Answer to Reviewer 1

May 15, 2019

A general remark:

The reviewer repeatedly suggested describing in greater detail issues which are already treated elsewhere in the literature (most notably concerning the design and shortcomings of the DICE model and impacts of climate change). While we do appreciate his effort to eliminate potential ambiguities or omissions, we also believe that a research paper should not contain too much repetition of existing literature, and therefore tried to address many of these requests by adding a suitable reference.

General Comment 2:

To indicate that we indeed deal only with SRM, we did:

- replace “Geoengineering” by “Solar Radiation Management” in the title
- mention in the introduction that negative emissions are not included
- mention in the discussion part that trade-off between negative emissions, adaptation and SRM might be interesting wo study (with a more detailed model)

General Comment 3 + 4:

We included in the introduction a short paragraph with references concerning model limitations (in particular: damage function; abatement costs not including learning-by doing; single decision maker), but stated that we still believe that DICE to be a useful testbed for exploratory studies, which should of course be expanded by follow-up studies with more detailed models.

We also expanded the part on model imitations in the discussion. In particular, we clarified better how the limitations of the energy sector in DICE impact our results and mentioned the omission of negative emission and adaptation.

However, we did not repeat details of the DICE model, e.g. the construction of the damage function, since these can be found in the cited literature. We did add Nordhaus and Boyer 2000 to the references, because this paper gives a lot of detail on how the damage function is constructed.

General Comment 5:

It is hard to compare the uncertainties of SRM and (unmitigated) climate change, because especially the former ones are hardly quantified yet.

The most important pair of options which arises from our study is not “Geoenginereering vs unconstrained climate change” (at least we strongly hope this will not be the decision humanity will face at the end!) but “Abatement-only vs Abatement+SRM (and if so, how much SRM)”. So basically, we want to investigate whether we should add SRM to our action portfolio or relate on abatement only. And for this question, the uncertainties surrounding SRM are absolutely vital - and far less well studied than the likewise daunting uncertainties surrounding climate change.

By the way, volcanic eruptions provide only an incomplete analog to SRM because their effect is relatively short-term; and it is highly unlikely that SRM-only will keep the climate “more or less at its present state”, in particular as far as precipitation is concerned.

We added a reference to IPCC WG2 for an overview on climate damage (introduction) and briefly mention the issues of irreversibility and delayed damage (e.g. in case of ice melt).

Specific Comment Title:

See General Comment 2

Specific Comment P.1 L.2:

- “lower” -> “to lower”: adjusted

- “may”: note that this is about damages from SRM, not from climate change. And there is *huge* uncertainty about adverse economic effects of SRM, so “may” is quite appropriate... no change made.

Specific Comment P.1 L.3-5:

This is the abstract, and therefore should be concise. So it is not the place to list everything *not* taken into account in this study.

To clarify that the focus is on economics, we replaced “gains and damages” by “(economic) gains and damages”.

Specific Comment P.1 L.6:

change “should therefore be taken” into “therefore merits being taken”: adjusted.

Specific Comment P.1 L.6-7:

Yes, we agree that CRD should be mentioned, but not in the abstract. See general comment 3-4.

Specific Comment P.1 L.9 & 12-13:

The paper is not primarily about reaching temperature targets. The statement in line 9 is a descriptive one; it describes a result but we did not enforce any target into the simulation itself. So the suggestion to try several targets is not applicable.

The statement in line 12-13 merely illustrates that humanity is not on its way to reach any suggested temperature target through abatement, which might make additional measures like SRM attractive. It is outside the scope of this study to discuss the justification of 2K or 1.5K as target.

For what is included in the damage function: Please consult the cited literature, in particular Nordhaus.

Specific Comment P.1 L.14-16:

Omission of CDR is clarified in introduction (see general comment 3-4)

Specific Comment P.2 L.6:

Again, this study is not an assessment of the severity of climate change, so it is not the scope of this paper to list or discuss all climate impacts in the text (as mentioned, we added a reference to IPCC WG2 for an overview).

