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Earth System Dynamics Discussions

Interactive comment on "The impact of model variation in CO_2 and temperature impulse response functions on emission metrics" *by* D. J. L. Olivié and G. P. Peters

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We first give a general response. It is followed by responses to the specific remarks of the reviewer.

General response

We would like to thank the reviewer for the constructive remarks.

We have modified the manuscript in different aspects, and describe these changes here.

In the revised version of the manuscript, we include data from the inter-comparison exercise Joos et al. (2012). During the preparation of our original manuscript, we were aware of this paper (one of us is a co-author), but preferred not to use the data as it was in review. We use the data from Joos et al. (2012) in the same way as the data from C^4MIP and LTMIP, and keep J07 (the IRF_{CO_2} used in Ramaswamy et al. (2007)) as our reference IRF_{CO_2}.

We compare our results with results from other studies, i.e., Reisinger et al. (2010), Reisinger et al. (2011), and Joos et al. (2012). To make this comparison easier, we now show 5- and 95-percentile values (instead of 10- and 90-percentile values), as used in Reisinger et al. (2010) and Reisinger et al. (2011). To present the impact on emission metrics, we now treat the horizon range from 20 to 500 yr continuously, not only the values at 20, 50, and 100 yr.

We acknowledge that the numbers in Figs. 3, 4, and 5 are difficult to read. We have replaced these figures, and the information is now presented in a different way. Figure 3 shows the (absolute) value of the metrics, while Figs. 4 and 5 show (i) the difference between the median of the metric distribution and the reference value, and (ii) the difference between the 5(or 95)-percentile value and the median. We do not show anymore the combined impact of variation in IRF_{CO_2} and IRF_T (originally shown in Fig. 5), as the spread is determined by the largest individual component. To allow the comparison of spreads caused by IRF_{CO_2} and IRF_T, we indicate the spread in GTP caused by variation in IRF_{CO_2} also in Fig. 5. We neither show the impact of variation in IRF_T onto the iGTP, as this impact is much smaller than the impact from variation in IRF_{CO_2}. We added two tables, containing the principal results for a time horizon of 100 yr.

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We explain in some more detail the numerical method used to estimate the parameters ("probabilistic inverse estimation theory"). For example, we mention the a priori values for the parameters we have used, and indicate that no correlation is assumed among the a priori parameter values, or among the results from the different years in the data from CC-models or AOGCMs.

We have considerably modified the introduction (Sect. 1). We hope to make the aim of the manuscript more clear, and additionally refer more to other work done in the field of metrics, i.e., Wuebbles et al. (1995) Tanaka et al. (2009), Reisinger et al. (2010), Reisinger et al. (2011), and (Joos et al., 2012). We think that there is now less overlap between Sect. 1 (Introduction) and Sect. 2 (Emission metrics and IRFs). Also the second part of Sect. 4, describing the impact of variation in $\mathrm{IRF}_{\mathrm{CO_2}}$ and IRF_T on the metric values, has been considerably modified.

Response to reviewer

The comments and remarks of the reviewer are written in *italic* font. The responses of the authors are written in standard font.

Overall comments

This study emulates various carbon cycle and climate models by using impulse response functions (IRFs) and investigates how the model differences lead to differences in emission metrics such as the GWP, GTP, and iGTP. The relaxation time scales and associated weights of the IRFs estimated in this study synthesize the results of several inter-comparison projects. The authors show a usefulness of IRFs to gain new insights into different models. This study is based on a substantial numerical work using data sets from several inter-comparison projects. However, I have several reservations with

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the manuscript as it stands.

1. *I would first suggest that the authors compare their results with those of previous studies (e.g. Joos et al. (2012); Reisinger et al. (2010); Wuebbles et al. (1995)). In the current manuscript the actual scientific contribution of this study to the literature is not very clear because the paper does not integrate previous studies in the discussion.*

   We have included the data from Joos et al. (2012) in the revised manuscript, and derive also an IRF_{CO_2} distribution for these data. Furthermore, we compare with results from Reisinger et al. (2010).

