Reviewer #2

This long and somewhat tedious algebraic expansion of ordinary differential equations used to describe atmospheric chemistry and global warming begins on the wrong foot, and fails to produce any enlightening results. In the Discussion and the Conclusions, the paper reiterates well established facts about greenhouse gas warming and draws mainly policy conclusions. It does not really belong in a science journal like ACP.

Despite our difference of opinion regarding whether our work belongs in ACP, we thank the reviewer for comments that materially improved our manuscript.

Our manuscript draws no policy conclusions. Our conclusions all state empirical information about radiative forcing or temperature changes from greenhouse gases, or they are statements of mathematical fact. We note the policy relevance of our work, but make no policy recommendations or conclusions.

We are not claiming that our results are novel, except that we found a small error in the work of Ocko and Hamburg (2022) repeating the same error in Warwick et al. (2022). We choice to submit our results as a commentary to ACP with the hope to provide important context to complement results of Ocko and Hamburg (2022). We now have sentences to emphasize our motivation that read:

"*In this commentary, we provide additional context for Ocko and Hamburg (2022) related to the climate consequences of replacing fossil fuels with clean hydrogen alternatives.*"

"*Ocko and Hamburg (2022) defined a metric based on time-integrated radiative forcing from continuous emissions. To complement their analysis, we further present results for temperature and radiative forcing over the next centuries for unit pulse and continuous emissions scenarios. Our results are qualitatively consistent with previous studies, including Ocko and Hamburg (2022).*"

"*Our aim here is to complement Ocko and Hamburg (2022), which emphasizes the near term, with an analysis that places greater emphasis on long-term outcomes using newly developed equations.*"

This is written as a commentary on Ocko and Hamburg's 2022 paper. At 18 pages of text, this is hardly a Commentary.

We agree with the reviewer that parts were a little wordier than needed. The abstract is now cut down to 227 words. The Introduction is now 233 words. We then have 7 pages of Methods and Equations, which is a concise tutorial for the derivation of equations found in Warwick et al (2022) and Ocko and Hamburg (2022). There are three concise pages of Results, two concise pages of Discussion and slightly over one concise page of Conclusions. This comes to 14 pages plus Abstract. We note that there are no specific page limits for commentaries in ACP, but of course concision is always appreciated.

We believe that the derivations of the equations are important in the main paper, as this derivation is not provided by Ocko and Hamburg (2022) nor Warwick et al. (2022). The derivation of the equations is not trivial. In fact, in the process of re-deriving these equations, we found a minor error in an integral presented in Warwick et al. (2022), as confirmed in a Zoom call and follow-up emails with the author team of Warwick et a. (2022). The fact that no derivation was shown and there is an error in the derivation is one of the issues we are trying to fix with our commentary.

The authors propose that this work goes way beyond Ocko and Hamburg because it considers impacts out to 500 years does not make sense to this reviewer because any of the impacts of CH_4-H_2 emissions disappear after 4-5 decades.

We do not understand how the reviewer got the impression that we claimed our work goes "way beyond" that of Ocko and Hamburg (2022). We would be happy to add additional language that would serve to dispel this notion. In our Abstract, we wrote:

"*In this commentary, we provide additional context for Ocko and Hamburg (2022) related to the climate consequences of replacing fossil fuels with clean hydrogen alternatives.*"

Our purpose was to reproduce their results and provide additional understanding of and context for those results. Our work aims to complement Ocko and Hamburg (2022) by be asking readers to consider the long atmospheric lifetime of CO_2 when assessing the relative costs and benefits of hydrogen emissions.

The reviewer is right about the short lifetime of methane and hydrogen. The reason we are showing longer time scales is to contrast this short lifetime with the relatively long lifetime of CO_2. We have a sentence in our abstract that reads:

"*After a cessation of clean hydrogen consumption, the earth cools rapidly, whereas after a cessation of carbon dioxide emissions, the earth continues to warm somewhat and remains warm for many centuries.*"

We also have a sentence in Conclusion that reads:

"*In contrast to the climate impact of carbon dioxide emissions, which persist for many millennia (Archer, 2005; Solomon et al., 2009), climate impacts decay on the timescale of decades after a cessation of methane or hydrogen emissions.*"

Admittedly fossil carbon counts as CO_2 does accumulate, but that is a different accounting and does not need the following of H_2 and CH_4 as done here. Thus integrating their equations out 500 years depends solely on the scenario for future technology and energy use at year 2600. This is not a scientific issue.

We did not make it sufficiently clear that we are doing highly stylized calculations of the sort performed by Ocko and Hamburg (2022). We are not making predictions or projections related to real energy systems. In a linear system, consequences of any time-varying forcing can be represented by a convolution integral of either a unit-pulse emission or a continuous emission. We are deriving the sort of equation that could be used in a Green's function calculation and not trying to represent any real emissions trajectory. We have added the following to the beginning of our Methods section:

"*We estimate the global mean temperature change from emissions of CO_2, CH_4, or H_2 using a linearized Green's function approach, and apply these equations to simple idealized cases*."