Here we change “associated damages” into “heating-induced damages”, to reduce ambiguity (since some damages associated with / sharing common cause with climate change, like precipitation changes and ocean acidification, are not directly caused by warming).

Specific Comment P.2 L.7-10:

Please note that part of the questions raised are treated in the literature cited.

Concerning the expected reduction in global precipitation: I am aware of no SRM study in which tweaking injection parameters (e.g. latitude, height, season of injection) avoids precipitation decrease, and there are plausible physical mechanisms why such a decrease should take place. Concerning precipitation patterns, there is more uncertainty and I expect that this more sensitive to injection parameters than the sign of the global precipitation trend. This is why we used the weak formulation “may”: It is quite perceivable that these pattern change (thus an important risk), but model uncertainty and the fact that injection parameters are undecided make it hard to quantity these effects.

Concerning sea level rise, this is one (important) component of temperature-induced damages (see Nordhaus’ paper for details).

Specific Comment P.2 L.11:

The cost of abatement is highly uncertain, because effects such as innovation, learning-by-doing, and customer behaviour are hard to predict (to say the least).

As a very crude indicator, we consider the cost of installing enough solar panels to meet its current energy demands.

Solar cells cost around 1 $/W of capacity; this rises to 4$/W when taking into account installation costs (at household level; may be different for large, remote solar parks). The sun does not always shine; let us be pessimistic and assume that only 20% of the capacity is actually used. This leads to an effective price of 20$/W of actual capacity. [Numbers from Cassedy and Grossmann; see below for precise reference.]

The current human energy consumption is roughly $1.1 \times 10^{17} Wh/yr$. Installing solar cells to meet this demand would cost $\frac{20\$}{W} \times \frac{1.1e^{17} Wh/yr}{(365 \times 24) h/yr} = 2.5 \times 10^{14}\$$ or about 6 times the world GDP. This is about 600 times as much as the yearly cost of SRM at an injection rate of 30Mt(S)/yr (typical SRM level in our Abatement+SRM scenario at standard model settings).

Obviously, this is a very crude indicator for abatement cost. Our measure does not include maintenance costs of the solar panels (which are currently about 20% of installation costs), nor the much larger costs of dealing with the intermittency of the energy supply (restructuring the energy grid, storage capacity...), or growing energy demand. On the other hand, it does not include the benefits of energy efficiency increase, cost reduction by technology improvement (currently about 20% cost reduction per doubling of installed capacity), reduced cost for carbon-based energy production, pre-existing renewable capacity, etc.

We include this estimate briefly for comparison.

Specific Comment P.2 L.13:

This paper is about whether or not to use SRM, not primarily about ethics of CO2 abatement. SRM has moral issues, such as the question of whom to put in charge of it, which are novel compared to those associated with global warming and abatement.

Specific Comment P.2 L.13-14:

Concerning the study by Robock on SO2 settling: The study used rather modest injection rates (about an order of magnitude below what we are considering here) and mainly dealt with the effect of SO2 on plants. The conclusion was that likely, at these small injection rates, the damage to vegetation is small, but at least in some regions, damage could occur if injections were tenfold.

Despite having severe impacts especially in large industrial agglomerations, industrial SO2 injections are a relatively regional phenomenon, because they are injected at low levels and washed after about a weak. SO2 from SRM could affect the entire atmosphere. So Robock's study certainly does not prove that SO2 loads from SRM have little effect. While the currently available studies, some of which we cited, give reason for concern, they are not extensive enough to quantify these concerns. Please note that in the Summary and Discussion section, we advocate to investigate these effects with a high priority.

The health issues associated with global warming are assumed to be part of the damage function for warming, and therefore - implicitly - reduced if the warming is reduced. A full assessment of health effects of unmitigated vs geoengineered climate is beyond the scope of this paper.

See also other comments concerning aspects of the damage function.

