

Interactive comment on “A synthesis of climate-based emission metrics with applications” by B. Aamaas et al.

Anonymous Referee #2

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General comments

I agree with the comments made by Daniel that the paper presents a valuable overview and summary of ways to calculate a broad range of physically-based metrics via analytical expressions. As such, it presents little novel science – the main novelty lies in the few, but valuable, applications that the paper demonstrates. This means its merit should not be judged merely by its the novelty of its scientific findings. I strongly support the publication of this article for the summary and consistent analytical expressions it offers, and the demonstrated applications, even though it offers little new scientific insights.

The focus on analytical expressions is in my view not a shortcoming but simply reflects the chosen focus of the paper. I do agree though with Daniel's comments that the

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title of the paper should better reflect this focus, and in several places a comment is warranted that the choice does not lie between either simple analytical expressions or GCMs to evaluate the climate impact of emissions of different gases. Simple or reduced complexity climate models have been used to calculate metrics and can do this fairly efficiently, and the paper should (fairly prominently, I suggest) recognise this, before then proceeding to lay out a coherent framework for analytical expressions. I think the focus on analytical expressions is a choice that does perhaps not need to be justified as Daniel argues, but it needs to be made clear that this is a choice, and other options exist to derive results of the same breadth and complexity as the authors present.

Given the focus on analytical expressions, I would strongly urge the authors to make use of this computational efficiency and strive to be more comprehensive in the gases they cover. The value of this article does not lie in its novel scientific insights but in being able to act as a reference document for the scientific (and applied science) community. To service this potential use and value of the article, I would like to also have results for SF₆ and some selected HFCs and PFCs included. Not in all figures and applications, but mainly in the presentation of results that are currently shown in Figure 9. That would mean that the article could act as the go-to place for anybody wishing to obtain consistent metric values for a wide range of gases. Right now, given that the results are shown only in a Figure but not in a table, and that some important GHGs are not included, it fails to do that. Including other gases, including some long-lived ones, will also help correct some incorrect generalisations that the authors draw from their results (see specific comments below).

Finally as general comment, and in keeping with this article being a review and hopefully one-stop shop for information on analytically-derived metrics, I would have liked a place somewhere in the manuscript (at the beginning and/or end of section 2) that summarises the issues canvassed in section 2, and more importantly, an assessment of the relative uncertainty introduced by the different choices and uncertainty in the

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parameters used. E.g. the authors devote substantial space to discussing the difference between small and marginal perturbations of CO₂ (2.2.1), but it then becomes clear that the differences for pulses up to at least 10ppm pulse are almost irrelevant compared to other uncertainties and choices. So a summary of the issues explored that gives a clear ranking of which choices and uncertainties actually matter, and which ones are intellectually interesting but of less relevance in practice, would be important to avoid skewed messages in this article.

The sum of those general comments in my view suggests that a major revision is required, though one could equally regard such revisions as 'extensive but minor' changes since I'm perfectly satisfied with the structure and scientific approach of the article.

Specific comments

Most of my specific comments, although there are plenty of them, should be fairly easy to deal with. There is one major specific comment though, which is whether in the applications listed in section 4, the changing background concentrations for sustained emissions has been taken into account, and if not, to provide an estimate of the systematic error introduced into the results.

P872/L19: spell out SLCF in the abstract since readers may not be familiar with the acronym

P873/L5-6: the sentence starting "A limitation of using RF.." is not clear to me and seems incorrect since RF has a simple relationship with atmospheric concentration. Perhaps you meant "transient response in the climate system" rather than in the atmospheric concentration?

P873/L11-12: you could extend the quote and highlight that it was meant as illustrative example to demonstrate the difficulties of using a single metric!

P873/L15: not clear what is meant by "related to its interpretation". The interpretation

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of the GWP seems about the only thing that is straightforward and not dependent on value judgements!?

P873/L15: might be useful to state here that the 100-year GWP is used in UNFCCC reporting and accounting under the Kyoto Protocol, and is also used in most LCA analyses as well as industry reporting and carbon accounting tools.

P873/L16: I'd challenge the words "in use". It's being discussed in the literature and receiving attention, but there is very, very little 'use' of the GWP in any applied GHG reporting and accounting sense. If the authors disagree with this, it would be useful to have some of those actual applications (in a practical, applied policy or industry context) referenced.

