Michael Prather

I have high esteem for the authors of this Letter, they are good colleagues and friends, and they have decades of experience in quantifying the impact of different greenhouse gases (GHG). In my personal view, unfortunately, this Letter is mistaken and adds more confusion than elucidation to our understanding of atmospheric trace gases, their distributions, their lifetimes, and how we assess their climate impacts. The letter seems to explain a small mistake or confusion that was made by the authors in calculating the global warming potentials (GWPs) for the assessments. Thus, maybe it should appear as an IPCC erratum on /ipcc.ch rather than as a scientific advance in ACP, although maybe corrections do belong in ACP.

We thank Michael Prather for his kind remarks and his thoughtful review of our paper. We assert that this correction factor is well-founded and that its publication is necessary to inform the community when calculating climate metrics and also for calculating radiative forcings from mean atmospheric concentrations. This will be important for future IPCC Assessments and WMO Ozone Assessments, as well as individual scientific studies. We also assert, as detailed below, that the "confusion" extends way beyond what has been done in recent assessments

The authors present a corrected methodology for calculating the conversion factor CF relating the increase in atmospheric GHG burden (in Tg) to the mean tropospheric abundance (X, mole fraction with respect to dry air in ppb, parts per billion). This revision results in a set of %-corrections for the recent set of GWPs that the authors calculated in the first place. The authors should be commended for correcting the record.

The correction factors go beyond specific calculations by the authors. There is no documentation of such factors having been used in previous calculations of GWPs or other climate metrics. This uncertainty further motivates the need to publish this Letter to document the information for the community.

CF (Tg per ppb) is used with the Radiative Efficiency (e.g., RE in W m^{-2} per ppb) to calculate the radiative imbalance caused by an increase of 1 Tg in the atmospheric burden of the GHG (W m^{-2} per Tg). CF values have been published and used extensively throughout the IPCC to convert emissions scenarios (Tg) into tropospheric mean X (ppb). From this Letter, it appears that the GWP calculations have used different, incorrect CF values. The recent IPCC AR6 assessment, Table 7.15 in ref [0], does not list the CFs used in the GWP calculation, and this Letter also fails to list the corrected CFs so that they can be checked by others. The CF is a very simple quantity and can be simply verified from the observed or modeled distribution of the GHG in troposphere and stratosphere.

This Letter highlights that what the reviewer calls "CF" depends on whether the "ppb" refers to the mean atmospheric mole fraction or to the surface mole fraction. The formula based on the mean atmospheric mole fraction is indeed simple and is stated in equation 1. The key point of this Letter is that radiative efficiencies are defined according to the surface mole fraction and therefore a correction needs to be applied. We realise that the reviewer is familiar with this, but we are not aware that this point has been made in the literature, hence this Letter is important to communicate this.

The central question here is what "ppb" should we be using in CF and RE. For RE it obviously the abundance in the middle to upper troposphere since GHG in the near-surface layer have no thermal infrared radiative effect being indistinguishable from the surface, and GHG in the stratosphere are not so coupled to the troposphere-climate system (as recognized by the stratospheric adjusted RF, SARF). For CF, thus it should relate total burden to the mean

tropospheric abundance, since that is a quantity that we typically measure (e.g., NOAA's marine boundary layer GHG record) or model, and the one that should be used in the RE calculations.

It is this central question that our Letter seeks to clarify, whatever quantity the reviewer believes should "obviously" be (and has been) used. There is no doubt (two of the authors (OH and KS), have been centrally involved in their derivation and compilation for many years) that calculations of radiative efficiency (RE) to date (Etminan et al., 2016; 39 Freckleton et al., 1998; Jain et al., 2000; Myhre et al., 1998; Myhre & Stordal, 1997; Naik et al., 2000; Hodnebrog et al., 2020, 2013; Sihra et al., 2001, Acerboni et al., 2001, Sellevåg et al. 2004) have reported RE values per ppb of surface mole fraction. Hence this Letter explains the importance of using the same variable (surface mole fraction) in the calculation of the corresponding atmospheric burden, and notes that this has not generally been done. A recommendation of the Letter is that in future calculations of RE they should be additionally reported per atmospheric-mean mole fraction. Until this practice is adopted, the correction described in this Letter is needed to use RE values reported in the existing literature.

