

1 Reviewer #1

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

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

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

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

25 *“Ocko and Hamburg (2022) defined a metric based on time-integrated radiative forcing from*
26 *continuous emissions. To complement their analysis, we further present results for temperature*
27 *and radiative forcing over the next centuries for unit pulse and continuous emissions scenarios.”*

28 We also have a paragraph in Introduction that reads:

29 “Here we provide context for understanding the results of Ocko and Hamburg (2022) in three
30 different ways: (1) We present equations underlying the time evolution of hydrogen and its
31 radiative and thermal consequences, and solve them analytically for a unit pulse and continuous
32 hydrogen emissions scenarios; (2) We present global mean temperature and radiative forcing in
33 the time domain; (3) We examine three emission scenarios, including a unit pulse emission, a
34 limited-duration (square-wave) emission, and continuous emissions. Our aim here is to
35 complement Ocko and Hamburg (2022), which emphasizes the near term, with an analysis that
36 places greater emphasis on long-term outcomes using newly developed equations.”

37 Comments

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

43 We thank the reviewer for pointing this out. We are not claiming that our results are new. The
44 goal of our analysis is to provide context for understanding the results presented by Ocko and
45 Hamburg (2022) by showing results in the time domain extending out to a time horizon of 500
46 years. Our attempt was to NOT do anything new, but in the course of this exercise we found a
47 small error in the work of Ocko and Hamburg (2022) repeating the same error in Warwick et al.
48 (2022). This is our only claim to novelty in this work.

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

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

52 Thus, our goal is to provide important context for interpreting the results of Ocko and Hamburg
53 (2022); our goal was not to produce novel results.

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

55 “Our results are qualitatively consistent with previous studies, including Ocko and Hamburg
56 (2022).”

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

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

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

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

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

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

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

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

80
$$CAGWP_{H_2}(H) = \int_0^H R_{H_2,cont}(t) dt \quad (12a)$$

81
$$= \int_0^H \int_0^t R_{H_2}(\tau) d\tau dt \quad (12b)$$

82
$$= \int_0^H \int_t^H R_{H_2}(\tau) dt d\tau \quad (12c)$$

83
$$= \int_0^H \left(\int_t^H dt \right) R_{H_2}(\tau) d\tau \quad (12d)$$

84
$$= \int_0^H (H - t) R_{H_2}(t) dt \quad (12e)$$

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

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

95 In the revised manuscript, we have revised this sentence:

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

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

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

102 *"The change of H₂ molar mass relative to an unperturbed background condition, as a function of*
103 *the time horizon t in units of year, is represented by..."*

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

106 “We derive and apply the equations underlying the estimate of radiative forcing from hydrogen
107 emissions as presented by Warwick et al. (2022), relying heavily on parameter values from Ocko
108 and Hamburg (2022), Table S1.”

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

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

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

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

114 “Uncertainty in the temperature response function is shown in **Text S3**.”

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

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

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

120 We now use the term “leakage” consistently in Text S4.

121 Line 235: “very beginning” is not correct for the temperature response

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

125 “Soon after emission, per kg emitted, methane and hydrogen show much larger impacts
126 compared to CO_2 .”

127 Lines 239-241: I did not understand this. If methane ($M=16$) is affected, then H_2 ($M=2$) should
128 be more affected. Section 8.SM.11.3 of the Supplementary Material to Myhre et al. (2013)
129 outlines the methodology for ppb to kg conversions. Using this, I couldn’t reconcile the numbers
130 in Figure S2. Again, apologies if I miss the obvious.

131 We used the conversion factor for converting H_2 mixing ratio into (ppb) mass (kg) from Ocko
132 and Hamburg (2022), which used a number from Warwick et al (2022) that seems to assume a

133 molar mass of the atmosphere that is only about 80% implied by Myhre et al. (2013). Because
134 this study is looking at the relative effects of different gases, and we are using a linearized
135 approach, this will not affect our results related to the relative impacts of leakage of different
136 gases. Nevertheless, we have updated all calculations to use the Myhre et al. (2013) numbers:
137 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
138 for carbon dioxide (explained in Table S1). The ratios of the conversion factors between species
139 remains unchanged. In the revised manuscript, we have updated our calculations, table, and
140 figures to reflect the Myhre et al. (2013) values.

