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Geoscientific Model Development Discussions

Interactive comment on "FAIR v1.1: A simple emissions-based impulse response and carbon cycle model" *by* Christopher J. Smith et al.

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Dear William,

Thank you for your time taken in your detailed and mostly positive review of the manuscript. Original comments are given in bold, which are responded to point-by-point in regular font.

General comments

This paper covers two topics, the description of a simple climate model and

calculation of observationally-constrained climate sensitivity. This may be doing too much in one paper. The question of climate sensitivity is long-running and of high importance, so if the authors believe they have new insight into this it would deserve its own paper with a title that reflects this and probably not in a model development journal. Conversely if the climate sensitivity calculations are intended more as an illustration of the FAIR model then much of the detail is overkill. My review will be more focussed on the model development aspects of the paper.

Again, thank you for the time spent in reviewing the manuscript. We appreciate the point that the second part of the paper which focuses on the application of FAIR to constrain climate sensitivity may be additional detail over and above the description of the model. However, we believe it is important to show how FAIR can be useful to the climate modelling community by demonstrating its application to well-understood scenarios (the RCPs). In fact, reviewers for a paper based on this model have asked for this evidence. Since ECS and TCR must be supplied to the model, and a constraint must be applied to ensure that each combination of model parameters results in plausible output (the constraining process), it is not much of a stretch to use these results to show which input parameters of ECS and TCR result in plausible output, and provides additional confidence that the model is providing sensible results.

The FAIR model has the potential for being a very useful tool that could be widely used. Therefore the authors need to take care that it is constructed in such a way as to be generally useful and not just for RCP scenarios. For instance it should be set up to be able to take in CO2-only emissions rather than having to subtract the non-CO2 effects from RCP8.5. The paper needs to be clear as to whether this is a model that is suitable for use by the wider community yet.

Thank you for this positive comment. It is possible to run FAIR with CO_2 emissions only, where forcing from non-CO_2 agents can be specified optionally. For the TCRE assessment we now do this. As the CO_2-only treatment has already been described

in Millar et al. (2017), we do not focus on this particular implementation in the present paper.

We claim that FAIR is useful for a wide range of scenarios other than the RCPs and has been designed to evaluate a wide range of emissions commitment scenarios. As the RCPs are familiar to many and span a wide range of future projections, we use them in this paper to evaluate the performance of FAIR.

For a few of the forcing agents (e.g. aircraft, land use) there is a convoluted methodology to recreate the original activity data from the RCP emissions. A tool like this should be designed to take activity data as its basic input. It is fine for this paper if the authors have recreated the activity data from the RCP in this instance to test the model, but if the FAIR tool were to be used in an aircraft or land use study it doesn't make sense to have to generate NOx or CO2 emissions from the activity data so that FAIR can then invert them to get back to the original activity data.

Thank you for this important suggestion. We agree that starting from the base activity data would be more useful in order to compare impacts. However, the model is designed to be as simple as possible, and where possible to use only the 39-species emissions data appearing in the RCPs. We argue that our simple treatment of land use forcing satisfies this simple treatment while still producing a plausible time series of ERF (noting the uncertainties in land use ERF are so wide that not even the sign of the forcing is known with confidence). See also responses below and our response to the other reviewer on this point.

For the RCP scenarios, aircraft activity data does not appear to be easily available, so we do not have any data on which to train the model to. We therefore specify the (time-varying) fraction of total NOx emissions that are due to aviation as a proxy for contrail forcing. This can be calculated from the RCP databases for RCP scenarios and is included as an option in the model where the user can use a specified RCP

aviation NOx fraction (or provide their own). We argue that this is an improvement over MAGICC, which does not include contrail forcing (up to 0.5 W m^{-2} in RCP8.5 at the 95% level). Including aircraft activity data would be a valuable future improvement.

It is entirely inappropriate to use the AR5 ozone and aerosol ERF time series to back out the response coefficients by linear regression. These time series were generated by a few models (it may only have been GISS) that ran forward to generate ERFs. These time series were intended to illustrate the evolution of the ERFs, not as the last word. These are not the time series that were used to force any of the CMIP5 GCMs, nor the forcings diagnosed from CMIP5 (apart maybe from GISS). Hence the ability or not to recreate the AR5 time series using FAIR is meaningless since none of the GCMs used these. Even if these time series had been more rigorously generated it is not sensible to use linear regression to derive the response coefficients as the covariances are so large. I suggest using Stevenson et al. 2013 and Aerocom to derive response coefficients. Whichever method is used, the coefficients need to be listed in tables.

