# Rejoinder to the review of our article „Making or breaking climate targets – the AMPERE study on staged accession scenarios for climate policy“ by Rich Rosen in the Journal Technological Forecasting & Social Change

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Commentaries and critical reviews published in scientific journals can foster scientific debates and ultimately lead to the advancement of research fields. However, such commentaries need to be based on knowledge and understanding of the topic under scrutiny, or they risk to leave the domain of productive scientific exchange. The critical review of our article „Making or breaking climate targets – the AMPERE study on staged accession scenarios for climate policy“ by Rich Rosen in the *Journal Technological Forecasting & Social Change* unfortunately is an example of a critique plagued by misconceptions, unsubstantiated assertions and false claims. The apparent lack of understanding of integrated assessment modeling, and possibly about general standards of scientific debate, may have led the review to alleging rather frivolously that our analysis has been unscientific. To this end, we note that the AMPERE project was accompanied by an external Scientific Advisory Board, and that all of its key outcomes have been published in the peer-reviewed literature.

We were not informed about the existence of the review article by the journal *Technological Forecasting and Social Change*, and therefore prepared this comprehensive rejoinder once we became aware of its online publication. Since the critique put forward in the review is a good illustration of several common misperceptions about the nature and science of integrated assessment, we include a longer annex to discuss these misconceptions and provide a clearer understanding of strengths, weaknesses, and ultimately the value of integrated assessment.

The critique in the review makes a number of erroneous points that fall into four general categories: 1. Our study advises against global cooperative action to mitigate climate change, 2. the integrated assessment models (IAMs) used in our study are not well documented, 3. the model comparison approach that we are using is unscientific, and 4. our estimation of mitigation costs are inadequate and misleading. We will refute these points one by one below, and provide a more detailed discussion of underlying misconceptions and false claims in the annex. The review specifically targets our overview article on the AMPERE study of staged accession scenarios (Kriegler et al., 2015a), but claims to speak for a much broader set of articles, in particular the 17 companion articles contained in the AMPERE special issue on the economics of climate stabilization in *Technological Forecasting and Social Change* (see references section for a full list of articles). These include overview articles on two further model inter-comparison studies on the role of delayed mitigation and technology availability (Riahi et al. 2015) and model diagnostics (Kriegler et al. 2015b). We will point out instances where the critique fails to recognize the broad coverage of topics by the articles in the special issue and the recent literature on IAM model comparison studies.

The critique's assertion that the AMPERE study was conducted to advocate staged accession scenarios, and that integrated assessment modeling analyses tend to discourage global cooperation on climate change, is unsubstantiated and rather an opinion. It is neither the task nor the goal of the AMPERE integrated assessment modeling studies to advocate specific climate policy scenarios. The fact that the AMPERE study analyzed staged accession scenarios vis-à-vis a benchmark case of full global cooperation is not motivated by the desire to advocate the former class of scenarios over the latter, but by the desire to assess the implications of the fragmented state of near-term international climate policy action for the attainability of long-term climate targets. There is an obvious value in better representing short term climate policy choices and understanding their alignment with long term goals, and the AMPERE study provided significant progress in this area.

The critique asserts a lack of documentation of the modeling tools in the AMPERE study. This claim does not withstand closer scrutiny. Our paper provides a comparative overview of key characteristics of the underlying models both in the main paper and the supplementary information. To this end, the supplement includes a detailed spreadsheet on model characteristics providing harmonized descriptions across models to allow for direct comparisons. In addition, we have provided a 50 page supplementary documentation of the study approach, the scenario setup (including the original study protocol) and the participating models. This documentation includes a summary paragraph on each model with further references to articles and model documentations for the interested reader. Quantitative information on model input assumptions, including cost assumptions, can be found in several of these references[1]. Moreover, we have published the data of the full set of AMPERE scenarios used in our study, and the two companion studies by Riahi et al. (2015) and Kriegler et al. (2015b), in a database hosted by IIASA and referenced in our article (https://secure.iiasa.ac.at/web-apps/ene/AMPEREDB). This database includes, e.g., information about capital costs of electricity generation technologies, fossil fuel prices, socio-economic drivers (GDP and population) and energy demand (which the critique falsely claims to have not been disclosed) along with a large set of other key variables characterizing the scenarios. Finally, the special issue contains a companion study (Kriegler et al. 2015b), which is one of the largest integrated assessment model diagnostic study to date with the explicit aim of increasing transparency about the differences in model response patterns. Given this wealth of information, we can only conclude that the critique of our model and study documentation is affected by oversights or bias.

