Reviewer #1

This is a useful systematic discussion of different ways of assessing the climate impacts of hydrogen emissions. I have no objection to it being published following modification and clarification, although overall many of the qualitative results could have been anticipated from discussions of the climate impacts of short-lived forcers, compared to CO2, already in the literature.

However, I struggled with the motivation. It is tied closely (in the title) to the Ocko and Hamburg (OH) paper, and the prime reason for doing this appears to be an objection to OH's use of a GWP assuming constant emissions. This objection (which I have sympathy with, although I find OH to be an otherwise very useful paper) only really comes out in the Discussion and Conclusions. It is only tangentially referred to in the Abstract, and not in a negative sense. I think the direct connection (in the title) to OH could be removed, without affecting the content, making this a more general discussion of how the perception of hydrogen climate impacts varies depending on the adopted framework.

Thanks for your comments. As mentioned in the paper, our work aims to complement the analysis of Ocko and Hamburg (2022) by showing time-evolving changes of radiative forcing and temperature response covering a wider range of timescales (e.g., 500 years). We also show the derivation of the analytical solution and sensitivity simulations. We emphasis that both the near-term and long-term impacts are important for decision making and therefore the purpose of the paper is to add values instead of denying their work. Since our paper can be treated as an extension to Ocko and Hamburg (2022), and we share the same parameter settings and underlying assumptions, it would be helpful to relate this paper to Ocko and Hamburg (2022) for broader interests.

To clarify our motivation, we now write in the abstract:

"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."

We also have a paragraph in Introduction that reads:

"*Here we provide context for understanding the results of Ocko and Hamburg (2022) in three different ways: (1) We present equations underlying the time evolution of hydrogen and its radiative and thermal consequences, and solve them analytically for a unit pulse and continuous hydrogen emissions scenarios; (2) We present global mean temperature and radiative forcing in the time domain; (3) We examine three emission scenarios, including a unit pulse emission, a limited-duration (square-wave) emission, and continuous emissions. 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.*"

Comments

Lines 19-26: I agree with all written here, but it is presented as if it is new. I believe these points are clear and well established from the previous extensive literature on comparing climate impacts of emissions of short and long-lived forcers. The wording should be changed to indicate this and more reference to this previous literature would be appropriate in the paper as a whole. This way, the reader can separate out what is new in this paper and what is already known.

We thank the reviewer for pointing this out. We are not claiming that our results are new. The goal of our analysis is to provide context for understanding the results presented by Ocko and Hamburg (2022) by showing results in the time domain extending out to a time horizon of 500 years. Our attempt was to NOT do anything new, but in the course of this exercise we found a small error in the work of Ocko and Hamburg (2022) repeating the same error in Warwick et al. (2022). This is our only claim to novelty in this work.

To make this clear, we have the first sentence in our abstract that reads:

"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."

Thus, our goal is to provide important context for interpreting the results of Ocko and Hamburg (2022); our goal was not to produce novel results.

Rather than claiming novelty, we have stated in the abstract that:

"Our results are qualitatively consistent with previous studies, including Ocko and Hamburg (2022)."

Based on the other reviewer's comments, we have substantially shortened the abstract, and we now have slightly modified sentences that read:

"Our results clearly show that for the same quantity of emissions, hydrogen shows consistently smaller climate impact than methane. As with other short-lived species, the radiative forcing from a continuous emission of hydrogen is proportional to emission rates, whereas the radiative forcing from a continuous emission of carbon dioxide is closely related to cumulative emissions."

We have also followed the reviewer's comment and added citations when similar results have been presented in previous studies.

Lines 111 and 113: This discussion (coupled with lines 330-342 – see later) is confused and unhelpful for what (line 13) purports to be "a step-by-step tutorial". Instantaneous radiative forcing (line 111) has a specific definition relating to the absence of any physical adjustment (with stratospheric temperature adjustment or full (ERF) adjustment) and so is inappropriate here. The adjustments referred to on line 113 are long-established chemical adjustments (ozone and stratospheric water vapor – including stratospheric temperature adjustment as I understand it) but for some reason this is not made clear to the reader.

We thank the reviewer for pointing out this error. We have eliminated the word "instantaneous". Further, as suggested by the reviewer, we now write:

"The chemically-adjusted radiative forcing, $A^*_{CH_4}$, from methane forcing uses factors f_1 and f_2 (Myhre et al., 2013) to represent the effect on ozone and stratospheric water vapor:"

Line 149: Apologies if I miss the obvious, but I cannot derive Equation (12d). It seems to imply that Equation (7b) with H replaced by t is (H-t) times Equation (6b), which it isn't.