This is another important point. FAIR v1.1 was designed initially to convert as best as possible raw emissions data into an ERF time series that is used to calculate temperature change for assessing a wide range of global emissions scenarios. The AR5 Annex II time series was used to correlate RCP emissions. Obviously, many forcing and emissions components do not follow linear or other simple analytical relationships, but where possible we had determined relationships that approximately track the AR5 historical ERF time series for each forcing component.

Following your comments we have implemented the ozone forcing treatment from Stevenson et al. (2013), direct aerosol forcing treatment from Aerocom (Myhre et al., 2013a), and a representation of the aerosol indirect effect from the simplified model of Ghan et al. (2013), informed by the aerosol indirect effect treatment in Stevens (2015). These changes result in different future forcing and temperature time series to v1.1, in particular a warming of around 0.5 K in RCP8.5 in 2100 compared to the old treatment

owing to the increase in tropospheric ozone and (decrease in negative) aerosol forcing in the new relationships. We feel that these changes are substantial enough to warrant an increment of the model version number, so the model version in this paper is now v1.2.

Since our modified aerosol treatment is quite similar to the model of Stevens (2015) (although with more predictor variables), we remove the Stevens aerosol relationship from the sensitivity analysis in section 5.

Specific comments

Page 2, line 14: Ocean sinks will become less effective too. Is this accounted for in FAIR?

Thank you for pointing this out – this is actually a small typo, and should read "land and ocean carbon sinks".

Page 2, lines 27-30: IPCC merely used the carbon cycle responses from Joos et al. 2013 rather than constructing anything new. The Joos et al. responses were in turn taken from fits to C4MIP so would have included any feedbacks for biospheric uptake and temperature inherent in C4MIP models.

We agree that the original statement was misleading, which was meant to imply that the Joos relationship as it is used in AR5 does not include any carbon or temperature feedbacks because all calculations are performed against a background concentration of 389 ppm. As you correctly suggest the original relationships in Joos et al. do include state-dependent feedbacks. We have changed "with no feedbacks assumed for biospheric carbon uptake or temperature" to "where time constants were taken from a multi-model intercomparison of full- and intermediate-complexity earth system models (Joos et al., 2013) with no feedbacks assumed for biospheric carbon uptake or

temperature".

Page 3, line 7: Replace "validated" with "calibrated"

We agree this is more appropriate terminology. Manuscript updated.

Page 3, line 11: It is not quite clear what "expected to be smoothed out in the global mean." Is trying to say. Obviously the global mean is an average of the regional variations by definition.

On reflection this sentence is superfluous and has been removed. We tried to highlight that we acknowledge that trying to account for a forcing mechanism in one global mean number may hide substantial localised variability, but this is implied from the zero-dimensional model described.

Page 4, equation 1: State that Ri are masses in kg.

Resolved in revised manuscript.

Page 5, equation 4: State that Ct are molar mixing ratios. Equation is missing a factor δ_t.

δ_t is 1 (at least in this model version) so was omitted but has now been added in for completeness. C_t has been explained.

Page 5, lines 3-5: The natural emissions in fig 2 look very unrealistic. What do MAGICC natural emissions look like? Do they have a different way of addressing this?

Originally, MAGICC natural emissions (for e.g. methane) are estimated by balancing the budget for the change in concentration, minus the contribution from anthropogenic emissions from the following relationship (see Meinshausen et al. (2011a) and the MAGICC Wiki page). The exact details do not appear to be straightforward.

We initially estimated natural emissions by adjusting them to approximate the observed

concentration time series over the RCP historical timeframe. However we have now taken a more accurate approach and formally backed out natural emissions by taking the difference in observed concentrations, accounting for atmospheric sources and sinks, and emissions, to obtain the natural contribution over the historical period. The future natural emissions are held fixed at their 2005 values. Figure 2 in the manuscript has been updated.

Page 5, lines 19-25: The methane lifetime is a function of methane concentration and this dependence is not difficult to implement, see eg MAGICC description or IPCC TAR 4.2.1.1. For increasing emissions the concentrations increase more rapidly than for a constant lifetime. This probably explains the discrepancy in the methane for RCP8.5 in fig 4(b).