   - *Reisinger et al. (2010) is a major study that quantifies systematically the uncertainties in the GWP and GTP based on not only model differences but also historical constraints. This paper under review does not characterize the uncertainty by using historical observations. Reisinger et al. (2010) is touched upon in the introduction but deserves more discussion.*

     We now compare more thoroughly with Reisinger et al. (2010).

   - *In addition, the goal of this study looks similar to that of Joos et al. (2012), which is not cited in this paper. Joos et al. (2012) looks into the responses of various carbon cycle and climate models to CO_2 pulse emissions and discusses the influence of model differences on metric values. Because it appears that the same group is involved in this paper, the authors could provide in-depth comparisons between these two papers.*

     We now also use the data from Joos et al. (2012) in our study.

   - *Furthermore, Wuebbles et al. (1995) is also a relevant study that investigates the uncertainty in the GWP, which could be discussed in this paper.*

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Interactive Comment

We now include references to the Wuebbles et al. (1995) study.

2. *My second comment is related to the non-linearity. Applications of a linear IRF, which is used by the analysis here, are by construction valid within the linear range of the global carbon cycle (below about the CO_2 concentration of 550 ppm) (Hasselmann et al., 1993; Maier-Reimer and Hasselmann, 1987). Although the authors acknowledge the linear limit in the introduction, from my reading they actually fit linear IRFs on the C^4MIP output in which the models are run till 2100 (reaching 700 to 1000 ppm). I speculate this created a bias in the estimates of the C^4MIP IRF parameters. To extend the applicability of a linear IRF beyond its linear range, one needs to consider the dynamic equilibrium for the ocean carbonate species under rising atmospheric CO_2 concentration, which affects the ocean CO_2 uptake (Hooss et al., 2001). A detailed biogeochemical underpinning is provided in Tanaka et al. (2007). Or, a quicker fix in this case would be to fit the IRF on the C^4MIP data only till 2050, which is when the atmospheric CO_2 concentration does not substantially exceed 550 ppm.*

   The values of atmospheric CO_2 concentrations obtained in the second half of the 21st century in the C^4MIP simulations, are above the 550 ppm limit and could induce non-linear behaviour. However, the reason for the shape of the C^4MIP IRF_{CO_2} is mainly due to the exponential shape of the CO_2 emissions. It can be shown analytically that if the emissions increase exponentially, then the IRF_{CO_2} is essentially indeterminate. We explain this in more detail in the appendix at the end of this document.

3. *The IRF based on the C^4MIP dataset shows a nearly constant airborne fraction beyond just a few years after the emission (Fig. 1). This strong short-term response contradicts with the behaviors of the C^4MIP models and is also not consistent with the current understanding on the global carbon cycle (Archer*

et al., 2009). I think that the C^4MIP IRF requires further investigation. This problem may be caused by how the C^4MIP IRF has been calibrated (issue #2 above), but I am not sure what the reason exactly is. The paper attributes this peculiar behavior of the C^4MIP IRF to the rising emissions (Page 953, Lines 2–8). But I think that the rising emissions do not explain the short term response of the C^4MIP IRF – the rising emissions are more relevant to the uncertainty ranges of IRF parameters, as the conclusion of this paper states that "the gradual evolution of the CO_2 emission scenario in C^4MIP makes it difficult to uniquely determine the CO_2 IRF". Because the IRF accounts for multiple time scales of the carbon cycle response, the emission pathway should not influence the estimates of the IRF parameters (as long as it is applied within its linear range). Note that, if one attempts to estimate a single time constant (equivalent to an IRF with just one decaying constant), the emission pathway would influence the apparent CO_2 time scale (Archer et al., 2009). Related debates are summarized in Tanaka et al. (2012).

