with the carbon dioxide IRF ($\boldsymbol{r_Q^{CO2}}$) to give the induced change in atmospheric CO_2 ($\boldsymbol{\Delta Q^{CH4}}$). And finally, this atmospheric CO_2 is then multiplied by the CO_2 radiative efficiency ($\boldsymbol{\varphi^{CO2}}$~~; units: W m^{-2} GtCO2^{-1}~~) and convoluted with the climate IRF ($\boldsymbol{\lambda\, r_T}$) to give the induced change in global surface temperature ($\boldsymbol{\Delta T^{CH4}}$). The ΔAGTP is deduced by normalizing the obtained temperature change by the size of the initial pulse. An analogous example can be produced for ΔAGWP, in which case one has to replace the last convolution by a convolution with the Heaviside step function (Θ).

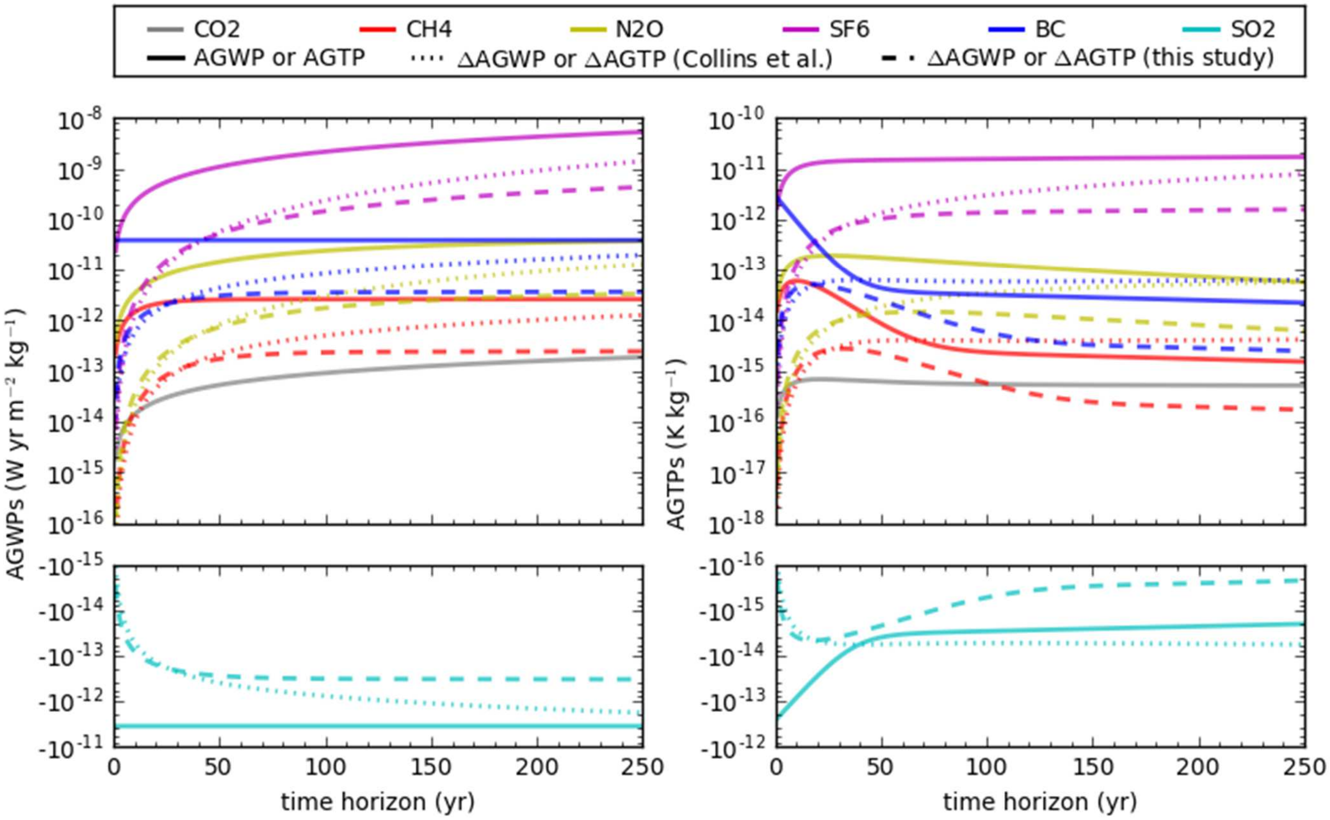

Figure 5: Absolute metrics, in the case of CO_2, CH_4, N_2O, SF_6, BC and SO_2. AGWPs (left-hand side) and AGTPs (right-hand side) obtained using the IPCC AR5 method are shown in solid lines. ΔAGWPs and ΔAGTPs obtained using the climate-carbon feedback IRF by Collins et al. (2013) are shown in dotted lines, and those obtained using ours are in dashed lines. Colours refer to the different species taken here as examples.