Specific Comment P.2 L.15-17:

While I agree that it is unlikely that the cost burden - or technical feasibility issues - will cause a sudden discontinuation of geoengineering, termination shock is still a relevant concern. (Armed) conflict could be one cause. More importantly, it cannot be excluded that the damage associated with SRM is higher than believed (for example, it could be that after a few decades of deployment it turns out that the long-term-effect on the ozone layer is detrimental, to name just one possibility).

And it is important to make clear that sudden disruption for whatever reasons could be detrimental, and therefore if choosing for substantial SRM, we are committed to continue it or at most phase it out slowly.

As you can see in the result section (fig. 2b, 3c), injection rates indeed decline towards the end of the simulation in the Abate+SRM cases. This is qualitatively the effect you mention, namely using SRM as a transition technology until abatement (which in principle could include CDR) sufficiently reduces CO2 concentration. We now highlight this important point in section 3.1.

Specific Comment P.2 L.18-19:

The main aim of the study is to investigate whether or not to add SRM to the policy portfolio in addition to the already well-studied abatement. Calling it a "comparison between SRM and abatement" would imply that these options are alternatives. But the main alternative is "Abatement+SRM" vs "Abatement only" (or for short: "SRM or not").

We reformulated the "should".

Concerning the explanation of the tipping point: It is numerically very hard to include several tipping points at the same time. We will now also study one different tipping point (namely sudden methane release from permafrost thawing causing intensification of warming) for diversity.

Specific Comment P.2 L.19-20:

"Inefficiency": There are studies (Kleinschmitt 2018) which suggest that the maximum achievable long-time cooling might be limited - which would imply that at least extreme scenarios like cooling "RCP8.5"-like temperatures to pre-industrial values are impossible. Inserted reference.

"Damage": while obviously the damage of unmitigated climate change would likewise be huge, it is not totally inconceivable that SRM causes some (unforeseen) damage, like massive destruction of the ozone layer or something we simply failed to think of. This may be unlikely, but we cannot know for sure. The inventors of the steam engine didn't think of global warming, either... We added the word "*unforeseen* damage" to clarify this, plus a reference to Robock2009.

Specific Comment P.2 L.20-21:

Reformulated "the optimal policy" into "the (economically) optimal policy" to clarify this.

Specific Comment P.2 L.31:

DSICE was developed based on the 2007 version of DICE, and later on the DSICE framework was adjusted in line with DICE2013. Now clarified and reference added.

The main objective of DSICE w.r.t DICE is indeed to add noise or tipping. This was a major piece of achievement.

In our study, as described later in the methods section, the stochastic elements are only the tipping point and SRM failure.

Specific Comment P.3 Table 1:

If one wants to give a detailed explanation of all variables, then the extra width will not be sufficient. Also, the symbols are explained in more detail in the methods sections. However, to ease orientation, we added references to the corresponding equations or sections (or, in one case, to the literature).

Concerning a) The value is the radiative forcing for e-folding (not doubling) CO2; see eq. 1

Concerning e) Precipitation goes up with CO2 because temperature goes up. If you add CO2 but keep surface temperature constant, then precipitation actually goes down. p_C only describes the pure CO2 effect; the temperature effect is in p_T. See eq. (6).

Concerning f) Damage is unitless, as it is expressed as fraction of GDP (see eq. 7); the damage associated with quantity X is of the form $\psi_X X^2$. This explains the units.

Concerning g) P2, L11 gives the range of cost per ton of injected gas; we assumed an intermediate value of $7 * 10^9 \$/Mt$. The table gives the cost per ton sulphur. Since we assume the injected gas to be SO2, which has twice the molecular weight of elementary sulphur, costs per ton (S) are twice as high as per ton of injected gas. This was also explained in line 27-30 on p. 6.

Concerning h) If you mean capital depreciation, this has nothing to do with the discount rate, but with the fact that capital (e.g. machines) looses value over time (e.g. by being worn-out our becoming outdated). This is explained in Nordhaus 1992 to which we now added a reference in the table.

Specific Comment P.4 L.2-8:

Indeed, tropospheric ozone is a relevant greenhouse gas and should be mentioned here.