We agree that the behaviour of the obtained C^4MIP IRF_{CO_2} is very different from J07, LTMIP, and J12, and probably incorrect. We mention this now explicitly in the manuscript. However, we have preferred to maintain the C^4MIP results in the manuscript as it clearly illustrates that the quality of the IRFs can be strongly influenced by the type of the experiment. We refer to the end of this document where we make clear that in case of exponentially increasing emissions, different IRFs can give similar results.

4. *The current manuscript narrowly focuses on the IRF approach. I believe that adding some background discussions would broaden the perspective of the paper. Various types of models are used in computing metrics (Tanaka et al. (2010); see Figs. 1 and 2 for references therein). Why are the authors revisiting the linear IRF approach? What are the advantages of an IRF*

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over a (more complex) simple carbon cycle and climate model to probe the uncertainties in metrics? Why is the linear IRF in spite of its limitation for applications? These questions do not have to be the ones to be discussed in the paper, but I think addressing this type of broad questions would benefit the paper.

We do not claim that the use of IRFs is the best, or only, approach to calculate or study metrics: a non-linear IRF_{CO_2} as in Joos et al. (1996) and Hooss et al. (2001) will lead to a better representation of more complex CC-models. Our motivation for this work (as now clearly outlined in the modified introduction) is to look at how model variation affects metric values in common use. Those metrics are expressed in terms of IRFs. It is not our intention to explore model variations in other metric approaches. Despite this, the linear IRF approach can characterize and quantify a large part of the model difference that exists among different CC-models.

5. *The limitations of the metric results in terms of the type of uncertainties explored are discussed at the end of the paper, but those could be brought up upfront. It is not a problem that this study explores the uncertainties in metrics arising only from the model differences (i.e. without looking at those characterized by historical constraints). But the paper could state clearly at the beginning that this study does not fully explore the uncertainties in the CO_2 response because the focus of this study is the differences in the models used in various inter-comparison projects. It could also be stated at the beginning that the analysis does not consider the uncertainties related to non-CO_2 components. It is not clear how these unaccounted uncertainties would play out and affect metric ranges. Furthermore, it may be worth pointing out the importance of the time horizon – as the metric results show implicitly, the choice of the time horizon in many cases influences more strongly the metric values than the choice of models.*

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We have taken this comments into account and moved some of the considerations on the uncertainties to the introduction part of the manuscript.

I brought up several issues with the current manuscript above. However, the paper will potentially be an interesting contribution to the literature. As a final remark, I felt that there is a room for improvement in terms of the presentation of this paper. It is my overall impression that the paper (including the abstract) can be shortened by improving the wording, polishing the text, removing redundancies, and etc. Also note that, because of the issue #2, which might significantly affect the metric estimates, I did not review the part dealing with the results for metrics (Sections 4.2 to 4.4). I have detailed comments (see Supplementary pdf).

Detailed comments

Page 936, Lines 3-4 : *It is unclear what this sentence exactly means.*

With "single" we meant that the IRF_{CO_2} as used in Ramaswamy et al. (2007) is based on only one model (Bern2.5-CC), and that studies like Boucher and Reddy (2008) and Fuglestvedt et al. (2010) use an IRF_T based on only one AOGCM.

Page 937, Lines 3-5 : *I am not very convinced by the need to develop further the inter-comparison exercise dedicated to CO_2 and temperature IRFs at this stage. An IRF inter-comparison project has just been completed (Joos et al., 2012).*

We now include the results from Joos et al. (2012) in the manuscript as one of the data sets used to derive an IRF_{CO_2} distribution, and have modified this sentence.

Page 937, Line 7 : *Comparing the global climate impact does not necessarily require*

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an emission metric. This sentence can be revised.

We have modified this sentence.

Page 937, Line 16 : *The idea of the MGTP is first proposed by Gillett and Matthews (2010). The iGTP is equivalent to the MGTP. The original paper also needs to be cited.*

We now also cite the study by Gillett and Matthews (2010).