As a side comment, the fact that surface values have little impact on the RE is irrelevant as the methods used in its derivation implicitly take into account the altitudes of the gas that are most influential in its forcing

For well-mixed (tropospheric that is) GHG, we must account for the stratospheric fall-off in abundance. I am familiar with this approach since I derived some of the key values, including the CF used in the IPCC SAR & TAR [1]. The 2001 TAR conversion factors, 2.78 Tg-CH_4/ppb and 4.81 Tg-N(N_2O)/pbb, are used in sections 4.2.1.1 & 4.2.1.2, respectively. I was unhappy with these simplistic TAR numbers and pursued a more careful and quantitative evaluation of the CF (Tg/ppb) values in [2] based on a well-mixed troposphere (from NOAA remote surface sites) and the observed fall-off of stratospheric profiles. These updated 2012 [2] values were published as 2.75 Tg-CH4/ppb ±1% and 4.79 Tg-N(N2O)/ppb ±1% (68% confidence interval), which as expected are notably less than the values 2.84 and 4.96 derived from Equation (1) in the Letter and well outside the expected uncertainty. What is surprising is that based on the references in the Letter (2005 and 2013), one side of IPCC did not apparently know what the other side was publishing. In addition, the 2013 AR5 Annex II GHG Tables [3] use and explicitly reference the ref [2] methodology. These values are used in the scenario calculations by Meinshausen and the MAGICC code to convert Tg to ppb. It appears from the authors of this Letter that the GWP calculations since the TAR have assumed that all GHG are well mixed to the top of the atmosphere and now need to be corrected. It is good to see the GWPs 'corrected' but the Letter could note the problem with a better historical perspective and make sure the AR6 GWPs are updated on /ipcc.ch.

We appreciate the past work by the reviewer in this area and have absolutely relied on this as the basis for our calculations. The lack of clarity and consistency in the existing literature, notably recent IPCC Assessments, highlighted by the reviewer is a key motivation for this Letter.

There remain two fundamental disagreements with the approach and derivations presented here: (1) the incorrect use of surface abundance, X_srf; and (2) the derivation of lifetimes for short-lived (<0.2 yr) gases simply as a function of their chemical rates.

(1) The "ppb" used in both CF and RE should be the tropospheric mean, X_trop. The repeated use of X_srf in this Letter is simply wrong. No one seriously uses the mean surface abundance for anything because it is not observed nor modeled correctly. For one, X_sfc globally averaged is impossible to measure. Near source regions (and these GHG do have regions of intense surface emissions) one could never assemble enough observing sites to accurately measure the global

mean, and satellite remote sensing cannot really detect surface abundances. That is why NOAA and ALE/GAGE have put together a latitudinal range of remote marine boundary layer sites which are combined to derive a mean tropospheric abundance, X_trop. It is this X_trop that is quoted in the rest of the IPCC as the mean abundance for the GHG, not a mean surface value that would have to include averages over source regions. For two, we really cannot model the global mean X_sfc very well either. In a model like CTM2, this value depends on the model layers and the nighttime boundary layer, including the diurnal cycle of emissions that will control the large build up overnight. While our ability to model X_sfc is very limited, our ability to model and measure X_trop is solid and insensitive to boundary layers, diurnal cycles, etc.