Their step-by-step tutorial might be useful somewhere if they had not missed the point entirely about indirect greenhouse gases (CO, H2) and chemical feedbacks (CH4). The authors demonstrate a basic lack of knowledge of the first principles of atmospheric chemistry. The concept of steady state and how it relates to the pulsed emissions is well known but seems to be overlooked here (e.g. lines 22-23). From the start (e.g., equation 1-3) the authors miss the point that their 'taus' are not constant but through chemical feedbacks depend on the second-order terms. Moreover, the budget lifetime used in the continuity equations ('tau') is most often NOT the time scale for decay of a perturbation.

One of the authors of this commentary (Caldeira) co-authored a paper on the non-linearities and scenario dependency in radiative forcing in a study published 30 years ago (Caldeira and Kasting, 1993, Insensitivity of global warming potentials to carbon dioxide emission scenarios, Nature 366), so we are well aware of the issues raised by the reviewer. Our goal here was to follow the practice of Warwick et al (2022) and Ocko and Hamburg (2022) in using a linearization around a time-invariant background state.

In our Discussion section we wrote:

"The radiative forcing calculation presented here is a linear approximation, with radiative forcing increasing linearly with concentration, when in fact absorption bands become increasingly saturated at higher concentrations, and this results in less sensitivity at higher concentrations. The radiative forcing calculation assumes an unchanging background atmospheric composition, whereas it is likely that the climate impact of an emission will depend on the background climate state (Duan et al., 2019; Robrecht et al., 2019). For instance, the indirect radiative forcing of hydrogen through its effect on methane's lifetime might depend on the background methane concentration. The effectiveness of radiative forcing at affecting temperature can vary substantially from gas to gas (Hansen et al., 1997; Modak et al., 2018)."

While we agree with the reviewer that it would be good to take into consideration these non-linearities, the derivations of the radiative forcing impact for hydrogen follows the same assumptions that are used for Warwick et al (2021) and Ocko and Hamburg (2022) so that everything is under the same framework. Incidentally, our understanding is that the convention is to use a constant atmosphere because the scientists want the number to be based on something that everyone can agree on and measure. If GWP values were to be defined in terms of future anticipated atmospheric concentrations, there would be no uniformly agreed upon scenario to serve as a basis for this calculation.

In responding to the reviewer's comment, we have changed the word "lifetime" to "perturbation lifetime" in the paper.

This manuscript is clearly not a tutorial. The concept of perturbation lifetime, budget lifetime and timescales is well known. As is the decomposition of the decaying perturbation into a sum of exponential decays as is done for CO_2. See for example,

IPCC WGI AR4 7.4.5.2

AR5 8.2.3.3

AR6 7.6.1.1, 7.6.1.5

Holmes, C. D. (2018). Methane feedback on atmospheric chemistry: Methods, models, and mechanisms. Journal of Advances in Modeling EarthSystems,10, 1087–1099. https://doi.org/10.1002/2017MS00119

Fuglestvedt, J. S., Isaksen, I., & Wang, W.-C. (1996). Estimates of indirect global warming potentials for CH4, CO and NOx.Climatic Change,34(3–4), 405–437.

Prather, M. (1994). Lifetimes and eigenstates in atmospheric chemistry.Geophysical Research Letters,21(9), 801–804. http://doi.org/10.1029/94GL00840

Prather, M. J. (2007). Lifetimes and time scales in atmospheric chemistry.Philosophical Transactions of the Royal Society A,365(1856), 1705–1726. http://doi.org/10.1098/rsta.2007.2040

Nguyen, N. H., Turner, A. J.,Yin, Y., et al (2020). Effects of chemical feedbacks on decadal methane emissions estimates. Geophysical Research Letters, 47, e2019GL085706. doi.org/10.1029/2019GL085706.

The Merriam Webster defines “tutorial” to mean “a paper … that provides practical information about a specific subject”. If we understand the reviewer correctly, the assumption is that we are trying to go “far beyond” Ocko and Hamburg (2022) and do something that was novel. However, our goal was to provide context for and help explain the results of Ocko and Hamburg (2022). Our understanding is that the word “tutorial” typically refers to the teaching of material that is well-understood by experts.

The derivations of equations describing the radiative forcing from hydrogen emissions are based on Warwick et al. (2022), and other equations, including radiative forcing from CO_2 and methane emissions and the temperature response function, are directly taken from previous papers that are cited in our text. For example, we have added sentences that read:

“*We derive and apply the equations underlying the estimate of radiative forcing from hydrogen emissions as presented by Warwick et al. (2022), relying heavily on parameter values from Ocko and Hamburg (2022), Table S1. Equations describing the radiative forcing of CO_2 and CH_4 is* *based on Myhre et al. (2013). The calculation of the global mean temperature response is based on Gasser et al. (2017).*"