CONTENTS

I. RESPONSES TO REFEREES AND CORRESPONDING REVISIONS

In the following I have copied the comments made by the referees. Many thanks for their criticism, to which I have responded by incorporating numerous revisions leading to what seems to me to to a better and more balanced discussion. Detailed responses and the corresponding revisions are interpolated in the referee reports.

In most cases page and line numbers below refer to the revised version of the paper; follow this link. In some cases, as indicated, these numbers refer to the original version of this paper.

I also posted a version, retrievable via the discussion tab of this link, of the new version of the paper that shows the detailed changes. There are sufficiently many changes that this version is barely readable, but it does provide an impressionistic overview.

A. Caldeira I

This section contains responses to the referee comments found by following this link to RC1.

1 (a) **Caldeira I:** The title of this paper suggests that its primary function is not to act as a scientific paper but rather to support a normative claim.

Concepts like what it means to be 'honest and responsible' in this context do not lend themselves to empirical tests, but rather express normative judgments.

GWPs are flawed metrics for almost every purpose, so I do not seek here to defend the use of GWPs. GWPs are metrics, and thus do not have a truth value. Like all tools, they can be more useful or less useful, but they cannot be right or wrong.

One could perhaps rephrase the title as: "Among global warming potential definitions, only the instantaneous global warming potential is useful", but this too is a matter of degree and depends on what you want to know.

(b) **Response:** The revised paper has a new title: *Proposed policymaker- friendly metric of radiative effects of greenhouse gases.* This should be a major step toward addressing the issues raised in these comments.

As far as values are concerned and statements that cannot be objectively confirmed, *i.e.*, are not strictly verifiable or falsifiable, this comes with the territory of policy tools and the trade-offs they necessarily imply. Whether or not a tool is useful depends on some sort of underlying utility function, which in turn will contain a scheme weighting choices and consequences, value judgments in other words.

The revised version of the paper avoids words such as "honest" and "responsible," which seem to have come across as inflammatory. They have been replaced by more neutral terminology. In addition, to address the confirmability issue, the new version of the paper features a more extensive discussion of and detailed references to IPCC's statement of purpose and criticism of the GWP going back to it Second Assessment; see page 2 paragraph at 20 and specific page references in footnote 1 on the same page.

Also included to provide context is a reference to a paper the title of which is *"Unmask temporal trade-offs in climate policy debates;"* see Ref. 1 in Science. All of this makes it clear that the proposed decision making tool contained in this paper is part of an ongoing scientific discussion at the interface between science and policy making.

2 (a) **Caldeira I:** If the author could come up with a cogent argument for what would be better than GWPs as conventionally defined that have not already been discussed in the literature, I would be open to reviewing that as a perspective or

opinion piece.

Addressing the normative claims, instantaneous radiative forcing values would seem to be a flawed metric for greenhouse gas accounting and attribution. Imagine two gases with the same instantaneous radiative forcing, but one decays in a year and the other remains in the atmosphere forever. Would it be wise to consider these two gases to be equivalent? The paper criticizes GWPs as conventionally defined but does not make a strong case for the use of instantaneous radiative forcing as an improvement. Indeed, many have criticized GWPs as conventionally defined for not considering effects on long time scales.

The author may want to resubmit as an opinion or perspective piece, but on a quick perusal I would not be enthusiastic to review that.

Ideally, in a policy context, one would like a metric to compare different greenhouse gases that would indicate the relative amount of damage that would be caused by an equal mass release of the different gases. This measure would be the ratio of the value of the damage caused by release of gas X to the damage caused by release of an equivalent quantity of CO2, where that time series of damage is appropriately turned into a scalar value to allow simple comparison.

Unfortunately, the estimation of future marginal damage and the conversion of time series to scalars (typically done in a net present value calculation) are rife with problems that have been widely discussed already. Further, the relative damage would also depend on the assumed background scenario against which these emissions occur. GWPs are used mostly because of historical legacy. They are clearly flawed metrics. Some people use them and are unaware with their deficiencies. Others use them, aware of their deficiencies. There are no doubt dishonest and irresponsible people who use these metrics to try to achieve nefarious ends. But users of flawed GWP metrics can be both honest and responsible.