On the other hand, since ozone is a byproduct of chemical reaction with anthropogenic emissions (e.g. CH4, CO in presence of NOx), the argument that "other forcing" can partly be abated, also holds for O3. So the coarse estimate on P4, L 11 can still be used.

Reference: Myhre et al., 2013 (IPCC WG1, chapter 8; see p. 760ff; fig. 8.4 also shows that scenarios with high CO4 concentrations tend to have high tropospheric ozone, confirming that ambitious abatement leads to lower tropospheric ozone)

Specific Comment P.5 L.5:

replaced "industrial processes" by "fossil fuel combustion"

Specific Comment P.5 L.6:

True, the model does not explicitly include the effect that a reduction in forest area reduces the CO2 sink stemming from Carbon fertilisation (while the direct loss of carbon, namely the loss of biomass and humus, is assumed to be included).

While this effect is surely relevant for detailed analysis of the carbon cycle, I am fairly sure that the effect is not bigger than, for example, the error bar on the total carbon fertilisation effect itself. Simulation the carbon cycle to such a degree of detail is beyond the scope of this exploratory study.

Specific Comment P.5 L.19-20:

No, this simply sets the initial conditions. While we are not doing so now, it is in principle possible to include CDR by having negative emission terms in equations 3b and 3c ($E < 0$).

Specific Comment P.6 L.11-13:

Yes, global warming increases global precipitation. However, the atmosphere not only warms due to heating from below, but also from absorbing long-wave radiation. This is explained in detail in the studies by Andrews 2010 and MacMartin and Kravitz 2016 (especially their fig. 2b) cited here.

If you suddenly increase CO2 (say, double it instantaneously), then you get a negative precipitation response at first (timescale of about a year) and only then, when the surface warming effect kicks in, precipitation increases. Likewise, when you both CO2 but keep surface temperature constant (say by SRM) then precipitation also decreases.

If you increase CO2 gradually and have no SRM (i.e. the current realistic conditions) then the warming effect is stronger than the direct CO2 effect, leading to net precip. increase when increasing CO2. We now pointed this out in the text.

Specific Comment P.6 L.13-15:

This sentence (line 13) is about (direct) precipitation response, not temperature response. The explanation here was merged with the previous one (just prior to eq. 6) and slightly expanded, to generate more clarity. We also explained the meaning of $p_T T$, $p_C F_C$, $p_S F_S$ here.

Specific Comment P.6 L.16-17:

While non-linear effects might be present, MacMartin and Kravitz show that a linear emulator yields quite a reasonable approximation. So there is no use here to introduce additional complexity.

Specific Comment P.6 L.19-21:

Since we are taking over the damage function approach from DICE, we will not repeat its explanations here, but added the corresponding reference to Nordhaus' work.

Nordhaus tried to incorporate non-GDP aspects into his damage function (see also Nordhaus and Boyer 2000, now cited in the introduction and section 2.1.3), for example through his "willingness to pay" approach. The DICE damage function has been criticised extensively, and with good reason. However, creating a proper (or even acceptable) damage function is tremendously complex and still an ongoing field of research. So this is outside the scope of this exploratory study.

In your example, the loss of the town would count as a damage in Nordhaus' approach. Basically, the idea is that each period, humans produce "something" ($\bar{Y}$, the gross GDP) and part of it gets lost due to climate change. Non-material losses are converted into material loss.

Specific Comment P.6 L.22-24:

10% economic drop *forever,* is quite significant (for comparison, the Great Depression around 1930 cut GDP by 15% for just a few years), and of the same magnitude of losses used in other studies, like Cai and Lenton 2016. Of course, you could come up with scenarios that are worse. However, doubling the tipping damage does hardly affect policy (see sensitivity run in table 4). A reason is that tipping usually occurs in the far future, which is strongly discounted. A second reason is that tipping is unlikely, if abatement and SRM are combined (i.e. it only occurs SRM failure).

In general one might question the DICE model's optimism about economic growth, and the way how discounting is used. This is outside the scope of this study.

Specific Comment P.6 L.26:

There would likely be initial development costs which don't increase with the amount of deployment.