A major problem of the critique is its confusion of model comparison studies with model sensitivity studies. Changing single parameters of a model to assess their influence on the model output is indeed a very valuable exercise, and, of course, frequently done in single integrated assessment model studies (e.g. McJeon et al., 2011; Luderer et al., 2013; Rogelj et al., 2013). However, it is not applicable as such to model comparison studies. Models differ in many input and structural assumptions, and what may be an input parameter to one model could be a constraint or a structural assumption in another model. Therefore, model comparisons undertake controlled variations of a set of policy assumptions (such as the AMPERE and EMF22 studies: Kriegler et al. 2015c; Clarke et al. 2009), technology assumptions (such as the AMPERE and EMF27 studies: Riahi et al. 2015; Weyant and Kriegler, 2014) or socio-economic development assumptions (such as the RoSE study: Kriegler et al., 2013; Mouratiadou et al., 2015) across models to understand robust and sensitive features of baseline and mitigation pathways. This is an effective way to capture model uncertainty, and to better understand model behavior and differences between model results. Such model comparisons

[1] E.g. Luderer et al., 2011, for the REMIND model (see https://www.pik-potsdam.de/research/sustainable-solutions/models/remind/REMIND_Description.pdf) or the reference for the GCAM model (www.globalchange.umd.edu/models/gcam) providing the entry point to the GCAM wiki (wiki.umd.edu/gcam).

are not only conducted in the integrated assessment modeling community, but in many other modeling communities, including the climate modeling community (CMIP1-5), the climate impact modeling community (ISI-MIP), the agricultural modeling community (AgMIP) and the water modeling community (WaterMIP). This does not prevent the critique to call the AMPERE approach unscientific, mostly based on the grounds that it is not a single parameter sensitivity study that model comparisons neither can be nor intend to be.

One of the fundamental misconceptions of the critique concerns the nature of the mitigation cost assessment in the AMPERE study. It is long standing practice in climate change economics to differentiate between cost-benefit and cost-effectiveness analysis of climate policy. By definition, cost-effectiveness analysis do not consider the magnitude of climate damages nor the intertemporal trade-off between mitigation costs and climate damages. Cost-effectiveness studies focus on the economics of reaching a pre-defined climate goals. The benefit of this approach is to gain a deeper understanding about the mitigation dynamics and the direct costs of mitigation policy. Unlike a cost-benefit approach that considers both the costs and benefits of a problem, the cost-effectiveness approach represents only the mitigation cost component of a climate policy. There is value in research that looks at both the cost and benefit components separately. The IPCC, for example, devoted separate working groups to benefits (Working Group II) and costs (Working Group III) as well as the joint consideration of both (Synthesis).

The AMPERE study follows the cost-effectiveness approach as many other integrated assessment modeling studies before, but the critique apparently fails to recognize this. In a cost-effectiveness study that explicitly does not model avoided climate damages it is rather non-sensical to expect negative cost estimates due to the benefit of avoided climate damages. The additional claim "*that the term mitigation benefits as opposed to mitigation costs is not even mentioned or discussed as a possible outcome of mitigating climate change*" (Rosen 2015, pg. 3) is simply false. Our article explicitly states that "*Reported values are direct (or gross) mitigation costs that do not include the direct benefits from avoided climate damages, or any co-benefits and adverse side-effects from mitigation action.*" (Kriegler et al. 2015a, pg. 33). Furthermore, we included an entire section (4.4) and a dedicated Figure (Fig. 4) to compare warming reductions due to mitigation with mitigation costs. In view of these multiple passages in the paper, it is rather astonishing that the review neither recognized the nature of the cost estimates nor the discussion of mitigation benefits in our article.