(b) **Response:** As mentioned, the paper is presented as an attempt to construct a tool for policy makers. Indeed, the IPCC for the longest time has done exactly that and came up with the global warming potential (GWP) as a single number to quantify the joint effect of several greenhouse gases. The paper presents arguments to show that the time has come for science to provide a better tool than

the UNFCCC-blessed, widely used GWP with a 100-year horizon.

To substantiate this claim I expanded the discussion about outpaced climate change projections and "erring on the side of least drama" by including more context and additional references, such as Ref 2 and 3. There also is a new reference on tipping points—Ref. 4 in addition to *e.g.* the Hansen Ref. 5, which had already been included; see lines 3–13 on page 2.

The paper states explicitly that coming up with a simple, user-friendly decision making tool for a complex system such as the earth's climate is intrinsically difficult, if not impossible; see in particular page 2, lines 25–28.

The case for treating CO_2 as permanent and CH_4 as decaying is made by referring to Ref. 6 in the paragraph starting on line 6 on page 6.

The abstract, introduction (Sections 1) and the conclusions (Section 4) have been changed to reflect this. To be more specific, I included new references; see *e.g.* Refs. 7 and 8 and to references to recent, 2018 papers that highlight the decision timeframe and possible implications: see page 2 paragraph containing line 15.

(a) **Caldeira I;** And scientific papers should report previously unknown empirical facts, not value judgments.

(b) **Response:** If this sweeping statement were correct, reviews would not be scientific papers, nor would a vast segment of the applied mathematics literature qualify as scientific. Many of my publications in computational physics would not qualify. More specifically, and more importantly in the context of this exchange, it would also be impossible for scientific journals to contribute to providing better alternatives for the UNFCCC-accepted 100-year GWP. The view expressed by the referee would also disqualify Ref. 1 in Science and many other such papers. However that may be, as argued above, I rewrote the paper to be more compatible with what I think the referee really had in mind with this comment.

B. Caldeira II

The following responses refer to referee comments posted in this link, RC2.

3 (a) **Caldeira II:** One way of framing this paper as a scientific paper would be to support the claim: "Different emissions scenarios that are equivalent on conventional GWP metrics produce very different climate outcomes." I am not sure how many papers, if any, already make that point compellingly. A more useful paper would be to provide a new metric such that different emission scenarios that are equivalent on this new metric would all have very similar climate outcomes.

The focus on instantaneous effects suggested in the title of the Nightingale's would not satisfy this objective.

(b) **Response;** The proposed tool produces a time-dependent metric, the area (as a function of time) under the curves shown in Figs. 3 and 4 and the curves in Figs. 6 and 7. These numbers are roughly proportional to the heat absorbed in the climate system over time by the radiative imbalance. Certainly, as far CO_2 is concerned that is what the referee is looking for. This is why I incorporated the citation to the results of Ref. 6. They are summed up in the paragraph starting on line 6 of page 6. Adding to that the integrated instantaneous effect of methane yields a reasonable approach to produce a heuristic measure to track the outcome of different emission scenarios.

The presence of "instantaneous" in the old title did not contradict any of this, because the actual tool features the time-integral of the instantaneous effect, *i.e.*, the cumulative effect. Reversely, by kicking the can down the road, as use of GWP_{100} does, one will suppress the effect of different emission scenarios on a decadal time scale, a point already made in the previous version of the paper.

The citations to the work of Refs. 7 and 8 were added to provide further context for this line of reasoning. Are these arguments rigorous? No, not in the least, but they seem plausible, as argued both in the introduction of the paper and in the items listed on page 4, of which the first one (lines 1–3) is new.

C. Anonymous

This section contains responses to the referee comments posted in this link, RC3.

4 (a) **Anonymous:** I recommend that this paper is rejected.