We agree with the reviewer on this point, and we actually implemented a variable methane lifetime from Holmes et al. (2013) originally in the development process. However there are several reasons why we did not proceed in the final version:

1. The future emissions scenarios are uncertain, and a discrepancy of 5% with MAGICC for 2100 in RCP8.5 is not critical compared to the 25% error in methane forcing between Etminan et al. (2016) and Myhre et al. (1998), the latter of which we believe is used in MAGICC. Also using a sophisticated methane lifetime relationship such as that in Holmes et al. (2013) results in a deviation from MAGICC as large (in the other direction) as using a fixed lifetime for RCP8.5 in 2100.

2. There is no guarantee that the relationships that hold for small departures from present-day concentrations would apply to the more than $2\times$ increase in methane concentrations projected in MAGICC for 2100; see for example the diversity in methane lifetime and equilibrium concentrations for the NOx attribution experiment in Stevenson et al. (2013, Table 7)

3. In the context of this work, some level of natural emissions have to be assumed

post-2005 to balance the historical time series up to 2005, and it is not clear what treatment has been applied in the RCPs post-2005. To illustrate this we have experimented by implementing a simple methane feedback lifetime (where a 1% change in concentrations results in a 0.32% change in lifetime, the best estimate given in AR5), starting with an unrealistically short lifetime of 7 years in the pre-industrial in order to approximate 9.3 years in 2005, with no dependence on other effects (Holmes et al. (2013) show that methane concentration is the most important single effect on methane lifetime, even if overestimated). This results in the time series of natural balancing emissions in fig. 1 below for RCP8.5. Natural emissions of methane become negative before 2100 even under this low estimate of methane lifetime which is probably implausible; for more realistic pre-industrial and present day lifetimes, the natural emissions become more negative.

We don't rule out returning to the question of a varying methane lifetime in future model versions, and agree with the reviewer that this would be more satisfying. In our experience so far however, the additional complexity is not only unjustified but leads to unlikely methane concentrations being returned.

Page 6, section 2.1.3: This section needs an explanation of how to avoid double counting as the CO_2 emissions are often based on the total fuel consumed rather than specifically how much is fully oxidised all the way to CO_2.

We take the same approach as in MAGICC and do not assume that the CO_2 emissions inventories include oxidation of fossil sources of CH_4 (Meinshausen et al. (2011a, eq. A1)). The RCP scenarios, derived from MAGICC originally and also used for evaluation of FAIR, are based on total fuel consumption as detailed in Meinshausen et al. (2011b), using data from Marland et al. (2008), which in turn uses the method of Marland and Rotty (1984). Although not explicitly stated, following the argument in Marland and Rotty (1984) implies that natural gas that is not burned, incompletely combusted, or used for ammonia production accumulates as methane in the atmosphere.

Thus a molecule emitted as methane and not combusted is counted as methane until it is oxidised to CO_2. The atmospheric sink provides a mechanism for removal of methane. We have added the following sentence:

"As atmospheric methane concentrations are reduced by exponential decay in eq. (5), this prevents against (approximately) double-counting an emitted fossil-fuel CH_4 molecule as both CH_4 and CO_2 after it has been oxidised."

Page 6, line 14: Myhre et al. 2013b did not show that ERF agrees with RF, rather they found that there had not been sufficient research to determine whether the ERF was different to RF. As the authors are well aware the PDRMIP project amongst others has compared RF and ERF more recently.

This sentence has been changed:

"Although Etminan et al. (2016) calculate RF, Myhre et al. (2013b) concluded that over the industrial era there was not sufficient evidence to state that ERF was significantly different from RF for these three gases, and ERF is taken to equal RF, although with a doubled uncertainty range."

Page 7, section 2.2.2: Use "well-mixed greenhouse gases" to exclude ozone.

Included in revised version.

Page 7, section 2.3.3: This linear regression is not an appropriate way to derive the response coefficients since the historical emissions strongly co-vary. Deriving a negative correspondence with NMVOC is not merely an interesting detail, it is physically wrong and so undermines the whole procedure. This must also mean that some or all of the other coefficients are overestimated to compensate. While this method may give acceptable agreement for the RCP scenarios in fig 5(e) it would give incorrect predictions when applied to more idealised scenarios e.g. if the FAIR tool were used to assess the climate impact of biomass stoves. There are sufficient data in Stevenson et al. 2013 to be able to derive more phys-

ically credible coefficients. The coefficients need to be provided in a table and compared with other studies.