P874/L10-12: add "to report emission trends of countries under the UNFCCC"

P874/L15: insert "scientific" before "need", since clearly from a policy perspective, it would be rather helpful if there were just one single metric as it would avoid a lot of potential confusion.

P875/L17-19: Not convinced by this statement. One could use SF₆ with its simple exponential decay rate, or even N₂O, and still end up with very different metric values for CH₄ depending on the value judgement of TH.

P875/L20: a significant omission in this article is the recognition that not only one could use different metrics in different applications, but also that there is no reason to have only one reference gas. Two-basket approaches have been discussed in the literature and should be briefly flagged here as one way of dealing with widely differing lifetimes. E.g. Smith, S.M., J.A. Lowe, N.H.A. Bowerman, L.K. Gohar, C. Huntingford, M.R. Allen, 2012: Equivalence of greenhouse-gas emissions for peak temperature limits. *Nature Clim. Change*. 2(7), 535-538.

P876/L13-22: Here would be an appropriate place to state very clearly that a variety of approaches exist and have been used in the literature to calculate metrics, ranging from

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analytical expressions based on parameterisations to simple and reduced-complexity models, to models of intermediate complexity. Give a very brief overview of the advantages and disadvantages of those approaches, and then state that in this article you focus on analytical expressions to be able to provide a single consistent numerical framework that allows the calculation of any metric as long as some key parameters are known.

P877/L20: This reads as if there was somehow a definition or otherwise requirement to use the Bern CC model to calculate IRFs for emission metrics. Make clear that in practice this has been a common choice, but note that others have used other models or even a range of CC models (e.g. Reisinger et al 2010, cited elsewhere in the article).

P878/L11: not clear from the discussion that follows that, or rather which aspects of, experimental setup have a large influence on the IRF. Perhaps replace by "some possible choices in the experimental setup"

P878/L27: The difference obviously depends on the scenario chosen. Can you indicate over which scenario range this holds true? E.g. if one assumes RCP8.5 scenario and extended to 2300, is it still only 0.1-0.2 over 500 years?

P878/L28: A very large pulse will also change the radiative efficiency of that pulse, which should be mentioned here as well

P880/L7-8: Here is another point where the text should make clear that using an IRF to derive temperature responses is a choice, not something in the definition of emission metrics. One could equally use climate models to derive the temperature response (even in combination with a parameterised IRF for concentrations).

P884/L5-15: Given the very small difference between a 1ppm and a marginal perturbation, I question the relevance of this paragraph. It might be more useful to derive a plausible threshold for when a pulse size is no longer appropriate and consistent with the definition of the metrics under discussion. It seems that a pulse up to 10ppm is

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defensible and introduces an effort in the order of only 1% - but what about pulses of 100ppm or even more? That would be useful guidance for future experiments, such as that in Joos et al (2012).

P884/L16-27: With Daniel, I struggle with the relevance of this. The main argument against using pre-industrial IRFs and RE surely must be that the values are counterfactual to their actual climate impact. This discussion needs to be more nuanced and extended, or perhaps shortened and focus purely on the policy trade-off between a constant metric value, versus a metric value that changes over time but in a predictable manner, versus a metric value that changes over time but not in a predictable manner if it depends on future emissions and concentrations. It would also help to have an estimate of the amount of change over time, and compare this with other uncertainties and importance of other choices. This goes back to my general comment that Section 2 needs a summary that helps rank alternative choices with regard to the actual difference they make for the metric values.

P885/L20: Would be useful to state here the range of uncertainties for REs, to help compare and contrast those with the impact of other uncertainties and experimental set-up choices.

P886/L8-17: It would be useful to emphasise here the order of magnitude greater uncertainties in calculating these simple parameters, and the reliance on much more complex models to derive those parameters. Also, it needs to be made clear up-front that deriving a single global exponential decay term for a species that is not globally well-mixed is a much more gross simplification than using similar exponential decay terms of LLGHGs, which puts a significant question mark behind the physical interpretation and uncertainty of parameters such as tau. This is another example why a summary comparison of the scale of uncertainties from the different issues considered in section 2 is important to avoid skewed messages.