However, the manufacturing of airplanes, fuel costs, costs for producing the injected gas, and manpower for operation all increase probably linear with injection rate. Given the large uncertainty (factor of about 5) in the cost estimates, we decided to stick to a simple, i.e. linear approach, and omitted initial costs, for which there is no good estimate.

Decreasing effectiveness of SRM is indeed treated separately, namely through the radiative forcing equation.

Specific Comment P.6 L.29-30:

It has been considered to use H2S. However, some raise concern that this gas is much more toxic than SO2. Also, we wanted to be conservative, and prefer to overestimate rather than underestimate the cost. This is now clarified in the text.

Specific Comment P.7 fig1:

The pathway CO2-warming-more rain is represented by the two arrows with a plus sign going from CO2 to T and from T to P. The arrow from CO2 to P is only the direct effect. The direct effect is quite important in the context of SRM which can partly offset the pathway via warming. Although the temperature-mediated effect of CO2 on precipitation is dominant in case of slow CO2 increase and zero SRM, it needn't be dominant when SRM is involved. So trying to scale the arrows could be misleading (especially if you want to do it in a quantitative way, e.g. letting the arrow width scale with the strength of the effect represented, because the corresponding constants have different units).

However, we inserted a reference to eq. 6 in the figure caption, so that the interested reader can refer back to the (now improved) explanations.

Specific Comment P.7 fig1 caption:

The mentioned damage from SRM is only the damage caused by SRM directly; reduction of damage from global warming is of course also modelled, but this goes via reducing the warming T. We inserted that SRM causes "*direct* damage of 20%" to clarify this.

Specific Comment P.7 L.4:

Sea level rise is accounted for as part of the temperature effect.

Adaptation is not included (yes, one of the many shortcoming of the DICE model... now mentioned in the introduction).

Specific Comment P.7 L.5:

Atmospheric CO2 may not be damaging in itself, but we use C as a (rough) proxy for ocean acidification, which we do not model explicitly. This is no clarified.

Specific Comment P.7 L.6-7:

The baseline for P is pre-industrial (as mentioned two lines above, L5).

The baseline for damage is also pre-industrial (in other words, if P, T, C would remain at pre-industrial levels, no "extra" damage from climate change occurs) which followed from the fact that $D = 0$ if $C = 0, T = 0, P = 0$.

I do not fully understand the last sentence. If the precipitation changes with SRM are smaller than without SRM, then the contribution of precipitation to the damage is smaller with SRM than without.

Specific Comment P.8 L.6:

The cost of sea level rise (which also makes storm surges more severe) is included in the temperature contribution of the damage (see Nordhaus and Boyer, 2000). That contribution goes quadratic with temperature, i.e. not linear (See eq. 8)

Specific Comment P.8 L.15:

See also previous comment on tipping.

The constraint is partly numerical. You could in principle allow multiple tipping points, for example one with higher likelihood and 10% loss, and one with lower likelihood and much higher loss. However, with dynamic programming, this will increase computational efforts very much, and given the relatively small impact of tipping on overall policy, we did not do this. (Cai et al, 2016 did, but they had only one decision variable, namely abatement.)

To add diversity to the representation of tipping points, we are now also investigating a different type of tipping point, in which global warming triggers a massive release of methane (suggestion by reviewer 2) and hence a positive feedback in the warming.

Specific Comment P.8 L.18:

Some tipping points are considered possible at lower, some at higher thresholds than 2K. Since we have included only one stylised tipping point (see previous point), we chose a compromise. The value is inspired by the Paris agreement to keep (well) below 2K warming. Of course, one could have chosen a different number here, say 1.5K, but the effects on the overall outcome are likely to be limited.

We will chose a lower threshold for the additional "methane" tipping point mentioned above.

Specific Comment P.8 L.21-22:

The idea here was to investigate in a stylised, qualitative way the hypothetical possibility that SRM might in whatever way be "unreliable". Being conservative towards geoengineering, we preferred to overestimate this possibility rather than underplay it (since no good estimates are available anyway).