In addition, the critique appears to miss the important fact that costs are calculated relative to a dynamic baseline as is common practice in integrated assessment modeling. This implies that if a low carbon technology outperformed a fossil fuel technology without any climate policy intervention, it would already be reflected in the baseline. The cost estimates thus capture the additional effort due to climate policy. The annex discusses the important topic of the choice of baseline and the assessment of the changes between baseline and policy scenario in greater depth. This discussion reveals a number of deep misconceptions about economic policy analysis in general, and integrated assessment modeling in particular, in the critique. A recent review of mitigation cost estimates by the same author - also published by Technological Forecasting and Social Change (Rosen and Guenther 2015) - suffers from the same misconceptions. Most of the arguments seem to be driven by displeasure with the reported results, rather than a careful inspection of methods and models.

A rather disturbing aspect of the critique is its use of the notion of scientific standards and peer review to serve its purposes. For example, the critique puts forward the false notion that the models used in the AMPERE study have never been peer reviewed (Rosen, 2015, Footnote 8). To the contrary, numerous studies based on these models have been published in the peer-reviewed literature, and the large majority of these articles included model descriptions and often links to publicly available model documentation resources on the web as was the case in our study (see Table 1 and supplementary information of Kriegler et al., 2015a). Of course, one can always ask for more in documenting complex numerical models with hundreds or thousands of equations, and the integrated assessment modeling community is actively working on expanding and harmonizing model documentation standards, e.g. in the ADVANCE project (see www.fp7-advance.eu). But any serious claim of a lack of model documentation and peer review should be based on the recognition of available resources[2] and peer-reviewed literature, respectively, and a comparison to other research areas using large-scale numerical models (such as climate modeling). Nothing of this kind can be found in the critique of our paper. Instead, we are presented with outcomes of private email exchanges in a footnote. These are fairly questionable practices for a review paper given how easy it is to frame people in such email exchanges, particularly if their true intention is not disclosed.

In the appendix to this response, we detail numerous additional errors. In summary, however, we show that the critique is riddled with false claims and misconceptions that reflect a rather limited grasp of climate change economics and integrated assessment modeling. As much as we believe in the value of scrutinizing published research and engaging in critical scientific debates in academic journals, this critical review published by *Technological Forecasting and Social Change* did not contribute to it.

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[2] For a few examples of web-based documentations of integrated assessment models see www.pik-potsdam.de/research/sustainable-solutions/models/remind (REMIND model, PIK), wiki.umd.edu/gcam (GCAM model, PNNL), themasites.pbl.nl/models/image/index.php/Welcome_to_IMAGE_3.0_Documentation (IMAGE model, PBL), and www.witchmodel.org (WITCH model, FEEM).

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## Annex: Detailed discussion of misconceptions and false claims in the review by Rich Rosen

**Documentation of modeling tools and study approach:** The review criticizes the AMPERE study for a lack of documentation: "*Unfortunately, however, from the perspective of a reader of this paper, each model is essentially a proverbial "black box". The reader can have little idea as to what is going on inside the "black box" based on the information presented in the TFSC article (Kriegler et al., 2014).*" (Rosen 2015, pg. 2). As pointed out above this claim does not withstand closer scrutiny. The AMPERE study was documented exceedingly well with (i) a 50 page supplementary document containing summary descriptions of participating models, a detailed description of the scenario approach and its adoption by individual models, and the full study protocol, (ii) a spreadsheet with an extensive description of model characteristics and (iii) a database containing the full scenario data used in our AMPERE study and its two companion studies (in addition to the description of methods and participating models in the main article). The detailed spreadsheet on model characteristics includes, e.g., solution approach, economic coverage, cost metrics, emissions coverage, representation of energy technologies (wind, solar, geothermal, nuclear, hydropower, coal, gas, biomass-fired power with and w/o CCS, biomass and coal to liquids) and resource availability (coal, oil, gas, bioenergy, uranium, CO2 storage). An important and innovative feature of the documentation is to provide harmonized descriptions across models and arranged by topic in order to allow for direct and easy comparison between models.