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

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

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

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

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

156 *“Many important uncertainties persist. For example, we considered the chemical adjustments to*
157 *radiative forcing for methane due to effects on ozone and stratospheric water vapor, as*
158 *considered by Ocko and Hamburg (2022). There are other effects that have been included in*
159 *previous works, which would affect the warming impact of methane emissions (Boucher et al.,*
160 *2009; Shindell et al., 2009). There are uncertainties related to cloud radiative effects from*
161 *thermodynamic adjustments and aerosol-cloud interactions (O’Connor et al., 2022). There are*

162 *additional uncertainties related to the fast physical radiative forcing adjustments to dioxide,*
163 *ozone and other radiatively active gases (Smith et al., 2018). Co-emissions from fossil fuel*
164 *combustions (e.g., aerosol precursors) can also affect climate and public health (Lelieveld et al.,*
165 *2019). Unlike the long-lived CO₂, the climate impact of short-lived forcers might depend on*
166 *locations of emissions (Persad and Caldeira, 2018; Burney et al., 2022). While their radiative*
167 *forcing might diminish quickly after emission ceasing, indirect impacts from these short-lived*
168 *forcers (e.g., by affecting carbon sinks and atmospheric CO₂ levels) could last longer,*
169 *introducing additional uncertainties (Fu et al., 2020). None of these considerations are expected*
170 *to be of sufficient magnitudes to qualitatively alter key conclusions presented here.”*

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

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

176 *“The radiative forcing calculation presented here is a linear approximation, with radiative*
177 *forcing increasing linearly with concentration, when in fact absorption bands become*
178 *increasingly saturated at higher concentrations, and this results in less sensitivity at higher*
179 *concentrations. The radiative forcing calculation assumes an unchanging background*
180 *atmospheric composition, whereas it is likely that the climate impact of an emission will depend*
181 *on the background climate state (Duan et al., 2019; Robrecht et al., 2019). For instance, the*
182 *indirect radiative forcing of hydrogen through its effect on methane’s lifetime might depend on*
183 *the background methane concentration. The effectiveness of radiative forcing at affecting*
184 *temperature can vary substantially from gas to gas (Hansen et al., 1997; Modak et al., 2018). In*
185 *addition, the framework used here only compares hydrogen with the avoided CO₂ emissions,*
186 *while fossil fuel adaptation are associated with emissions of other radiatively active species and*
187 *air pollutants (on Climate Change, 2018).”*

188 *Many important uncertainties persist. For example ...”*

189 *Lines 369-370: “not captured”? Why is it not captured by the GTP? Figure 1 seems to capture it*
190 *very well.*

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

195 *“This important distinction is not captured by the CAGWP metric proposed by Ocko and*
196 *Hamburg (2022).”*

197 Line 379: Several of the points here are not new and have been well represented in the literature
198 discussing the climate impacts of short-lived forcers compared to CO₂. e.g., Lines 384-387, 393-
199 396 (e.g., Fuglestedt et al. 2010; Allen et al. 2016 and references therein). Some change of
200 wording is needed to make this clear.

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

202 *“In line with previous studies (Fuglestedt et al., 2010; Allen et al., 2016; Balcombe et al.,*
203 *2018), both the radiative forcing and global mean temperature response from hydrogen and*
204 *methane are proportional to the underlying emission rates, whereas climate impacts from*
205 *carbon dioxide are closely related to cumulative emissions. For the same quantity of emissions,*
206 *hydrogen shows consistently smaller climate impact than methane.”*

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

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

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

211 We have updated these tables as suggested.

212 Other comments

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

215 These references have been updated.

216 2. The front page of the AR5 Chapter 8 makes it clear that it should be cited as Myhre et al.
217 (2013) not Myhre et al. (2014).