We agree that it will be important to clarify that this is not a strict surface abundance, but a modelled value, however we do not accept that "no one seriously uses the mean surface abundance for anything" nor that tropospheric mean abundances are widely used. As examples, the NOAA Annual Greenhouse Gas Index https://gml.noaa.gov/aggi/aggi.html uses "annual-mean global surface mole fractions". The 2022 WMO Ozone Assessment states it uses "global mean lower-tropospheric mole fraction". AGAGE is quite clear (see Western et al., 2025, 10.5194/essd-17-6557-2025)that it reports surface mole fractions (see Section 5 of that paper). The values used in IPCC AR6 radiative forcing calculations (its Chapter 7) are referred back to Section 2.2.3 which states that "surface global averages are determined from measurements representative of the well-mixed lower troposphere". A point here is that if they are indeed reporting mean tropospheric abundance, as claimed by the reviewer, this is not clearly stated, nor is the methodology to derive this abundance explained. This would further strengthen the point in our paper that any such assumptions need to be made explicit.

The key argument in this Letter is that the correction factor should reflect that used in the calculation of the RE. For CH_4 and N_2O the most recent REs (Etminan et al. 2016) used profiles that were constant up to the tropopause (from Myhre and Stordal 1997), hence tropospheric and surface mole fractions are equivalent. For halocarbon REs (Hodnebrog et al. 2013 and 2020) model-derived profiles were used that varied throughout the troposphere and stratosphere. These REs were reported per surface mole fraction.

(2) Lifetime is defined correctly as total burden of the entire reservoir divided by source or sink. That definition is critical if we are to use the steady-state lifetime as an integral of the impacts of the emitted gas, see review and derivations in ref [4]. Lifetime depends on the kinetics of the chemical loss throughout the atmosphere, but it also depends on the location and time of the source. For example, in the derivation of the N_2O chemical feedback on lifetime [5], it is shown that emissions of N_2O at 40 km result in a lifetime of 2.5 years, rather than 120 yr for tropospheric emissions. These facts about lifetime are woefully and incorrectly propagated in this Letter. The authors assume that their corrections and lifetime adjustments for short-lived GHG (<0.2 yr) are meaningful and valid and can be used for GWPs. This assumption is simply wrong and can have very large errors. The burden that accumulates following emission, and the resulting GWP, will vary by large factors depending on where and in what season the gas is emitted. Tropical emissions will have a short lifetime, probably <0.1 yr, and small GWP (lower X_trop), but high latitude winter-time emissions will accumulate and probably have an effective lifetime >0.5 yr and correspondingly greater GWP. Consequently, one cannot scale ERF to emissions with a metric like GWP. For different example, recent studies of tropospheric O3 [6] show a lifetime ranging from 6 to 27 days depending on latitude and season of its production. This result means that the ERF of an O3 "source" has range of a factor of 4 or more, and cannot be simply "corrected" as done here. The idea of an RE (per ppb) is still mostly valid

but the CF is now widely variable. To be fair, we should not publish single GWPs for such short-lived GHG unless we deliver a range based on possible emission scenarios.

It is not the focus of this Letter to argue for or against GWPs for short-lived GHG. The facts on the ground are that these GWPs are already published and are currently used by industry and by legislators. For example, HFC-152, with its lifetime of 0.5 years, is included in the Kigali Amendment's list of controlled substances. We accept that there are multiple issues in computing reliable GWP values for such short-lived gases, but, since they already exist and are used, providing best estimates for these values clearly has utility beyond atmospheric science.

The current Letter could be revised in its discussion of surface abundances and lifetimes to avoid propagating old science. Then, collectively, we – as authors of the past IPCC chapters on trace gases and radiation – must accept our failure to communicate with one another.

We thank the reviewer for their advice and agree to clarify the discussion of surface mole fractions.

Chris Smith

This paper introduces an adjustment for AWGP that takes into account vertical profiles of greenhouse gases which are not uniform and a function of lifetime. It aims to relate radiative efficiency in W m-2 kg-1 surface emission terms to W m-2 ppb-1 atmospheric average terms, where the current treatment is to convert W m-2 kg-1 at the surface to W m-2 ppb-1 at the surface. The adjustment is required because the radiative transfer models used to calculate the W m-2 ppb-1 radiative efficiencies are calculated based on a whole-atmosphere average W m-2 ppb-1 value (possibly a constant vertical profile).