Disruption of geoengineering is not totally inconceivable (who knows, it *might* destroy the ozone layer, or it *might* be that a war destroys the SRM infrastructure).

Since the result is that SRM is used anyway, despite the rather large failure probability (see also section 3.4, sensitivity runs), choosing a smaller failure probability is unlikely to affect the overall results.

Specific Comment P.8 L.24-25:

The matter of the termination shock is briefly investigated in section 3.3. We now added a remark here to clarify that the matter will come up later.

Specific Comment P.8 L.26-27:

DICE is a globally aggregate model. While it would be interesting - indeed, highly important - to study the effect of inequality, this is outside the scope of the current, exploratory, study.

Specific Comment P.9 L.1-6:

This is really getting repetitive.

I agree it is helpful to clarify in the beginning and / or discussion that our model is limited and our study therefore only an exploratory one, (as we did now in the introduction); but it makes no sense to discuss the same things over and over and over again throughout the text (or the review).

Discounting is a different phenomenon from the duration/reversibility of damage. The way to take into account the irreversibility of loosing the Amazon rain forest would not be to fumble around with two discount rates, but to change the damage function such that this contribution to the damage does not decrease again even if temperature deceases again. Note that our tipping point obeys this type of irreversibility dynamics - once tipped, the associated loss remains constant (even if temperature should decrease again), as pointed out in the beginning of section 2.1.4.

Specific Comment P.9 L.20-21:

Reformlated (following also a suggestion by reviewer 2) as: "Although the objective for the optimisation is the expectation value of the welfare, it is also interesting to investigate the range of possible welfare outcomes, especially the worst (or at least relatively bad) case scenario."

Specific Comment P.9 L.29:

Yes, in the sense that it was used as benchmark (see also result section), e.g. to compute performance. Clarified.

Specific Comment P.10 L.6:

The idea of this scenario is again to be conservative and check whether SRM might make sense even under unfavourable conditions - e.g. taking a long time to develop the technology. The scenario serves as comparison to the Abatement+SRM scenario where no such restrictions were present.

As a matter of fact, while obviously the parameters (time till SRM becomes available and likelihood of becoming available) are arbitrary and not constrained by data (so yes of course, one could have taken 2045 instead of 2055), I do not think they are particularly implausible. Properly assessing risks and efficiency, developing the technology, developing a legal framework for international collaboration, stopping security leaks, and convincing the general public that SRM is a good idea, are all difficult processes with an uncertain outcome. Nearly all big (government) projects take much longer than planned. Many pilot project of CDR/CCS have been stranded or at least greatly delayed due to public discontent, technical difficulties, economic problems, and so on.

In addition, it is unclear whether sufficient radiative forcing can be generated (see Kleinschmitt et al, 2018 cited in the introduction).

Clarified motivation for the scenario at the end of section 2.3.

On irreversibility of damages, see remark p.9 L.1-6

Specific Comment P.10 L.14:

I doubt whether the term you mention is bigger than the extra energy demand to build and fuel the SRM airplane fleet... or the change in need for warming in winter... or the effect of temperature on plant respiration and the solubility of CO_2 in the ocean. Most of these terms are quite uncertain. Without denying that SRM could influence CO_2 emissions and uptake, it makes little sense to try and include them into such a simple, exploratory study, especially not if we cannot even be sure whether we get the overall sign right.

Also, which study do you refer to? If it is the report by IEA: I just heard a presentation by a student of Guus Velders, mentioning that the study has considerable flaws.

Specific Comment P.10 L.16:

We did not include running-out of fossil fuels to avoid computational complications.

While the reserves of gas, oil and coal that we are relatively sure to be mineable will last a limited amount of time (about 50, 50 and 100 years, respectively; estimates from 2010-2014), it is believed that there are vast amounts of additional resources (e.g. fracked gas which since then came more and more into use; and potentially clathrates for gas), and undiscovered coal reserves. Some estimates assume that at present consumption rates, coal might last for 1000 years. So our no-action scenario is maybe extreme, but not impossible.