Following these comments we have re-introduced the Stevenson et al. (2013) relationships, and they are tabulated in table 4 in the manuscript. To be consistent with Stevenson et al. (2013) and the natural emissions from Skeie et al. (2011), recognising that anthropogenic emissions in 1750 were not zero, we implement a different coefficient set for RCP scenarios before 1850.

Page 8, section 2.2.5. The AR5 value assumed stratospheric water vapour added 15% of the Myhre et al. 1998 methane RF. It would add a lower percentage of the Eminan et al. ERF.

You are correct in that the upward revision from Etminan et al. (2016) for methane forcing would cause an upward increase in stratospheric water vapour forcing. This was detectable in fig. 5g. In v1.2, the model has been updated so that the scaling relationship depends on the forcing option used. We change the default value to 12% of the methane forcing reflecting that the ratio of Myhre et al. (1998) to Etminan et al. (2016) methane forcing is approximately 4/5. The actual scaling factor can be overridden by the user. The impact is actually fairly small at about 0.02 W m^{-2} in the present day.

Page 8, section 2.2.6: It is dangerous to build in this back calculation of aircraft activity into a tool. It is much safer to use activity data as the input. If the authors have chosen to back activity data out from RCP datasets for the purpose of this paper that's fine, but it shouldn't be hidden within the tool.

We appreciate your concern with the treatment of contrail radiative forcing. To our knowledge, the aircraft activity figures going into the RCP datasets are not readily available so could not be used for this particular instance. Additionally, the contribution to total emissions from aviation are not available for most species; NOx is one of the few where this is available. In any case, the aviation NOx fraction is specified by the

user as a time series or constant, and the RCP values can be pre-loaded from within FAIR. We agree that an option to specify aviation activity would be a useful future development.

Page 8, section 2.2.7: As with ozone, linear regression is not an appropriate way to derive the response coefficients. Using speciated RFari forcing from AR5 and Aerocom to divide up the total ERFari+aci is a more transparent method. The coefficients need to be provided in a table and compared with other studies.

We agree that the old treatment may have been deficient so have updated the treatment of aerosol forcing for v1.2. Aerocom only includes the direct aerosol effect so we need a separate treatment of the indirect effect. For this we use a curve fit to the model in Ghan et al. (2013) which depends logarithmically on emissions of SOx and OC+BC. For this we have borrowed the functional dependence of aerosol forcing on SOx emissions from Stevens (2015). This is detailed in the re-written section 2.2.7 and in the supplementary material.

Page 9, section 2.2.9: Again, it is dangerous to build in this back calculation of land use activity into a tool. It is much safer to use activity data as the input. If the authors have chosen to back activity data out from RCP datasets for the purpose of this paper that's fine, but it shouldn't be hidden within the tool. The forcing is missing a minus sign.

The missing minus sign has been included. Thank you for spotting this omission.

We wanted to make FAIR simple, and as far as possible able to derive forcing and temperature from a single RCP-style emissions dataset. For a long time during the model development, land use was supplied externally along with solar and volcanic forcing. This was slightly unsatisfactory as land use is an anthropogenic forcing. It was noticed that land use ERF in the AR5 Annex II time series actually scaled fairly well with cumulative land-use CO_2 emissions, for the reasons given in the manuscript (total deforestation since pre-industrial is linked in some way to the total amount of

carbon lost to the atmosphere). Furthermore, in the future, the shapes of RCP land use forcing time series in MAGICC and FAIR are very similar, suggesting that land use CO_2 emissions are an important component of land use forcing in MAGICC. Our treatment is therefore no worse than in MAGICC, and probably simpler.

Requiring activity data for land use changes (from e.g. the gridded LUH dataset) would add significant complexity to the model for a small and uncertain forcing (see also response to other reviewer). However, like other suggestions, it would be a welcome development and could be implemented in a future version.

Page 11, section 3.3: Note the +/- 20% uncertainty in the CO2 ERF reflect uncertainty in our best estimate of the CO_2 forcing, not how it is implemented in the climate models. The actual CO2 ERF "seen" by individual GCMs may lie outside this range.