The study is well motivated but flawed. I had expected (from the abstract) to find some coherent reason why the instantaneous GWP is superior to the normal GWP(100). However, all I find (p 3;l 18-19) is an assertion that this IS the case and then the rest of the paper follows as if that assertion is justified. In fact the abstract contains no useful information about the content of paper, but only really states the assertion.

(b) **Response:** The main scientific reason why the 100-year horizon lacks justification was explained in the original version of this paper: it is the mismatch of timescales mentioned in lines 8–15 of page 3 of the original paper; these lines are currently in slightly expanded form—see next paragraph for details—present on line 27 of page 3 through line 8 of page 4. Also, there is the mathematical argument making GWP_{100} unsuitable for dynamical tracking emssions time. This is mentioned on lines 18 and 19 of page 3 of the original paper, which correspond to lines 10–12 of page 4 in the current version.

Also the revised abstract makes reference to much of this; see lines 4 and 7 of page 1. In addition, the timescale mismatch argument has been strengthened in lines 14–16 of page 2, and lines 1–3 of page 4, as mentioned in the response in item 3.(b), and the newly incorporated references, *viz.* Refs. 7 and 8 on page 2 and Ref. 4 on page 4.

5 (a) **Anonymous:** I am no great fan of the GWP and the difficulties of using it to represent temperature change have long been known (its equivalence is formally restricted to time-integrated radiative forcing following a pulse emission). See for example Figure 3 of Fuglestvedt et al. (Climatic Change 58, 267-331, 2003) and many of the figures and references in Myhre et al. (2013).

There is much I disagree with in this paper, but I restrict myself to those aspects that I feel justify the rejection. The principal problem is that no account is taken of the much greater persistence time of CO2 perturbations, especially the fact that some of that CO2 is an essentially permanent addition to the atmosphere. This is acknowledged at p 2;l 28-29, but plays no subsequent part in the analysis. The only timescale used in the paper is methane's decay time.

(b) **Response:** As explained in detail in my response AR3, the principal problem

identified by the referee is based on a misconception. To clarify matters, the revised paper contains a more elaborate explanation of the approach. More explicitly, the fact that the equations feature two greenhouse gasses—one with an an infinite decay time, namely $CO_{@}$, and the other on CH_4, with a finite decay time—is explained more carefully in lines 6–8 on page 6 and also in the comments following Eq. (10), *i.e.*, in lines 15–18 on the same page. The rationale for the approach is also explained more carefully in the the newly added Ref. 6 on line 8 of page 6, which contains the justification of the treatment of the decay time of CO_2 as infinite in the proposed tool.

6 (a) **Anonymous:** The problem with the key figures (Figs 3 and 4) is that they just demonstrate the result of applying the assertion, rather than demonstrating that the assertion leads to a better representation of the resulting climate change than applying GWP(100), which is surely what matters. If the temperature effects (a simple physical model could be used in an illustrative context)) of using CO2-equivalents calculated using the GWP(0) was adopted, and compared with that resulting from the actual emissions (in the author's thought experiment) the temperature evolution of actual and CO2-equivalent emissions would be quite different. The impact of methane emissions from any given year would decay to near zero in a few decades, while much of its (large) equivalent in terms of CO2 using GWP(0) would remain in the atmosphere influencing climate for long periods.

(b) **Response:** I cannot follow what exactly the referee is driving at and do not know how to respond other than to say that part of what the referee writes seems to conflict with the contents of Figs. 1 and 2, but most of it might be related to the misconception addressed in the previous response, item 5.(b). As a matter of fact, the Eq. (10) describes exactly what the referee expects. I suspect that the revised version of the paper and in particular the new title explain more clearly what the purpose of the exercise is, namely to provide a better decision making tool than one of current UNFCCC scantioned, general use.

7 (a) **Anonymous:** The author invokes the precautionary principle but this only applies if the chosen metrics have demonstrable integrity. By placing a very large

multiplier on CH4 emissions, it would encourage large cuts to methane emissions in preference to those of CO2, but the longer-term consequences of such a choice would have to be explored to assess the extent to which such a policy is precautionary or ultimately leads to a greater climate change (which could only be reversed by the negative emissions that the author (p 9;l 7) regards as "fraught with danger").