The review asserts: "*To get a somewhat better, but still not complete idea, of how the different models function, one would have to undertake a major research project consisting of trying to find documentation of all the eleven models on the websites of the research teams. However, a reasonably complete set of the important input assumptions, especially cost assumptions, used in this paper cannot be found anywhere, including in the supplementary online material that was published with the paper.*" (Rosen 2015, pg. 2). This is false. Both the main paper and the supplementary information provide references to individual model descriptions and/or articles for each model. Thus, the reader is not required to research these references by him or herself. As pointed out above, quantitative information on model input assumptions, including cost assumptions, can be found in several of these references (see Footnote 1 above). In addition, the AMPERE database provides a host of quantitative information by model, scenario, region and point in time. This includes, inter alia, capital costs of electricity generation technologies, deployment of energy technologies, fossil fuel use and prices, socio-economic drivers (GDP and population) and energy demand. Thus, the review's claim that "*major baseline assumptions are not provided.*" (Rosen 2015, pg. 3) is also false.

**Sensitivity analysis:** The review asserts a lack of sensitivity analysis in our article, and by implication the larger set of AMPERE studies presented in the special issue in Technological Forecasting and Social Change (as stated: "*I will focus attention on this single overview paper as representative of the others.*" (Rosen 2015). Concretely, it is claimed: *"In fact, no sensitivity analyses based on varying key cost input assumptions are presented at all."* (Rosen 2015, pg. 2), and *" … higher levels of possible cost effective investments in energy efficiency than the models allow for in the mitigation scenarios have been totally ignored. [Footnote: In fact, no discussion at all of enhanced energy efficiency as a technological mitigation strategy is even mentioned in the article.]"* (Rosen 2015, pg. 4). However, the AMPERE companion study conducted by Riahi et al. (2015) looks inter alia into the effect of limited mitigation technology availability and increased energy efficiency on mitigation pathways. As a result, energy efficiency is an important topic in Riahi et al. (2015) as well as in some other papers (e.g. Bertram et al. 2015; Bibas et al. 2015) of the AMPERE special issue. Also, Riahi et al. (2015) conduct a sensitivity analysis that takes individual technologies off the table and thus provides an upper bound on the impact of increased technology costs on mitigation pathways. In addition, two other recent integrated assessment model intercomparison studies have focused on sensitivity analyses of socio-economic assumptions (RoSE study: Kriegler et al., 2013; Mouratiadou et al., 2015), and technology and energy efficiency assumptions (EMF27 study: Weyant and Kriegler, 2014; Kriegler et al., 2014).

**Model comparison studies**: Our major point on the difference between model comparison study and single parameter model sensitivity studies has already been made in the main letter. Here we want to highlight that model comparisons provide an effective way to capture model uncertainty, and – contrary to what the critique asserts - to better understand model behavior and differences between model results. The interactive process of comparative analysis of model output and discussion about the underlying reasons among modelling teams generates the insights that make model comparisons so valuable. Even though our study is based on a model comparison, the critique claims: "*Note that uncertainty regarding policies is mentioned, but the uncertainties inherent in IAM modeling work are not mentioned.*" (Rosen, 2015, pg. 4). The entire idea of conducting a model comparison is to bring out the uncertainty due to different model assumptions and structures. We write in the conclusion of our paper: "*Several caveats of this study need to be mentioned. First, the results are contingent on the models that participated in this study. While the use of multiple models in a comparison exercise greatly improves the assessment of uncertainty due to different model assumptions and structures, it cannot capture the full range of uncertainty.*" (Kriegler et al., 2015a, pg. 41).