The 20% uncertainty applies to the 5–95% range around 3.71 W m^{-2}, so 10% of values going into the FULL ensemble lie outside of this range.

We should also be clear on the treatment; the Etminan et al. (2016) CO_2 forcing calculated by the model, from changes in GHG concentrations, is scaled to ensure that it equals the user-specified $F_{2\times}$ to ensure consistency between forcing and temperature change. A note at the end of section 2.2.1 has been added to explain this. In reality the user-specified value of $F_{2\times}$ actually makes very little difference to the temperature change (section 5), but affects the impact of non-CO_2 forcing on temperature as a low $F_{2\times}$ requires less non-CO_2 forcing to achieve the same unit temperature change.

Page 12, section 4.1: It is plausible that there may be an anti-correlation between a models F2x and its climate sensitivity (in K/(W/m2)). Is this accounted for in this study? Defining ECS and TCS in terms of F2x rather than in K/(W/m2) might hide some of the model variation in F2x.

No correlation was assumed between ECS and $F_{2\times}$ in the FULL ensemble. Analysing

the NROY ensemble also does not indicate any preference for high or low values of $F_{2\times}$. This is indicated in fig. 2 in this response.

Furthermore, we quickly investigated the relationship between ECS and $F_{2\times}$ from the CMIP5 results reported in Forster et al. (2013, Table 1), which shows there does not appear to be any correlation (fig. 3; this response).

Page 13, line number 15 (actually the first line!): Given that the FAIR parameters were derived from the historical GHG concentrations, it doesn't seem much of a test that it can reproduce them.

You are correct in that for methane and nitrous oxide, the comparisons to MAGICC6 can be made arbitrarily good by changing the natural emissions, and indeed have been in this updated model version (fig. 2 in paper), so a direct comparison over the historical period may not be relevant. The same could probably be argued for CFC12-eq and HFC134-eq, although natural emissions are only included for a few species and are not tuned to match concentrations except in the pre-industrial equilibrium case. However, concentration plots can demonstrate to potential users that the model is doing what is expected. Concentrations could also be used as an additional observational constraint in the CO_2 case, although we do not do that formally here as it can be seen in fig. 4a that the uncertainty around the present-day value is small in the NROY ensemble.

In the future simulations, concentration plots are useful to determine how FAIR differs from MAGICC under the simpler model setup in FAIR. It can be demonstrated the the impact of using a constant lifetime and constant natural emissions for methane and nitrous oxide is actually quite small for the RCP scenarios.

Page 13, line number 18 (3rd line): How can MAGICC reproduce the kinks in CO2, but FAIR can't?

The CO_2 time series that we compare to for pre-2005 is from observations. It is our understanding that MAGICC used prescribed GHG concentrations prior to 2005 and

emissions after this date. The historical emissions time series supplied in the RCPs are from Marland et al. (2008). By this observation we were not originally correct to *assert* that MAGICC could replicate the historical concentration time series (we don't know if it can or cannot). This has been changed in the revised version. Following a suggestion from the other reviewer we have plotted the historical CO_2 as an inset so the differences are easier to see.

The behaviour of CO_2 concentrations in the future between FAIR and MAGICC when forced with the same post-2005 emissions (fig. 4a; manuscript) shows that the carbon cycles in both models actually behave very similarly over the range of RCP scenarios in the default case. It has previously been demonstrated that MAGICC and FAIR can both be set up to emulate different carbon cycle models (see Millar et al. (2017); Meinshausen et al. (2011a)).

In reality, it is likely that there is significant inter-annual variability in the exchange between land, ocean and atmosphere that affect atmospheric CO_2 concentrations on yearly timescales (a recent example being the strong El Niño of 2016-17). By its design, FAIR will not capture these small deviations with four time constants. The more appropriate test is whether it can capture the long term trend between pre-industrial and present-day. Although not used formally as a constraint, CO_2 concentrations in the best estimates for the RCPs range between 403.4 to 408.4 ppm for 2017 in the temperature-constrained ensemble, in line with observations.

Page 13, line number 28: The authors recognise the problems with a fixed methane lifetime. It is not difficult to implement this to rectify this errors.

As previously discussed, using a variable methane lifetime results in implausible methane concentrations using the RCP time series. A time-varying methane lifetime would be desirable to implement, but we feel that this is not compatible with the RCP emissions data.