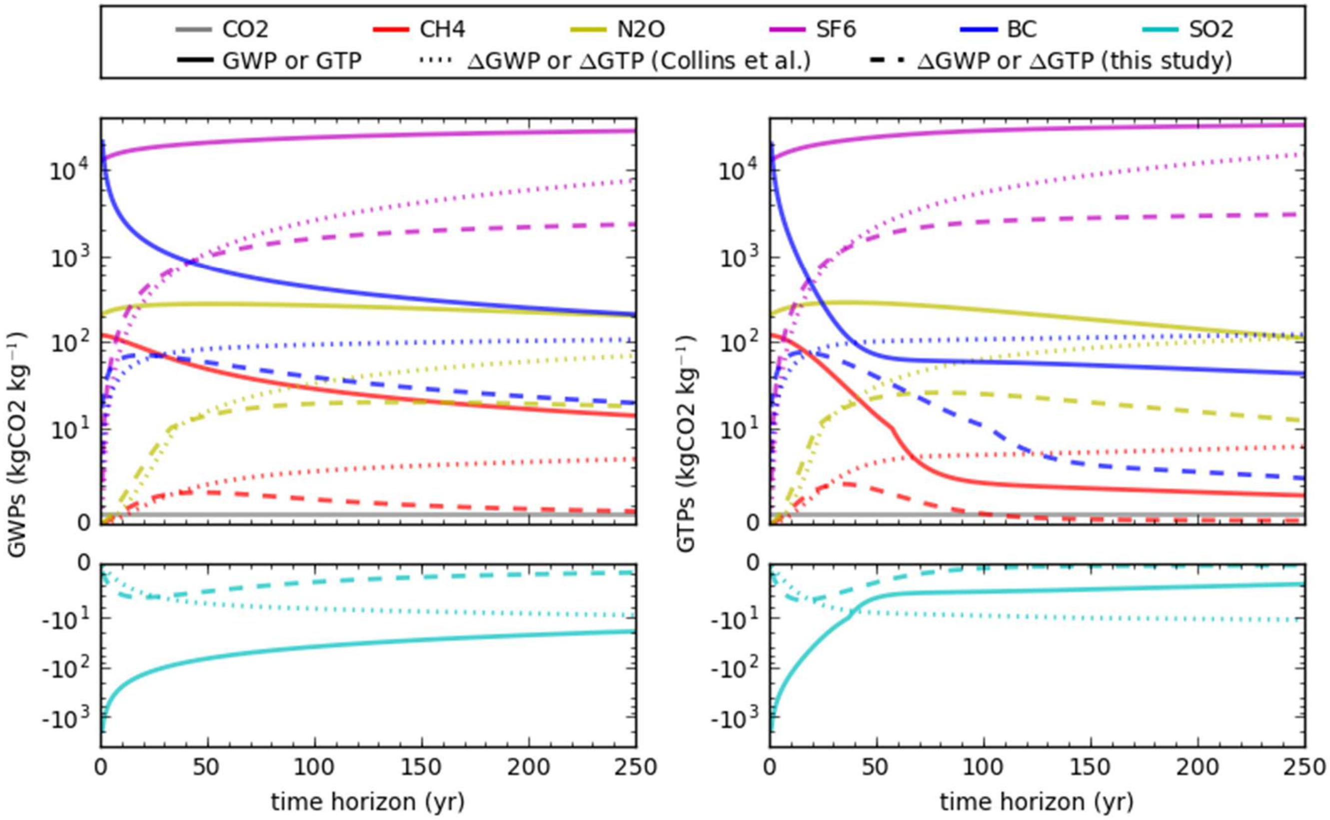

Figure 6: ~~Normalized~~ Relative metrics, in the case of CH_4, N_2O, SF_6, BC and SO_2. GWPs (left-hand side) and GTPs (right-hand side) obtained using the IPCC AR5 method are shown in solid lines. ΔGWPs and ΔGTPs obtained using the climate-carbon feedback IRF by Collins et al. (2013) are shown in dotted lines, and those obtained using ours are in dashed lines. Colours refer to the different species taken here as examples. Note that the scale of the *y*-axis is linear between 1 and ±10 and logarithmic afterwards.