(b) **Response:** As to the Precautionary Principle, its value as a public policy tool is not dependent on the proposed tool. The dependence runs in thee reverse direction. This, once again, is related to the timescale issue mentioned in item 4.(b). Given the vital importance of the decadal timescale, use of the 100-year time horizon is incompatible with the Precautionary Principle, as stated on line 32 of page 2, on line 8 of page 4, on line 29 of page 11, and one final time in line 2 of page 12. The revised version of the paper should do a better job of addressing this issue; indeed there are 7 references to the precautionary approach in the current version compared to the 5 in the original. The newly added Ref. 9 may help to convey that the Precautionary Principle is an accpeted ingredient of international treaties. It is therefore, the supreme law of the land. according to Article VI of the U.S. Constitution.

The sentence containing "fraught with danger" is no longer present; "negative emissions" is now part of a new first paragraph in the Conclusions section, lines 4–8 of page 11.

8 (a) **Anonymous:** The discussion surrounding Figures 1 and 2 is confused – again we are left with an assertion that the similarity between the Figures show consistency, when such consistency can only be demonstrated by converting emissions to changes in concentrations, changes in concentrations to radiative forcings and (transient) radiative forcing to (transient) changes in temperature. To do otherwise is to ignore the physics of the climate system. In essence, the attribution statements in IPCC AR5 are tracing through those necessary links.

(b) **Response:** If the climate system is perturbed by CO_2 emissions, a simple perturbation argument suggests that the changes in the temperature anomaly will develop with a delay along a similar trajectory. In other words, if one can fit

the emissions with an simple exponential curve, one would expect that the same exponent—hence the use of the term linear regression on line 7 of page 7—would describe the time dependence of the temperature anomaly. That is indeed the contents of lines 6 and 7 on that page. Maybe, if the goal were explicit prediction of the various proportionality constants and delays, one might have to follow the line of reasoning the referee seems to suggest. That would be an interesting calculation, but not one relevant within the scope of this paper.

I have to admit that it is not completely clear to me what the referee is looking for, but I expect that the newly included comments drawn from Ref. 6, mentioned above in the penultimate paragraph of items 2.(b) and in the first paragraph of item 3.(b), help explain the rationale for Figs. 1 and 2.

Let me also note that, in contrast to what the referee seems to suggest, the paper makes not attempt to provide an improved version of the Global Temperature change Potential (GTP) for example discussed on page 663 of Ref. 10. This might be an interesting exercise, but it is beyond the scope of the paper nor does it fit with the current UNFCCC practice of using the GWP_{100}.

II. CLOSING COMMENTS

Upon submission of the fist draft of the paper to Earth System Dynamics I conveyed to the editor that this was an unusual paper and not one that is typically found in science journals, my own publication record included.

Humanity is part of the dynamics of the earth and has brought about the Anthropocene. Prt of that dynamic is the fact is that the GWP_{100} is used as a decision making tool. This is true in spite of the fact that, as Caldeira stated that GWPs "are flawed metrics for almost every purpose"—see item 1.(a)—while the anonymous referee mentioned to be "no great fan of the GWP"—see item 5.(a). It is indeed hard to think of a serious climate model that would use the GWP_{100} as an input parameter. The problem that IPCC has tried to address by introducing the GWP as an decision making tool is that it it equally true that even the simplest climate models are too complicated for most policy makers.

This is the conundrum we face and all of this strongly suggests, to me at least, that it is important for the scientific community to bridge this gap and deal with the issues covered

in the paper. FOr that reason I do indeed greatly appreciate the editor's decision to put the paper out there for discussion.

I chose the original title of the paper—to a lesser extent the same applies to its contents—to convey that it contained both values and science. I seem to have gone overboard, bur in responding with changes to the referees' comments, I have tried to redress this problem.

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