Authors' reply on

Interactive comment on "The Role of Bioenergy and Carbon Capture and Storage (BECCS) in the Case of Delayed Climate Policy — Insights from Cost-Risk Analysis" by Jana Mintenig et al.

by Anonymous Referee #2

First of all we would like to thank the referee for critically reviewing our manuscript. Below we will theme-wise respond to the raised issues and indicate the corresponding changes in a potential new version. We will highlight the referee's comments by italic font while our reply will be in roman font.

This article analyzes the impact of BECCS in light of delayed climate policy under CRA using the integrated assessment model MIND. The results, main insights and conclusions of the analysis does not seem to differ from usual CEA analyses and I wonder what the additional insights from using CRA really are. The conclusions the article draws are basically that i) BECCS allows more flexibility (avoids corner solutions), and ii) has a moderating effect on welfare loss because it allows a smoother transition. This seems all very well known already and could be regarded as almost trivial (you add a relatively cheap option that allows negative emissions, so what else would you expect?). The same conclusions have been made with CEA analyses many times already. Perhaps the analysis could be made more interesting if not only climate risk is considered in the analysis, but also the risk of using BECCS itself. The latter is discussed, but not taken into account in the analysis.

The conclusions the referee draws as main conclusions from our paper are indeed not surprising and we would not have written a paper to convey them. However this paper mainly strives at another discussion: how society should act if the decision-analytic framework were changed from cost effectiveness analysis to cost risk analysis. It Roth et al., 2015, this change inverted the functional development of mitigation cost with delay, in the sense of flipping the sign of the derivative of mitigation cost with respect to delay. Here we answer the following two specific questions, (i) to what extent this observation is an artifact by the optimal solution being a corner solution and (ii) how the order of magnitude of cost would change if the currently most economic negative emissions technology were included. Answers to these questions we understand as our key findings. Hence part of the phenomena discussed in our ms is in stark contrast to what we know from cost effectiveness analysis. A revised version of our ms would make much clearer what our key findings are. However taking BECCS risk into account would lead into a very different discussion making a like-with-like intercomparison with Roth et al. difficult.

Furthermore, I have some reservations regarding some of the results and assumptions of the model. Especially I do not understand why there are no renewables in the baseline up to 2080 (Fig 3a and 3b) – as currently about 20% of the global energy mix is already based on

non-fossil fuels according to the IEA Energy Outlook 2017 (based on Mtoe, see p. 79). Where is the wind, solar, and hydro in the results? I also do not understand very good why first fossil fuel use decreases and then increases again in the mitigation scenarios (see Fig 3c and 3d). Finally, an important mitigation option in almost all IAMs is to increase energy efficiency, but here, there seems to be no additional improvements in energy efficiency in the mitigation scenarios. Finally, why is TNF (I guess total non-fossil, which probably means nuclear?) in mitigation scenarios the same as in baseline scenarios?

The baseline scenario has not been updated to include an elevated level of renewables to preserve comparability with older studies of the model. A new version of our ms would offer a respective adjustment as a sensitivity study. The MIND model is a stylized model to include endogenous technological change, hereby being more realistic on mitigation cost than e.g. Nordhaus' DICE model. It is the optimal model to study stylized effects when decision-making uncertainty is to be included (in our case on climate system response to greenhouse gas-induced forcing of the climate system). The MIND model by definition does not resolve the renewable sector any further. It is the underlying technical innovation of this paper to explicitly model BECCS within MIND in order to represent the option of negative emissions. Regarding the tradition of all MIND-based papers we prefer sticking to leaving the exogenous TNF (in fact nuclear energy, large hydropower, and traditional biomass policy) untouched to compare like with like. We would make much clearer in a revised version of the ms that these days, the MIND model is not meant to compete with high-resolution energy system, yet also intertemporally optimizing integrated assessment models like REMIND or WITCH, but to demonstrate order-of-magnitude effects when changing the analytic framework for decision-making under uncertainty.

Other remarks: According to model set-up, the change in emissions is limited to 13% annually. If the restriction is indeed applied like this in the model, it would be impossible to achieve net negative emissions (if emissions are close to zero, in fact hardly any reductions are possible anymore). Why not restrict absolute reductions instead of relative reductions?

This relative restriction origins from Kriegler & Bruckner, Climatic Change, 2004, who found a relative restriction more intuitive than an absolute restriction. The rationale behind this is a convex 'social and infrastructure change-induced cost' curve, in analogy to a convex mitigation cost curve. The referee is perfectly right in that if such a restriction were applied to the net emissions, no net negative emissions were possible. However we do apply it to the gross emissions for which the restriction was invented. We would make this subtle, yet crucial point much clearer in a new version of the ms.

I guess I do not see a fundamental difference using CRA and using CEA with different likelihoods of achieving the target. Isn't a discussion about how much risk we are willing to take similar to the discussion on the likelihood with which we want to achieve the target? Any CEA study is under a certain assumption as to the likelihood with which a target is achieved.

We agree with the referee that these days – at the latest since the Copenhagen Diagnosis as of 2009 – CEA is always meant to be seen as an approximation of CCP (chance constraint programming, i.e. optimization under a probabilistic climate target, as no temperature target can be complied with for 100%). However the key point of CRA is not usage of a probabilistic target, but the fact that this probabilistic target, for the first time, is

made tradable against mitigation cost, while in CEA/CCP it remains a rigid target. In the language of decision theory: the change from CEA/CCP to CRA is the change from a lexicographic (climate first) preference order to a non-lexicographic, utilitarian one. A rigid target is replaced by a somewhat 'porous' one – it somewhat resembles a second order phase transition from thermodynamics. This is the core reason why delay has just the opposite effect in CEA as against CRA. We would make this central point much clearer in a new version.

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Regarding the more detailed comments made by the referee we will comply with all the suggestions – except for the last comment which re-iterates above misunderstanding of CRA: we in fact have to insist that mitigation cost decline, the reason being that we have replaced a strict by a soft target, the very point of this and the Roth et al. paper.

Overall we are convinced that we can address most of the referee's concerns in a new version. However we ask for the referee's understanding that we cannot have a detailed energy system discussion here as it would be possible for high-resolution models like REMIND or WITCH. Here it is about to demonstrating the key effect of changing the decision-analytic framework addressing uncertainty, in the very presence of BECCS.