P888/L4: It would be helpful to quantify the important of those interactions with other

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gases on overall radiative forcing. Also you might wish to point out that given the complexity of interactions, there is a largely arbitrary cut-off point about which ones are included in the calculation of feedbacks and their contribution to the overall RF and temperature response. This even applies to the RF from CH₄ emissions, since the interaction with aerosols can significantly add to the net RF caused by CH₄ emissions (Shindell, D.T., G. Faluvegi, D.M. Koch, G.A. Schmidt, N. Unger, S.E. Bauer, 2009: Improved Attribution of Climate Forcing to Emissions. *Science*. 326(5953), 716-718).

P888/L8: state whether the impact depends on the location of emissions.

P888/L17: insert "is" after "contrails"

P895/L5: include reference to Azar, C., D.J.A. Johansson, 2012: On the relationship between metrics to compare greenhouse gases – the case of IGTP, GWP and SGTP. *Earth Syst. Dynam. Discuss.* 3(1), 113-141.

P895/L9: here also you should cite Azar and Johansson 2012 (cited above)

P895/L17: one can argue about the "arguably better": it is a useful alternative interpretation, but there is no reason given why this should be universally better.

P897/L11-16: As elsewhere above, I would like to challenge the word "use" here: most other metrics are not really used; they are presented and discussed in the scientific literature, but they are hardly ever used in an applied sense (e.g. emissions tracking, reporting, accounting by policy or industry; any use there seems to occur only in a context that is still primarily a scientific one).

P898/L8: not sure I understand the expression "in a normalized gas" – what is that?

P898/L20: not just for SLCFs but also for short-lived LLGHGs, most notably CH₄, but also a range of HFCs. That should be made clear.

P898/L22-23: I don't understand what the authors mean by "characteristic of moving towards a target, not necessarily a characteristic of the economic model". Can you reformulate? It clearly is a property of economic models that aim to achieve cost-effective

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abatement.

P899/L17-25: the authors seem to have an inconsistent use of SLCFs: the discussion here is highly relevant and appropriate to CH₄ and short-lived HFCs, not just SLCFs. Or are those meant to be included here? Please spell this out.

P899/L21-22: sentence repeated from P898/L22-23. Delete here I suggest.

P899/L24-25: based on Peters et al (2011a), the iGTP isn't really a new metric but in fact was there before the GWP, even though not given this name. Also cite Azar and Johansson here.

P99/L1-11: Here would be another place where approaches using multiple baskets and using multiple reference gases would be highly appropriate.

P901/L10-11: I don't think it's a value judgement but rather it depends on context. If you only have a single emissions pulse to contemplate in a certain context then it's not a value judgement whether you use a pulse or sustained emissions metric.

P902/L13-20: An important point that seems to be missing in this discussion, and in the analytical expressions derived, is the fact that a sustained emission can change the background concentration and hence the RE and possibly IRF of subsequent pulse emissions. The equations derived seem to be correct strictly only for sustained emissions that are small enough that they don't change the background concentration – which they clearly do, however!

P903/L11-14: This seems to be somewhat irrelevant numerical detail P904/L5: Here again, recognition needs to be made of changing background concentrations. MAJOR: This is an important point: in the calculations that the authors show in the next section, did they assume a constant IRF and RE for CO₂ and other gases (which would make the results strictly incorrect, and would require them to at least give an estimate of the error introduced by this approach; note that the error is not random but systematic for each gas, and hence it is rather significant to make this issue clear and if necessary

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quantify it).

P904/L8: revise wording: this reads as if climate metrics are “by definition” based on simple parameterisations. But that is not the case, as one could equally (and perhaps, more justifiably) use simple or even complex climate models to calculate metrics. Re-word: “Since in the framework we present in this article, all climate metrics are...”

P904/L27: perhaps cross-ref back here to section 2.2.3

P905/L1: for readability and since the use of this acronym occurred a while ago, spell out AIE here again

P905/L18-P906/9 and Figure 9: Following from my general comment, I would strongly encourage the authors to make significant changes to the results presented. This overview is potentially extremely useful, but as presented at present it is impossible to obtain concrete numbers, and the list of species covered is highly incomplete in that it misses key LLGHGs. I would encourage the authors to substitute, or add to, Figure 9 a table that lists metric values for the standard time horizons (say, 20, 50, 100 and 500 years), and give values also for SF6 and some of the most important HFCs and PFCs. Providing this information in a numerical and comprehensive form would make this article much more useful. I imagine that this should not be an impossible amount of work given that the key parameters for those gases are readily available in the sources already used for this study. I’m not sure if there are page limitations for ESD, but I would argue that there would be a lot of value in providing rather extensive tabulated results.