Page 937, Lines 16-18 : *This sentence can be revised to reflect the fact that the GWP is by far the most frequently used metric not only in research but also in climate policies such as the Kyoto Protocol.*

We have modified this sentence.

Page 937, Line 19 : *A few sentences to introduce what an IRF is would be helpful for the readers, I believe.*

We have modified the introduction, aiming also to better explain the aim of IRFs.

Page 938, Lines 1-2 : *This is not correct. From the carbon cycle side, non-linear IRF approaches have been put forward (Hooss et al., 2001; Joos et al., 1996).*

We mention in the revised manuscript that non-linear IRF approaches have been proposed (Hooss et al., 2001; Joos et al., 1996).

Page 938, Line 9 : *The CO_2 fertilization has been introduced earlier (Page 387, Lines 5-7). But the temperature effect on soil respiration is not pointed out, which is a major component of climate-carbon cycle feedbacks.*

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We now mention also the temperature effect on soil respiration.

Page 938, Line 11 : *Reisinger et al. (2011) and Tanaka et al. (2009) have also looked into this.*

We have added these references.

Page 938, Line 13 : *Joos et al. (2012) has also shown this.*

We mention this study in the revised version of the manuscript.

Page 938, Lines 18-19 : *I would rather think that the authors assume a single time scale to apply the IRF concept for the non-CO_2 components. Here a clarification is needed for the non-linearities involving CH_4 in particular. The OH chemistry influencing the CH_4 adjustment time is taken into account as the indirect GWP (Section 6.12.3.1 of IPCC (2001)).*

We describe now in Sect. 2 how the non-linearities related to CH_4 are taken into account.

Page 938, Lines 22-23 : *An alternative is to replace a linear IRF with an energy balance model (compare Bruckner et al. (2003), Tanaka et al. (2007)).*

Yes, we realize this. Our intention is to compare with common emission metrics (GWP, GTP, etc) that are based on linear IRFs. This is explained more clearly in the revised introduction.

Page 938, Line 25 : *For clarification, the saturation effect for CH_4 and N_2O is also strong and considered in the respective parameterizations (Table 6.2 of Ramaswamy et al. (2001)).*

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In the original version of the manuscript, we only described the spectral overlap between CH_4 and N_2O. Now we additionally mention the saturation effect which explains the non-linear dependence on the atmospheric concentration (Ramaswamy et al., 2001, Table 6.2).

Page 938, Line 27 : *Table 6.2 can be directly referenced.*

We modified this in the revised version of the manuscript.

Page 938, Line 29 : *The size dependency is also due to the non-linearity in the carbon cycle.*

We agree with the reviewer. In the revised version of the manuscript we clearly make the link between the dependence of the IRF on the pulse size and the non-linearity of the carbon cycle.

Page 939, Lines 23-24 : *I think that an original purpose to compute an IRF is to avoid running the AOGCM, which is computationally expensive.*

In the original version of the manuscript, we stated that "the derivation of IRF suffers from the fact that AOGCM simulations are computationally expensive". As the reviewer points out, it is maybe better to formulate this in another way, stating that "the purpose of an IRF is avoiding running an AOGCM". We state this now in this way in the revised version of the manuscript.

Page 940, Lines 11-16 : *Related the issue #5, I suggest that the discussion also touches on the limitation of this analysis and clarify what types of uncertainties are considered in the metric results (compared to Reisinger et al. (2010)).*

In the revised version of the manuscript, we clearly describe which type of

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uncertainties we accounted for. We also clearly state which uncertainties have not been taken into account, compared with other studies.

Page 941, Lines 7-8 : *Related to my earlier comment (Page 938, Lines 18-19), I do not think that one always imposes a single time constant on the models of all the non-CO_2 components (for example, see Reisinger et al. (2010); Tanaka et al. (2009)).*

We agree with the reviewer. E.g., Tanaka et al. (2009) and Reisinger et al. (2010) have simple descriptions of the atmospheric chemistry, allowing to describe the dependence of the CH_4 removal rate on the OH burden and the temperature, and possibly the dependence of the removal rate of N_2 O on stratospheric O_3.