We thank Chris Smith for his thoughtful review of our Letter.

This has knock on effects for ratios of AGWP (i.e. the GWP, where the comparator gas is CO2) and related metrics like AGTP/GTP, with the general conclusion that not taking into account the vertical profile adjustment leads to an underestimation of GWP.

It's a short paper which I appreciate. I am convinced it is technically correct. However, it provides the potential to cause confusion. The authors suggest reporting both the GWPs based on surface and full-atmosphere concentrations (lines 126-129). I am not sure this would be well-understood by policymakers and has the potential to be misused. The question to ask could be "how significant are these changes in light of the overall uncertainty in certain metrics?" If the answer is not very, then are the current practices, though including this inconsistency, sufficient for policy purposes?

We do not recommend reporting two different GWPs as there is no change to the definition of GWP. Rather we recommend that studies that calculate the REs of gases report both the radiative efficiency per surface mole fraction, and per atmospheric-mean mole fraction (or atmospheric burden). We will ensure this is clearer in the revised text. While the error in the current misapplication of radiative efficiencies is of the order of a few percent, the error systematically underestimates the GWP (and other climate metrics). Hence it would be negligent not to correct it. We explain in the Letter that correcting this error could push the GWPs for some gases over critical legislative thresholds. It is up to policymakers to decide how to respond to this, but it seems preferable that they are aware of it.

Also, this further highlights the difficulty of attempting to distil multi-faceted climate effects into one number. Though this isn't the focus of the paper, related to this is why indirect effects are reported for some GHGs and excluded for others (and since the authors report CFC and HCFC values in their table 1, it is in scope). For example, tropospheric ozone formation and stratospheric water vapour formation for CH4 is included, but stratospheric ozone depletion for chlorinated gases isn't at least in headline figures (WMO2022 reports it in chapter 7 but the headline Annex A values do not). I understand the policy implications of quoting negative GWPs for CFCs, but the inconsistency is rarely mentioned.

As the reviewer comments, this Letter is not focussed on the utility or otherwise of the GWP (or other metrics). GWP100 is currently widely used in climate legislation and by other stakeholders and hence it is important that it is determined correctly. We completely agree about the inconsistency in the way indirect effects are sometimes included and sometimes excluded, but it is not impossible that, for example, the stratospheric ozone climate impacts of HCFCs could become more widely reported and included in legislation.

Specific comments:

Lines 15-17: are gases with species of less than one year that important anyway? Radiationally active short-lived forcers like SO2, CO and NOx are emitted in much larger quantities than these GHGs but we don't talk much about their direct GWPs (we rightly focus on their chemistry implications).

As in our response to above comments, the focus of this Letter is not to justify or otherwise the use of GWPs but to correct an error in the current calculations of their values. These GWPs are currently used by industry and by legislators. As just one example, HFC-152, with its lifetime of 0.5 years, is included in the Kigali Amendment's list of controlled substances. We accept that there are multiple issues in computing reliable GWP values for such short-lived gases, but providing best estimates clearly has utility beyond atmospheric science.

Line 39: are these assumed vertical profiles invariant? If emissions are increasing (sources exceed sinks), steady (sources equal sinks) or reducing (sources are less than sinks), does this change the profile and adjustment factor?

The corrections need to be derived from the vertical profiles originally used in the radiative transfer models to generate the radiative efficiencies. We will revise the text to make sure this is clear. It may be that those involved in radiative transfer modelling could investigate different profiles, but it is not within the scope of this Letter to recommend this. For existing literature, it is necessary to correct the radiative efficiencies based on the profile used. For future literature if the radiative efficiency per atmosphere-mean mole fraction were documented there would be no need for a correction.

Line 49: Hodnebrog et al. (2013) – please write down these equations somewhere.

We will add these equations.

Line 50: spatial (3D) or vertical (1D)? Spatial brings in new challenges. Where things are emitted matters and this will depend on regional socioeconomic interplays. For the shortest lived GHGs this is probably important.