Page 13, section 4.3, lines 13-14: It's not surprising the linear regression repro-

duces the time series it was fit to. The future ERFs need to be compared to Stevenson et al. 2013, not MAGICC.

This has now been done in v1.2 of the model.

Page 13, line 15. It is not surprising that the model can reproduce the AR5 stratospheric ozone ERF as FAIR uses exactly the same formula as AR5 (scaling with EESC).

This is by design, as the model was originally intended to emulate AR5 where there was no evidence to suggest an updated treatment (as for methane ERF). Added "as it follows the same functional relationship as AR5".

Page 13, line 17. The reason FAIR overestimates the AR5 stratospheric water vapour value is because it scales up the Etminan et al. methane forcing which is 25% larger than the Myhre et al. 1998 forcing.

This has now been updated because of the inconsistency pointed out between taking 15% of the AR5 methane forcing and 15% of the new Etminan et al. (2016) methane forcing. The resulting time series as expected is similar to AR5.

Page 15, section 4.5: Since the methane forcing is 25% stronger in FAIR, presumably the TCR has to be lower to compensate. Does this explain the lower future projections?

As you may be aware, several of the author team are involved in the IPCC Special Report on 1.5C and this was a question that was considered. We found that the median TCR was 4% lower with the Etminan et al. (2016) relationship. It is a contributing factor, but not the only one. Changing the aerosol and tropospheric ozone relationships have in fact had a bigger impact on the TCR estimates (increase of 8% between v1.1 and v1.2).

Bottom of page 15, top of page 16: I don't understand this complicated method for calculating the TCRE to CO2-alone. Surely FAIR can be forced with just CO2

emissions and will output the temperature? If this is a CO2- alone calculation why does equation 22 account for the effect of non-co2 temperature changes?

We agree that this was originally too complex and had been re-run in CO_2 only mode with much of the old description in this section deleted. Figure 9 has been updated. The TCRE does not change much, although the all-forcing (red) curve is quite a lot higher than previously, a result of an upward revision of the future non-CO_2 forcing.

Page 18, section 5.2: This section needs to be expanded to discuss the difference between relative sensitivities in terms of F2x and absolute sensitivities in terms of K/(W/m2). If F2x is lower then the absolute sensitivity must be higher and hence the larger response when including the non-CO2 forcings.

This behaviour of higher temperatures for the same forcing under a lower F2x has now been explained using the energy budget framework in eq. 21.

Page 19, line numbered 18: I didn't understand why with a smaller (magnitude) present day aerosol forcing the 2100 temperatures are higher. Surely smaller aerosol forcing means lower TCR/ECS?

It is actually the future evolution in aerosol ERF that affects 2100 temperature change rather than the present day aerosol forcing, which has more of a bearing on ECS and TCR. As a low present day aerosol forcing also implies a low future aerosol forcing, and the full impact of ECS and TCR on temperature change has not been realised by 2100, a low aerosol forcing implies greater temperature change over this timeframe. This is further evidenced by the switch to a new aerosol relationship which has a lower 2100 aerosol forcing than in the previous version.

Tables 5–7 (now 7–9) in the manuscript have been updated with the new values.

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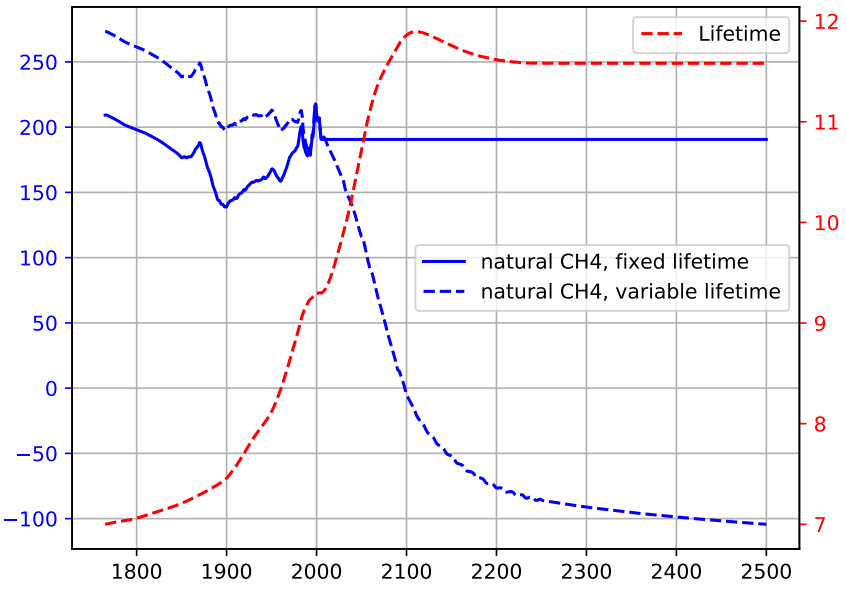