Page 942, Line 8 : *See my earlier comment (Page 938, Line 27).*

Ok.

Page 943, Line 8 : *Hooss et al. (2001) also finds n=2.*

We have added this reference. Additionally, we added for the case $n = 1$ the reference Shine et al. (2005).

Page 948, Line 8 : *Why is "total climate sensitivity" rather than "equilibrium climate sensitivity"?*

λ is the "equilibrium climate sensitivity". As in our analysis it is expressed as the sum of two contributions (f_1 and f_2), we called it the "total climate sensitivity".

Page 948, Lines 10-12 : *Are the climate sensitivity estimates used as prior estimates or*

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directly prescribed to the IRFs?

The climates sensitivity constraint is directly prescribed to the IRFs. In the IRFs, f_2 is replaced by $f_2 = \lambda - f_1$, where λ is constant, and then the optimization is done with only 3 parameters, i.e., τ_1, f_1, and τ_2. We explain this now in the revised version of the manuscript.

Page 949, Line 11 : *It is stated that an inverse estimation technique of Tarantola (2005) is used for the estimation of the IRF parameters, but I wonder how it is actually applied and what the specific assumptions are. For example, what are the prior for the IRF parameters? Is there any correlation assumed between the IRF parameters? How does the objective function look like? To use the inversion theory of Tarantola (2005) in the context of optimization, Tanaka et al. (2007) extract and discuss relevant assumptions.*

In the revised version of the manuscript, we give more specific information on the method used to estimate the parameters of the IRFs: a priori values of the parameters, the correlation between these a priori values, and the correlation between the observations (i.e., the results from the CC-models or AOGCMs).

Page 950, Lines 12-13 : *Why is a delta-pulse experiment more difficult for climate models than carbon cycle models?*

Based on the definition of an IRF, one can imagine an experimental setup, such that the resulting time evolution directly gives the IRF_{CO_2}: one emits 1 kg of CO_2 at a certain point in time. However, an experiment with an AOGCM giving as response an IRF_T, would ask for an infinite pulse in radiative forcing. As imposing an infinite pulse is technically and physically very challenging, one prefers imposing a step-forcing (often one takes as amplitude 3.71 $W\,m^{-2}$ or 7.42 $W\,m^{-2}$). In case of a linear system, the response will then be the integral of the IRF_T.

Page 953, Lines 27-29 : *How strong does this discrepancy in the long-term constants affect the metric values analyzed in this paper (with a time horizon of < 100 years)?*

Up to 100 yr, there is already a strong variation in GTP for BC (−50 %/+80 %) or CH_4 (−20 %/+50 %). The impact on iGTP is much smaller.

Page 954, Lines 6-8 : *Why is there a substantial difference between the climate sensitivity estimated in this study and the one in IPCC (2007)?*

We refer therefore to Olivié et al. (2012). In IPCC (2007), the climate sensitivity was estimated based on an experiment with an AGCM coupled to a Mixed-Layer Ocean model. The climate sensitivities as estimated in this study are based on short transient simulations, for which it is rather hard to obtain an estimate of the climate sensitivity when studying only the temperature response. This is because the temporal behaviour of those experiments can be well described by a small climate sensitivity combined with a short time constant as well as by a large climate sensitivity combined with a large time constant.

Page 962, Lines 13-14 : What are the examples of metrics comparing two non-CO_2 species?

We suggest the absolute metrics AGWP, AGTP, and iAGTP. In anthropogenic activities where there is a trade-off between CH_4 and N_2O emissions, it might be relevant to study $AGWP_{CH_4}/AGWP_{N_2O}$.