In the revised manuscript, we have extended equation (12) which now reads:

$$CAGWP_{H_2}(H) = \int_0^H R_{H_2,cont}(t)dt \qquad (12a)$$

$$= \int_0^H \int_0^t R_{H_2}(\tau)\, d\tau\, dt \qquad (12b)$$

$$= \int_0^H \int_t^H R_{H_2}(\tau)\ dt\, d\tau \quad (12c)$$

$$= \int_0^H \left(\int_t^H dt \right) R_{H_2}(\tau) d\tau \quad (12d)$$

$$= \int_0^H (H - t) R_{H_2}(t) dt \quad (12e)$$

Also see page 98 in Paul Nahin's book, "Dr. Euler's Fabulous Formula: Cures Many Mathematical Ills".

Lines 161-162: "better reflect the underlying conceptual model". I cannot make sense of this. As I understand it, this paper adopts a different conceptual model (pulse emission from time zero) compared to Fuglestvedt et al. (2010 10.1016/j.atmosenv.2009.04.044). They adopted a 1-year emission, rather than an instantaneous pulse, with a stated aim of reducing time of year dependence for short-lived species. Of course, that conceptual model is open to question, but their equations (noting the interesting comment at Lines 31-32 in Text S1) reflect their conceptual model. I agree that the differences reflect the different underlying conceptual models, but the "better" seems too far of a reach.

In the revised manuscript, we have revised this sentence:

"When used to estimate radiative forcing for identical cases, numerical differences between our equations and equations presented by Warwick et al. (2022) are small and are unlikely to make a material difference."

Line 194: You need to state the units of the quantities in these equations. Nowhere prior to this are we told that t is in years.

Thank you for pointing out this oversight. When t is first introduced, we now write:

"The change of H_2 molar mass relative to an unperturbed background condition, as a function of the time horizon t in units of year, is represented by…"

Further, we have added a new table in SI (Table S1) to show parameter values and units used in our analysis:

"*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.*"

Lines, 194, 207 etc: I wondered why the asterisks suddenly appear in these equations.

Thanks for noticing that. We have removed the asterisks in these equations.

Line 195-201: Since these equations are only used in the SI, I suggest they are moved there so as not to burden the main text.

We have moved equations 25 to 27 to SI Text S3:

"*Uncertainty in the temperature response function is shown in **Text S3**.*"

We have further moved equations 28 to 31 to SI Text S4 to shorten the length of the main text:

"*Climate impacts from hydrogen or fossil fuels are the summation of climate impacts of one or more components in a linear system (**Text S4**).*"

Lines 206 and 209: "emissions" is used on one line and "leakage" on another. Be consistent. I think "leakage" is better in both. Are the leakage rates assumed independent of time?

We now use the term "leakage" consistently in Text S4.

Line 235: "very beginning" is not correct for the temperature response

Obviously at t = 0, the temperature response is zero for all species emitted, but the initial rate of warming is proportional to the radiative forcing for all species emitted. To make this clearer, we now write:

"Soon after emission, per kg emitted, methane and hydrogen show much larger impacts compared to CO_2."

Lines 239-241: I did not understand this. If methane (M=16) is affected, then H2 (M=2) should be more affected. Section 8.SM.11.3 of the Supplementary Material to Myhre et al. (2013) outlines the methodology for ppb to kg conversions. Using this, I couldn't reconcile the numbers in Figure S2. Again, apologies if I miss the obvious.

We used the conversion factor for converting H_2 mixing ratio into (ppb) mass (kg) from Ocko and Hamburg (2022), which used a number from Warwick et al (2022) that seems to assume a molar mass of the atmosphere that is only about 80% implied by Myhre et al. (2013). Because this study is looking at the relative effects of different gases, and we are using a linearized approach, this will not affect our results related to the relative impacts of leakage of different gases. Nevertheless, we have updated all calculations to use the Myhre et al. (2013) numbers: 2.82×10^{-9} ppb kg^{-1} for hydrogen, 3.53×10^{-10} ppb kg^{-1} for methane, and 1.28×10^{-10} ppb kg^{-1} for carbon dioxide (explained in Table S1). The ratios of the conversion factors between species remains unchanged. In the revised manuscript, we have updated our calculations, table, and figures to reflect the Myhre et al. (2013) values.

Lines 254-257, 293-295, 368-369 and elsewhere: These statements are all consistent with the Allen et al. (2009) cumulative emission framework and I wonder why this prior understanding is never acknowledged.

We have added this citation in the revised manuscript as suggested:

"*To a close approximation, on the time scale of decades or more, temperature change from methane or hydrogen emissions are proportional to rates of emission whereas temperature change from carbon dioxide is proportional to cumulative emission (Jones et al., 2006; Allen et al., 2009).*"

Lines 330: "More important uncertainties". There are others not referred to here, including the impact of fossil-fuel co-emissions (such as ozone and aerosol precursors) and possible role of where emissions occur, known to be important for some short-lived forcers. In addition, the possible role of oxidant-related aerosol forcing needs alluding to (e.g., O'Connor et al. https://doi.org/10.1029/2022MS002991)

Thanks for pointing these out. We have added sentences in the revised manuscript to discuss these uncertainties:

"Many important uncertainties persist. For example, we considered the chemical adjustments to radiative forcing for methane due to effects on ozone and stratospheric water vapor, as considered by Ocko and Hamburg (2022). There are other effects that have been included in previous works, which would affect the warming impact of methane emissions (Boucher et al., 2009; Shindell et al., 2009). There are uncertainties related to cloud radiative effects from thermodynamic adjustments and aerosol-cloud interactions (O'Connor et al., 2022). There are

additional uncertainties related to the fast physical radiative forcing adjustments to dioxide, ozone and other radiatively active gases (Smith et al., 2018). Co-emissions from fossil fuel combustions (e.g., aerosol precursors) can also affect climate and public health (Lelieveld et al., 2019). Unlike the long-lived CO_2*, the climate impact of short-lived forcers might depend on locations of emissions (Persad and Caldeira, 2018; Burney et al., 2022). While their radiative forcing might diminish quickly after emission ceasing, indirect impacts from these short-lived forcers (e.g., by affecting carbon sinks and atmospheric* CO_2 *levels) could last longer, introducing additional uncertainties (Fu et al., 2020). None of these considerations are expected to be of sufficient magnitudes to qualitatively alter key conclusions presented here."*

Lines 330-344: This seems confused. On line 330, the fast adjustments referred to are the fast chemical (ozone, strat water vapor) adjustments. The adjustments on line 332 are the physical adjustments that act beyond (or in concert with) these adjustments and are functionally strongly related to the efficacy implied in lines 340-342.

We have separated these discussions into two paragraphs in the revised manuscript:

"*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). In addition, the framework used here only compares hydrogen with the avoided* CO_2 *emissions, while fossil fuel adaptation are associated with emissions of other radiatively active species and air pollutants (on Climate Change, 2018)."*

Many important uncertainties persist. For example ..."

Lines 369-370: "not captured"? Why is it not captured by the GTP? Figure 1 seems to capture it very well.

We agree the GTP metric can be a useful metric. The focus of our paper is largely to argue that the CAGWP metric proposed by Ocko and Hamburg, being equivalent to a front-loaded weighting of radiative forcing from a pulse emission, can overweight near-term considerations for many applications. We now write:

"This important distinction is not captured by the CAGWP metric proposed by Ocko and Hamburg (2022)."

Line 379: Several of the points here are not new and have been well represented in the literature discussing the climate impacts of short-lived forcers compared to CO2. e.g., Lines 384-387, 393-396 (e.g., Fuglestvedt et al. 2010; Allen et al. 2016 and references therein). Some change of wording is needed to make this clear.

We have updated these sentences based on the reviewer's suggestion:

"*In line with previous studies (Fuglestvedt et al., 2010; Allen et al., 2016; Balcombe et al., 2018), both the radiative forcing and global mean temperature response from hydrogen and methane are proportional to the underlying emission rates, whereas climate impacts from carbon dioxide are closely related to cumulative emissions. For the same quantity of emissions, hydrogen shows consistently smaller climate impact than methane.*"

Figure 2 and 3: The font on the y axes of these is too small. I am not sure all this text is needed as long as it is clear in captions and legend.

We have modified the font for Figure 2 and 3 to make it easier to see.

Tables S1, S2 and S3: The units of AGWP and AGTP need attending to.

We have updated these tables as suggested.

Other comments

1. The references to both Ocko & Hamburg (which only points to ACPD rather than the ACP version) and Warwick et al. are incomplete in both the main text and Supporting Information.

These references have been updated.

2. The front page of the AR5 Chapter 8 makes it clear that it should be cited as Myhre et al. (2013) not Myhre et al. (2014).

Reference has been updated.

3. This is not meant to be a hostile comment. The authors claim no competing interests, but the second author lists a commercial “net zero” company as a second affiliation. As I understand ACP’s competing interests policy, this should be declared. Or perhaps it is already enough that it is declared in the affiliations? This is for the Editors.

We have updated our competing interest statement to read:

“The authors declare that they have no conflict of interest. However, in the interest of transparency, we would like to point out that K.C. is an employee of a non-profit organization that funds early commercial demonstration projects related to clean alternatives that can displace carbon-intensive technologies, and this can include clean hydrogen to decarbonize industry. In the further interest of transparency, note that L.D. is a consultant for a for-profit entity that has no known investments related to clean hydrogen.”

K.C.’s job is to provide the best available scientific information to make better decisions. His job is not to torque the science to support decisions previously made.

4. I wondered why the authors adopted AR5 rather than AR6 methodologies (e.g., on the indirect chemical forcing for methane). Consistency with OH?

Our analysis follows Ocko and Hamburg (2022). For carbon dioxide and methane, Ocko and Hamburg (2022) used the IPCC formulations and parameter values from both AR5 and AR6. Parameters are updated according to AR6 including radiative efficiency, perturbation lifetime, and indirect effect scaling factors for methane.