Source: Cassedy, E.S. and P.Z. Grossman, “Introduction to Energy”, third edition, Cambridge University Press, 2017; p31-32.

We acknowledged this in the manuscript.

Specific Comment P.10 L.19:

To cool the earth to pre-industrial from, say, RCP8.5 radiative forcing in 2100 would involve sulphur injections amounting to something like *14 Pinatubo eruptions* every year. Surely, that would be disruptive, too...

The sentence is simply a description of results using the current damage function. You can argue that maybe the damages for warming should be higher and for sulphur lower, especially for situations with high CO2 and high SRM. This would lead to more SRM and lower temperature in the SRM-only scenarios. But even then, you would very likely see that, since SRM is so inefficient at high forcings, it is a bad idea to lower extremely high temperatures back to pre-industrial with SRM alone. It is way better to use also abatement, or, if this were not done, you would still resort to reducing some, but not all, warming with SRM (also because admitting some warming will reduce the precipitation-related damage which would result from compensating all warming by SRM).

So the main conclusion to draw from the SRM-only scenario is that if you care about long-term effects, you'd better do abatement. We pointed this out more clearly.

Specific Comment P.11 fig.2:

The constraint of SRM starting not before 2055 only holds for the scenario dubbed “realistic storyline” (for which the results are only discussed later, see fig. 4), not in the stylised scenarios of fig. 2. This is outlined in section 2.3.

Concerning the time axis: We usually omitted the first time step, as it is a spin-up step. Now provided a consistent figure.

Specific Comment P.12 tab.3:

Indeed, this is the value in the first time step - we no point this out in the caption of table 2 and 3.

For remarks about reliability of SCC, see p14, L25ff.

For the calculation of abatement costs, see the cited literature on the DICE model. As we now briefly mention in the introduction, DICE assumes abatement always to be costly, although abatement costs go down in time. This is certainly questionable; but here we wanted to explore qualitatively what happens when SRM is added, and not rewrite the entire DICE model at once...

See also p.19 lines 1-3 for a short remark on the issue of learning by doing.

Specific Comment P.12 L.14-15:

See numerous previous comments concerning tipping points

Specific Comment P.13 fig. 3:

Is this actually a question? Concerning abatement and energy transition, see remark P.12, tab.3

Specific Comment P.14 L. 7:

This is the SRM-only scenario, for which we did *not* assume SRM to start only in 2055. See Methods section (sect. 2.3).

All we state here is that (in fig. 3b) the policy maker increases SRM when hitting the 2K threshold, in order to reduce the chance of tipping.

Specific Comment P.14 L. 16:

this seems not to be a question or suggestion...

Specific Comment P.15 fig. 4; P.16 L. 1:

See numerous previous comments on damage function and general DICE problems.

The conclusion from "realistic storyline scenario" vs. "Abatement+SRM" (which assumes immediate availability of SRM at 100% likelihood) is: As soon as SRM becomes available, if at all, it should be used to complement Abatement - unless termination shock damage and failure probability are really high.

This result would not change under a sensitivity study as suggested here.

Specific Comment P.16 L. 2-3; L. 5-6:

Clarified in Methods section on failure that it is indeed irrevocable. Also clarified there that failure is at present state speculative (though not impossible).

The sensitivity run on κ_{fail} showed that the main results are not very sensitive to the exact failing probability (if SRM is available, it is used even if it might fail somewhen).

Specific Comment P.16 L. 17:

I cannot see a connection between this remark and P16, L17, sorry.

abatement costs have been discussed already in previous comments....

Specific Comment P.16 L. 26:

Discussion on reliability of absolute CCS values: See p.14, l.25, including the reference to van den Bergh and Botzen cited there, which explains why CCS should be higher than DICE-like models.

Specific Comment P.16 L. 31-33:

Do we really need to estimate/conjecture/contextualise "huge" damages for an effect which we did *not* take into account? All we are saying here is that we *do not include scenarios* in which research on SRM (which would probably include testing at some stage) will cause huge/significant/whatever damages.