P905/L19-22: This strikes me as a tautology, since by definition an integration of something remembers what happened before the end of the integration period. As worded, it gives the sense that this is somehow wrong, whereas it is purely a matter of choice: do you want an integrating metric, or not?

P905/L21: this statement seems to causally mix two things: replace “as” with “and”,

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since the decaying RF and the energy being radiated back into space are two different processes. The latter is only of relevance to the GTP, whereas the former matters for both GWP and GTP.

P905/L27-28: This sentence needs correcting, since it is only true for the limited gases covered at present, and for the limited 100-year time horizon. Certainly not only for N2O but also for other long-lived GHGs such as SF6 and some PFCs, their value increases over several centuries.

P906/L1-3: With Daniel, I can’t make sense of the claim that the climate impact is governed by species with strong/short-lived and weak/long-lived impacts, since species with strong/long-lived impacts clearly matter even more! It becomes a somewhat empty statement though, since it’s obvious that species with weak/short-lived impacts don’t matter! Delete?

P906/L8-9: Again, you seem to use LLGHGs inconsistently: very clearly, for short-lived LLGHGs such as CH4, and some HFCs, the metric values certainly aren’t constant throughout the period. This needs to be revised.

P906/L10: perhaps insert “emissions from” to make clear what basis the ranking has.

P906/L11 and Figure 11: I would find Figure 11 more helpful if it used stacked bars, as it would more easily show (in my view) how the total effects add up. But that is a matter of judgement.

P906/P17-26: Might be worth stating that which countries are the top 10 emitters is also almost independent of metric – only France gets pushed into the top if GWP20 is used (presumably because of their relatively large fraction of CH4 emissions). In summary, it seems that metrics affect the ranking of countries less than one might have assumed – this is actually quite an important conclusion that might be worth even bringing into the abstract?

P907/L1: You need to specify which metric you use to determine the top 10 emitters

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since under GWP20, France would be in that list.

P907/L10: Explain why/how China can have more than 100% of its emissions from CH₄. I assume this is because of the negative emissions from SLCFs outweighing the emission of CO₂, leaving CH₄ to be effectively the only net GHG emission. Make explicit?

P907/L11: The heading would make more sense to me if it said “ranking of gases and emissions from sectors by metric and delta T”

P907/L13: Spell out that you are assuming constant 2008 emissions into the future for the sustained emissions scenario. It’s almost clear, but might help to say this explicitly.

P907/L12-28: MAJOR Following up on earlier comment: please state clearly whether you included changing background concentrations resulting from sustained emissions in your calculations, and if you haven’t, provide an estimate of the error introduced by this.

P908/L17: this summary provides a false advertisement since at present, you have included only a very limited subset of GHGs with simple exponential decay; as suggested in other comments, I would strongly encourage you to make this a more comprehensive analysis and provide data for a more complete set of gases (and time horizon out to 500 years)

P908/L23-25: Rather than saying you showed their utility, you might want to say that you showed examples of where metrics matter in policy-relevant applications such as the ranking of emissions from countries, sectors, and different gases, but also where they don’t actually matter all that much. In my experience there is a lot of unsubstantiated assumption in some policy circles that different metrics would radically change our view of the mitigation challenge (which could be a good or a bad thing, but that’s another matter), but your analysis, along with others, seems to demonstrate that while metrics matter in some important details and aspects, they don’t matter in some others.

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Spelling this out in some detail here would make the conclusions you present far more relevant to a wide audience. You may also wish to consider whether some more of the key results you obtain are worthwhile bringing into the abstract.

P908/L23-25: the statement creates a false dichotomy in that it implies that there is a choice between either using metrics calculated based on analytical expressions, or using GCMs without any metrics. But there are many other ways of addressing the question of how different emissions compare, namely using simple or reduced complexity climate models, either directly or to compute metrics and then use those metrics – or one could use GCMs to calculate metrics and then use those metrics for further applications.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 871, 2012.

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