**Mitigation costs:** As pointed out above, several fundamental misconceptions of the critique concern the nature of the mitigation cost assessment in the AMPERE study. For a proper interpretation of the mitigation cost estimates, it is crucially important to recognize the nature of cost-effective analysis as opposed to cost-benefit analysis, and the role of the baseline in describing the counterfactual case of no climate policy intervention. Integrated assessments of the cost-benefit type include a representation of climate damages on the economy, and attempt to calculate a cost-optimal climate policy trajectory. The outcome of such fully integrated cost-benefit analyses is sensitive to assumptions about climate damages and the discount rate of consumption. In contrast, cost-effectiveness analyses deliberately do not account for the trade-off between mitigation and climate damages, but instead focus on the economics of reaching pre-defined climate goals. The benefit of this approach is to gain a deeper understanding about the mitigation dynamics and the direct costs of mitigation policy, because it allows the use of more detailed energy-economy-land-climate models, and the sensitivity of results to uncertainty about climate damages and consumption discounting is significantly reduced.

Despite the fact that cost-effectiveness analysis is a standard approach taken by many integrated assessment modeling studies, the critique apparently fails to recognize it[3]. The critique goes on to

[3] For example, the critique states on pg. 3, 2nd paragraph: "*Every model run reported in Fig. 3 shows net "consumption losses" and none show net benefits. Furthermore, the authors state that they "expect global direct mitigation costs to rise with mitigation stringency". Why, they never say. Their reason is probably that if there are net costs for less stringent mitigation, then since the marginal costs of more stringent mitigation would be higher than the average costs for less stringent mitigation, the average costs of more stringent mitigation scenarios will increase. But what if there were many scenarios that the modeling teams could have*

claim "*that the term mitigation benefits as opposed to mitigation costs is not even mentioned or discussed as a possible outcome of mitigating climate change*" (Rosen, 2015, pg. 3) and *"..., the study neglects to mention that many possible types of economic benefits of mitigating climate change over the long run have been completely left out of the models used, especially the avoidance of damages from climate change to the world's economy, people, and ecosystems.*" (Rosen, 2015, pg. 4). These claims are false. For example, we state in our paper: "*Reported values are direct (or gross) mitigation costs that do not include the direct benefits from avoided climate damages, or any co-benefits and adverse side-effects from mitigation action.*" (Kriegler et al., 2015, pg. 33). Furthermore, we included an entire section (4.4) to compare warming reductions due to mitigation with mitigation costs. This section starts as follows: "*A global assessment of staged accession has to contrast the benefits in terms of avoided climate change and the mitigation costs relative to the reference case of fragmented and moderate climate action over the 21st century. Fig. 4 provides such an overview.*" (Kriegler et al., 2015a, pp. 33-34). We also state in the conclusions: "*Several caveats of this study need to be mentioned. … Second, we have used a range of metrics to explore the benefits (maximum and 2100 global mean warming, probability of exceeding two degrees) and costs (aggregate mitigation costs, transitional costs, carbon price expenditures) of climate action. While these cover key elements of cost-benefit considerations, a full assessment of the costs and benefits of climate policy will rely on a broader set of indicators, including regional climate impacts, institutional challenges, and co-benefits and adverse side effects.*" (Kriegler et al., 2015a, pg. 41).