Page 962, Lines 16-17 : *I would think the other way. This study shows that the IRF parameters for a long time scale are less well constrained due to the limited length of model runs available. Thus, results with a time horizon of 500 years* *would not easily be obtained. Even if obtained, one would need to take it with caution.*

We intended to say that the expressions for the IRF distributions allow in principle analysis for all possible time horizons. However, as the reviewer points out, uncertainties are considerably larger for long time horizons. This sentence is not anymore present in the new version of the manuscript, as we now treat horizons in the range of 20 to 500 yr.

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Appendix: Deriving IRFs in case of exponentially increasing CO_2 emissions

The IRF

Imagine that $IRF_{CO_2}(t)$ is the IRF_{CO_2}. This IRF must fulfil some conditions. One condition is

$$0 \leq IRF_{CO_2}(t) \leq 1 \tag{1}$$

for all $t \geq 0$. Another condition is that

$$\frac{\mathrm{d}}{\mathrm{d}t} IRF_{CO_2}(t) \leq 0\,. \tag{2}$$

An additional condition might be that

$$IRF_{CO_2}(0) = 1\,, \tag{3}$$

but at the moment we do not impose this.

The emission

We assume an exponentially increasing emission with a time scale τ_e. So the emission can be written as

$$E(t) = E_0 \exp \frac{t}{\tau_e}\,. \tag{4}$$

For simplicity, we assume that the emission has started already at $t = -\infty$.

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The burden

The atmospheric burden $B_{CO_2}(t)$ can be expressed as

$$B_{CO_2}(t) \quad = \quad \int_{-\infty}^{t} IRF_{CO_2}(t-t')\, E(t')\, \mathrm{d}t' \,.$$

For general functions f and g (with maybe some conditions on integrability), one has the equality

$$\int_{-\infty}^{t} f(t-t')\, g(t')\, dt' = \int_{0}^{\infty} f(t')\, g(t-t')\, dt' \,. \tag{5}$$

In our case $g(t)$ is the exponential increasing emission $E_0 \exp \frac{t}{\tau_e}$ and $f(t)$ is the general $IRF_{CO_2}(t)$. The burden will always be proportional to the exponential emissions, whatever the IRF_{CO_2} is, as

$$\begin{aligned} B_{CO_2}(t) &= \int_{-\infty}^{t} IRF_{CO_2}(t-t')\, E_0\, \exp \frac{t'}{\tau_e}\, dt' \\ &= \int_{0}^{\infty} IRF_{CO_2}(t')\, E_0\, \exp \frac{t-t'}{\tau_e}\, dt' \\ &= E_0 \int_{0}^{\infty} IRF_{CO_2}(t')\, \exp \frac{t}{\tau_e}\, \exp \frac{-t'}{\tau_e}\, dt' \\ &= E_0 \exp \frac{t}{\tau_e} \int_{0}^{\infty} IRF_{CO_2}(t')\, \exp \frac{-t'}{\tau_e}\, dt' \,. \end{aligned} \tag{6}$$

This shows that the evolution of the burden is also exponential with a time constant of τ_e. So

$$B_{CO_2}(t) = E_0\, Q\, \exp \frac{t}{\tau_e}, \tag{7}$$

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where we define Q as

$$Q = \int_0^\infty IRF_{CO_2}(t') \exp \frac{-t'}{\tau_e} dt' \,. \tag{8}$$

This implies that any other IRF_{CO_2} which gives the same value of Q, will lead to an identical evolution of the burden. Below we investigate some different functional forms, leading all to the same value of Q.