Fig. 1. Effect on natural methane emissions for fixed and varying methane lifetime for RCP8.5

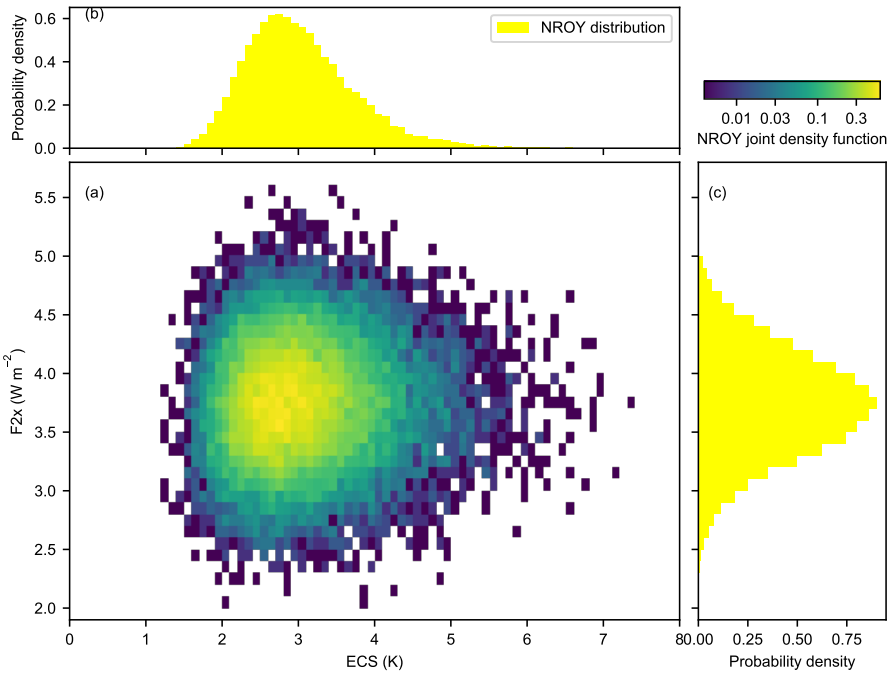

Fig. 2. Joint and marginal histograms of ECS and F2x in the NROY ensemble (after temperature constraint)

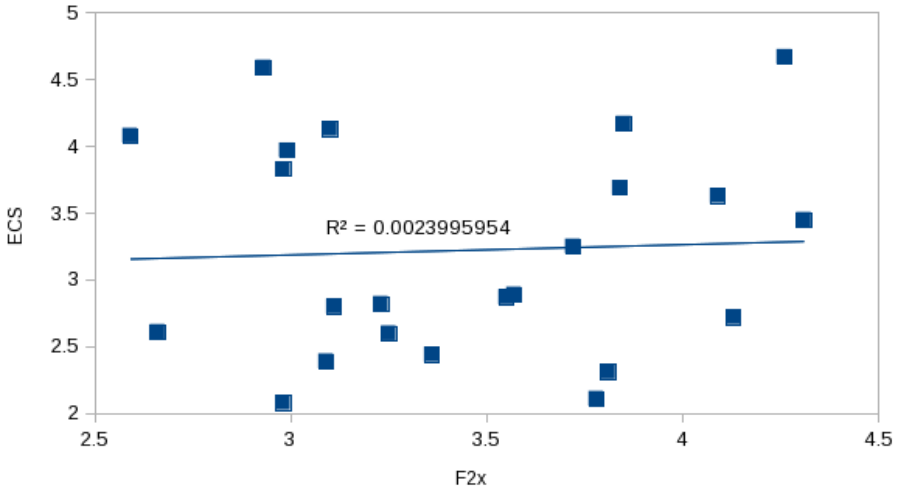

Fig. 3. Scatter plot of ECS and F2x for CMIP5 models in Table 1 of Forster er al (2013).