**The role of the baseline:** A close reading of the critique reveals a lack a basic understanding about the nature and role of the baseline in integrated assessment modeling studies. First, these baselines are fully dynamic, i.e. they will include any energy transition processes that are triggered by other factors than climate policy intervention (e.g. by fossil resource scarcity or optimistic assumptions about future performance of renewable energies). Such transitions happen indeed in some of the baselines, e.g. the REMIND model begins to substitute substantial amounts of fossil fuels with renewable energy in the second half of the century due to falling investment costs for renewable energy and increasing extraction costs for fossil fuels. Nevertheless, a robust message from the multitude of integrated assessment modeling studies of mitigation pathways is that mitigation does not happen at the scale required for climate stabilization (implying net zero greenhouse gas emissions in the long run) without climate policy intervention. In other words, the message is that mitigating climate change needs climate policy, not the least due to the worldwide availability of significant resources of cheap coal.

Second, the changes between the baseline and climate policy scenario are the central result of integrated assessment models and therefore should emerge from the model dynamics, not from changing model input assumptions between baseline and policy cases. The latter is actually a no go in integrated assessment modelling, as it would open the door to arbitrary results directly related to ad hoc changes of exogenous input assumptions. To our surprise, the critique seems to call for exactly

*run that would have produced net economic benefits from mitigating climate change? Then more stringent mitigation scenarios might yield even greater net benefits to society. Why weren't those scenarios run and described in this article? Based on my review of many other papers written by the authors of this paper, I suspect that, in fact, each modeling team only made scenario runs with one set of most input assumptions, including only one set of input cost assumptions, even though no research team could possibly know which values of such input assumptions were most likely to occur in 20, 50, or 90 years from now.*" This suspicion is off the mark. The overall finding of positive mitigation costs has nothing to do with the choice of cost input assumptions, it is due to the fact that the AMPERE study is a cost-effectiveness analysis.

this approach when it states (Rosen, 2015, Footnote 3): "*This is not a minor point, namely the need to consider baseline vs. mitigation scenario assumptions separately, since most studies like the AMPERE study fail to discuss the need to create systematically different sets of input assumptions for these qualitatively different kinds of scenarios. This is especially true for energy efficiency assumptions, which should be one of the highest priority mitigation options, but which the article ignores entirely. Clearly, most analysts would assume more energy efficiency in the mitigation case than in the reference or baseline case as an input. Also, the input costs of renewable energy supplies should generally be lower in the mitigation cases than in the reference cases, since more investment in them would occur in the mitigation scenarios, and there would be more cost reductions via "learning by doing". Similarly, the cost of fossil fuels should be higher in the reference case, since demand for them would be much higher.*" We should clarify that even though the model input assumptions are identical between baseline and policy scenarios (as they should be), this does not mean that energy efficiency, costs of renewable energy and fossil fuel prices are identical as well. These are endogenous variables in integrated assessment models, and they respond to climate policy. As an example, and contrary to what the critique asserts, energy efficiency indeed increases in the climate policy scenarios in response to higher energy prices. And fossil fuel prices decrease because of a cut in demand for fossil fuels induced by climate policy. And higher deployment of renewable energy technologies leads to lower investment costs for these technologies in those models that include learning by doing effects. So the type of dynamics that the critique is calling for lies at the heart of integrated assessment models as those used in the AMPERE study.

Third, the dynamic baseline is used as a counterfactual to calculate the costs of mitigation policy by comparing household consumption, economic output etc. in the mitigation policy scenario with the baseline scenario. At times, the critique seems to be unaware of this fact, e.g. when writing: "*The authors seem blind to the fact that, certainly, there must be some sets of reasonable technology cost and availability input assumptions for energy supply and demand technologies, and for fossil fuels, that would lead to high net economic benefits of mitigating climate change over the long run.*" (Rosen, 2015, pg. 3). If conditions would be so favorable that climate policy would not be needed to induce a decarbonization of the energy system, then the transition to a low carbon economy would occur already in the baseline, and the costs of the (superfluous) policy intervention would be zero. The misperception that the policy cost measure should show direct economic benefits under favorable technology assumptions may be related to the misconception that input assumptions should be changed between baseline and policy case.