218 Reference has been updated.

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

223 We have updated our competing interest statement to read:

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

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

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

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

238 Reviewer #2

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

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

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

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

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

257 *“Ocko and Hamburg (2022) defined a metric based on time-integrated radiative forcing from*
258 *continuous emissions. To complement their analysis, we further present results for temperature*
259 *and radiative forcing over the next centuries for unit pulse and continuous emissions scenarios.*
260 *Our results are qualitatively consistent with previous studies, including Ocko and Hamburg*
261 *(2022).”*

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

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

267 We agree with the reviewer that parts were a little wordier than needed. The abstract is now cut
268 down to 227 words. The Introduction is now 233 words. We then have 7 pages of Methods and
269 Equations, which is a concise tutorial for the derivation of equations found in Warwick et al
270 (2022) and Ocko and Hamburg (2022). There are three concise pages of Results, two concise
271 pages of Discussion and slightly over one concise page of Conclusions. This comes to 14 pages
272 plus Abstract. We note that there are no specific page limits for commentaries in ACP, but of
273 course concision is always appreciated.

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

281 [The authors propose that this work goes way beyond Ocko and Hamburg because it considers](#)
282 [impacts out to 500 years does not make sense to this reviewer because any of the impacts of](#)
283 [CH₄-H₂ emissions disappear after 4-5 decades.](#)

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

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

289 Our purpose was to reproduce their results and provide additional understanding of and context
290 for those results. Our work aims to complement Ocko and Hamburg (2022) by be asking readers
291 to consider the long atmospheric lifetime of CO₂ when assessing the relative costs and benefits
292 of hydrogen emissions.

293 The reviewer is right about the short lifetime of methane and hydrogen. The reason we are
294 showing longer time scales is to contrast this short lifetime with the relatively long lifetime of
295 CO₂. We have a sentence in our abstract that reads:

296 “After a cessation of clean hydrogen consumption, the earth cools rapidly, whereas after a
297 cessation of carbon dioxide emissions, the earth continues to warm somewhat and remains warm
298 for many centuries.”

299 We also have a sentence in Conclusion that reads:

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

303 Admittedly fossil carbon counts as CO₂ does accumulate, but that is a different accounting and
304 does not need the following of H₂ and CH₄ as done here. Thus integrating their equations out 500
305 years depends solely on the scenario for future technology and energy use at year 2600. This is
306 not a scientific issue.

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

314 “We estimate the global mean temperature change from emissions of CO₂, CH₄, or H₂ using a
315 linearized Green’s function approach, and apply these equations to simple idealized cases.”

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

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

330 In our Discussion section we wrote:

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

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

348 In responding to the reviewer’s comment, we have changed the word “lifetime” to “perturbation
349 lifetime” in the paper.

350 This manuscript is clearly not a tutorial. The concept of perturbation lifetime, budget lifetime and
351 timescales is well known. As is the decomposition of the decaying perturbation into a sum of
352 exponential decays as is done for CO₂. See for example,

353 [IPCC WGI AR4 7.4.5.2](#)

354 AR5 8.2.3.3

355 AR6 7.6.1.1, 7.6.1.5

356 Holmes, C. D. (2018). Methane feedback on atmospheric chemistry: Methods, models, and
357 mechanisms. *Journal of Advances in Modeling Earth Systems*, 10, 1087–1099.

358 <https://doi.org/10.1002/2017MS00119>

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

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

363 Prather, M. J. (2007). Lifetimes and time scales in atmospheric chemistry. *Philosophical*
364 *Transactions of the Royal Society A*, 365(1856), 1705–1726.

365 <http://doi.org/10.1098/rsta.2007.2040>

366 Nguyen, N. H., Turner, A. J., Yin, Y., et al (2020). Effects of chemical feedbacks on decadal
367 methane emissions estimates. *Geophysical Research Letters*, 47, e2019GL085706.

368 doi.org/10.1029/2019GL085706.

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

375 The derivations of equations describing the radiative forcing from hydrogen emissions are based
376 on Warwick et al. (2022), and other equations, including radiative forcing from CO₂ and
377 methane emissions and the temperature response function, are directly taken from previous
378 papers that are cited in our text. For example, we have added sentences that read:

379 *“We derive and apply the equations underlying the estimate of radiative forcing from hydrogen*
380 *emissions as presented by Warwick et al. (2022), relying heavily on parameter values from Ocko*
381 *and Hamburg (2022), Table S1. Equations describing the radiative forcing of CO₂ and CH₄ is*

382 *based on Myhre et al. (2013). The calculation of the global mean temperature response is based*
383 *on Gasser et al. (2017).”*