Exponential approximation for the IRF

Imagine that we want to approximate the IRF_{CO_2} with terms $IRF_i(t)$ like

$$IRF_i(t) = a_i \exp \frac{-t}{\tau_i} \,, \tag{9}$$

then the contribution $B_i(t)$ to the burden can be calculated,

$$\begin{aligned}
B_i(t) &= \int_{-\infty}^{t} IRF_i(t-t')\, E(t')\, \mathrm{d}t' \\
&= \int_{-\infty}^{t} a_i \exp \frac{-(t-t')}{\tau_i} E_0 \exp \frac{t'}{\tau_e} \mathrm{d}t' \\
&= a_i \exp \frac{-t}{\tau_i} E_0 \int_{-\infty}^{t} \exp \left(t' \left(\frac{1}{\tau_i} + \frac{1}{\tau_e} \right) \right) \mathrm{d}t' \\
&= a_i \exp \frac{-t}{\tau_i} E_0 \frac{1}{\frac{1}{\tau_i} + \frac{1}{\tau_e}} \exp \left(t \left(\frac{1}{\tau_i} + \frac{1}{\tau_e} \right) \right) \\
&= a_i\, E_0 \frac{1}{\frac{1}{\tau_i} + \frac{1}{\tau_e}} \exp \frac{t}{\tau_e} \\
&= a_i \frac{1}{\frac{1}{\tau_i} + \frac{1}{\tau_e}} E(t)
\end{aligned} \tag{10}$$

Different options are now possible, dependent on the number of modes one uses.

- **One mode** With one mode, this would imply that one has to choose a_1 and τ_1 such that

$$Q = \frac{a_1}{\frac{1}{\tau_1} + \frac{1}{\tau_e}}, \tag{11}$$

i.e., one has one condition for two unknowns (a_1 and τ_1). Often we want that $a_1 = 1$ and than also τ_1 can be deduced. Another option would be to take $\tau_1 \rightarrow \infty$, and than we deduce a_1, which can be different from 1.

- **Two modes** With two modes, the condition would be

$$Q = \frac{a_1}{\frac{1}{\tau_1} + \frac{1}{\tau_e}} + \frac{a_2}{\frac{1}{\tau_2} + \frac{1}{\tau_e}}, \tag{12}$$

where it is natural to impose that $a_1 + a_2 = 1$. This allows still a large degree of freedom in the choice of the parameter values, as we have two conditions and four variables to estimate: a_1, τ_1, a_2, and τ_2.

- **More modes** Using more modes will increase further the degrees of freedom.

Piecewise linear approximation for the IRF

We assume an IRF of the form

$$IRF(t) = a_1 \left(1 - \frac{t}{\tau_1}\right) \quad \text{for} \quad 0 \leq t \leq \tau_1\,,$$

and 0 elsewhere. When one calculates Eq. (8), one finds that

$$Q = a_1 \left(\tau_e + \frac{\tau_e^2}{\tau_1}\left(\exp\frac{-\tau_1}{\tau_e} - 1\right)\right) \tag{13}$$

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which is again one condition for two variables (a_1 and τ_1). One can choose $a_1 = 1$, which then allows to determine τ_1. In the limit $\tau_1 \rightarrow \infty$, the solution approaches a constant function: $a_1 = \frac{Q}{\tau_e}$. This is the same limit as one would find in the exponential case.

A Dirac δ-function as approximation for the IRF

Imagine that this function is defined as

$$\int_{-\infty}^{\infty} \delta(t)\,\mathrm{d}t = 1\,, \tag{14}$$

where we see it as the limit of a triangle which lies completely at the right hand side of the Y-axis. So we suggest as solution $A_1\,\delta(t)$. This leads to the condition

$$Q = A_1\,. \tag{15}$$

Conclusion

An infinite number of different IRFs can perfectly fit the case of an exponential increasing emission. Some of these IRFs are:

- a constant function, but then one cannot expect that $a_1 = 1$.
- one decaying exponential mode, with initial amplitude 1 ($a_1 = 1$), and where the time constant is determined by the relation in Eq. (11).
- sum of two decaying exponential modes, with the sum of their amplitudes equal to 1: $a_1 + a_2 = 1$. Even with this extra condition, this gives already an infinite number of possible combinations.

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- a piecewise linear function.
- a Dirac δ-function.

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