We will add a mention of the dependence of the radiative efficiency on the location (and potentially the time of year) of the emission, but note that this does not affect the conversion factor from surface-mean to atmospheric-mean mole fractions.

Line 97: For gases with tropospheric and stratospheric loss pathways, does evidence suggest that using the larger of equation (3) and (4) the most appropriate or is there some combination based on inverse lifetimes? As a general point, as a practitioner who will be calculating GHG metrics for IPCC AR7, I would need the information for every GHG on whether the primary loss is stratospheric or tropospheric.

We agree that we need to clarify this and will add text to explain that this depends on which is the dominant loss term.

Figure 1b: This looks like it could very easily be an exponential – Is there a justification for choosing this shape as the curve fit?

In figure 1b the form is indeed based on the exponential form in equation 3. If the reviewer is referring to figure 1c then the form of equation 4 is based on those in Hodnebrog et al. 2020. We keep the same form here for consistency with that paper.

Equation 1 and surrounding text – to make the units balance, the 10^9 is strictly [ppb atm-1] and T_M is [kg atm].

We think the units we use here are correct since atm would have the dimensions of pressure. T_M is not mass per unit of atmospheric pressure, but mass of the entire atmosphere.

Line 102-103: similar question to line 39 on whether the profiles from Myhre and Stordahl, based on a 1997 atmosphere (or earlier), are still valid today. Getting these numbers correct for CH4 and N2O are important. Then the authors report that these numbers are less than expected from equation 3 (should it also be equation 4 for CH4?), so why are these two gases treated as special cases and why do they deviate from the trend? When plugging this into a generalised formula to calculate metrics which are often reported to three significant figures in IPCC, I would want another decimal place in the precision of these ratios (even if not justified by the uncertainty in them). An additional potential complication is that the radiative efficiencies of CH4 and N2O are not constant and are reported in Etminan for a present-day (at the time) concentration. The Etminan radiative forcing relationship was further updated by Meinshausen et al. (2020) that provided a closer fit to the Etminan radiative transfer results. How do I apply all of this?

The corrections need to be derived from the vertical profiles originally used in the radiative transfer models to generate the radiative efficiencies. We will revise the text to make sure this is clear. Hence for the Etminan/Meinshausen efficiencies the corrections should be derived from the Myhre & Stordal profiles. The correction factor does not depend on the functional form of the radiative forcing formulae, hence it can be applied to the radiative efficiencies starting at any preferred baseline and (for CH4 and N2O) using either the Etminan or Meinshausen formulae.. We will add a further decimal place as requested.

Line 117: Why is it fine to assume a constant vertical profile for CO2? We know that this isn't quite true. The CO2 should be adjusted similarly to the other GHGs. The authors extend the use case of adjusting radiative forcing for vertical profiles to reduced complexity climate models. Then we really start to think about how the models were run. Abrupt-4xCO2 is often used as a calibration experiment for simple climate models because it has a constant uniform forcing. Any ESM experiment running concentration driven would typically (with one notable exception I

know from CMIP6) specify a uniform atmospheric profile. Running CO2-emissions driven as many simple models would do, and some carbon-cycle enabled ESMs which would typically emit CO2 into the lowest atmospheric layer, would again result in this surface concentration/full atmosphere mismatch.

As in our replies to the other related comments, the corrections need to be derived from the vertical profiles originally used in the radiative transfer models to generate the radiative efficiencies. We will revise the text to make sure this is clear. For CO2 a constant profile with height has been used by all the studies calculating its radiative efficiency (at least, certainly for those reported by the authors of this letter and used in recent assessments). If a new study were to use a varying CO2 profile in the radiative transfer modelling then the radiative efficiencies per surface mole fraction they reported would need to be corrected unless efficiencies per atmospheric mole fraction were reported too.

Minor and stylistic comments:

13: climate scenarios

This will be changed

27: put equation in a new line and update the sentence structure so that this reads better.

This will be changed.