Of course, research cost on SRM is one of the myriad things one could investigate. But this would really blow up computational costs due to having another variable - especially if you want this to be an extra decision variable -, it would also introduce yet another layer of ill-constrained parameters (likelihood and size of damage during tests) and the effect on the results would likely be small. The possibility of "something going wrong early on with SRM" is qualitatively included by having a high failure probability in the first steps after SRM becomes available.

Specific Comment P.17, L1:

What DICE really needs is a proper energy sector, including effects like learning-by-doing, and that is outside the scope of this study.

We can, however, perform a sensitivity study where abatement costs remain exogenous, but are lower than in the standard settings, to investigate whether abatement costs have a large influence on policy (and thus whether this is an important aspect to study with better models).

Specific Comment P.17, L9:

If you do more abatement, you need less SRM.

CO2 has a long residence time; so abatement done now also affects future carbon levels and therefore reduces the need for SRM. SRM will always come at costs, so for a future generation it is always beneficial of they have to do less of it because a previous generation has done more abatement for them.

The lower ρ, the more prepared the earlier generations are to pay for abatement.

By "SRM causes damage" it is meant that SRM makes a contribution to damage, see ψ_S.

Specific Comment P.18, L3:

Nice suggestion, we will do that.

Specific Comment P.18, L21 ff:

As mentioned, I don't believe in constantly repeating things. Scientific papers are meant to be concise.

We have already put a cautioning remark on the most prominent DICe problems in the introduction, and expanded the section in the discussion on how these problems may affect our results, and repeated that CDR and adaptation are not included (even though tradeoff between SRM and adaptation and CDR would be important to study in folow-up research).

We also want to do an additional sensitivity study on abatement costs (See answer to comment p17, l1), to get a better idea of how large the influence of this effect on the policy.

Specific Comment P.19, L5-7

Concerning what is / is not included in damage function, see numerous previous comments.

Added "...uncertainties, especially concerning efficiency and damages of SRM and *the extent by which SRM can* mitigate damage inflicted by global warming"

Specific Comment P.20, L.30 ff

Interesting remark, although not of practical importance here, because throughout all simulations, no situation occurs in which the planet is too cool. As a side remark, adding CFCs or HFCs would not be exactly reverse SRM, because for example the effect on precipitation would be different.

Technical comments

- P1L1: we keep in spite of, to stress that (unlike abatement or CDR) SRM has impact even though greenhouse gas concentration remains high.
- P1L8: no "can" found in this line. changed "cannot replace but only complement" to *"can not* replace but only complement" to elucidate the structure of the sentence.
- P1L14: I see your point, but the reference was more to the geoengineering as such, not to the history of the discussion of geoengineering.
- P1L20: the connection with volcanic eruption is made just 1 sentence later, so I'd prefer to not make this sentence overly clumsy.
- P1L21: thanks, corrected.
- P2L4: reformulate for clarification
- P2L23-24: Moreno-Cruz corrected. that: corrected
- P8L2: refers to Heutel's parametrisation for CO2-related damage (not ours). Rephrased.
- P10L18: indeed, SO2 is injected (and leads to sulphate formation). Clarified.
- P12L10: corrected
- P13Fig3: time axis: because we were interested in how tipping continues in the course of time. For the other plots we zoomed in on the first ca 300 years for better readability, because nothing interesting happens in the last 100 years. (will clarify)
  We prefer not having the same y-axis for the mentioned graphs because then some graphs become very hard to read, especially SRM plots in simulations with abatement.
  Spelling: thanks, will be corrected.
- P14L8: performance is explained in eq. 11
- P14L9: the simulation goes to year 2400 even though the graphs do not (see remark above). CDR is not modelled, as you know.
- P20L21: this means that for "safety", the actual margin is made wider than the one suggested by the test trajectories, namely by adding 30% to the maximum and subtracting 30% from the minimum.

- P22ff: Stanford: corrected; title case: sorry, what is meant by this?; ncomms=nature communications (journal); Stowe: corrected.