**1$^{st}$ and 2$^{nd}$ best settings:** In order to describe the only circumstances, in which a policy cost measure in a cost-effectiveness analysis using a dynamic baseline can become negative (i.e. showing economic benefits), we need to briefly introduce the concept of first and second best settings and how it relates to the choice of baseline and climate policy intervention. Even though this goes far beyond the level of the critique, we think it is useful to discuss it here because it is part of the real scientific debate about mitigation costs. In a first best policy environment characterized by functioning markets in the baseline, with climate change being the only market externality, any addition of climate policy would lead to aggregate economic costs. In such a setting, a first best policy introducing a global carbon price will be the least cost strategy. In a second best policy environment with imperfectly functioning markets in the baseline, e.g. due to distortionary taxes and subsidies, a second best policy would still lead to aggregate economic costs, but those are potentially lower than for a first best "carbon pricing only" policy imposed on the second best environment (Lipsey and Lancaster, 1956). Only if policy instruments are added to reduce some of the ancillary externalities (e.g. revenue recycling to reduce labor market imperfections or spillover externalities), economic co-benefits of mitigation policy can occur. The extent to which they can lower costs or even lead to net economic benefits (before including the direct economic benefit of avoided climate damages) is an empirical question, and depends on the formulation of the baseline and the second best policy. These important points have been highlighted repeatedly in the literature, not the least in the 5th Assessment Report of the IPCC.

So what assumptions about the baseline and the climate policy did the AMPERE study make? First, many models included some element of second best policy environment in their no policy baseline (e.g. fossil fuel subsidies), although the assumed market distortions were relatively small in most models - with the exception of the IMACLIM model which accounts for labor market distortions and inertias in technical systems (Waisman et al., 2012), and the WITCH model which accounts for international externalities of innovation (Bosetti et al., 2008). Second, all models calculated a reference policy case that included technology policies (renewable energy portfolio standards, minimum capacity requirements). The costs of more stringent climate policy cases were calculated relative to both the no policy baseline and the reference policy case. Third, the mitigation policy imposed in most models was a comprehensive policy implementing a carbon pricing at the optimal level ("first best") combined with the technology policies of the reference case (with the exception of the IMACLIM model which added an infrastructure policy and recycled carbon pricing revenues to reduce labor market distortions). Thus, the AMPERE study already moved a good deal away from the previously common approach to only consider first best baseline and carbon pricing only policies, although, of course, more research on economic co-benefits and adverse side effects of mitigation policies is needed. And it happened to include one model, IMACLIM, that showed large enough co-benefits of the added infrastructure policy to yield aggregate net economic benefits of mitigation policy in some cases (Bibas et al., 2015; see also the caption of Figure S2 in the supplementary information of Kriegler et al., 2015a).

**Net present value cost estimates:** The review criticizes our presentation of aggregate mitigation costs in net present value terms: "*With respect to the discount rates used in the various IAMs, which strongly affect the magnitude of the reported results, footnote #5 on page 10 states that different models used different discount rates for optimization purposes when computing results, ranging from 3% to 8%. However, Fig. 3 seems to indicate that cost results from every model were discounted at a 5% discount rate for presentation purposes. However, unless the models were actually run using a 5% discount rate for the preparation of Fig. 3, it is totally meaningless, inconsistent, and visually deceptive to take results that were created utilizing one discount rate and then present them (in Fig. 3) based on a different discount rate*" (Rosen, 2015, pg. 3). This is a strong exaggeration. The discount rate of most models that performed an intertemporal optimization (as opposed to other models that did not optimize over time) clustered around 5%. We also provided a sensitivity analysis of net present value cost estimates to the choice of discount rate in the range from 3% to 8% in Figure S2 in the Supplementary Information of Kriegler et al., 2015a. The qualitative results drawn from Figure 3 are unchanged.

**Cost input assumptions**: The critique directs large attention to model input assumptions on technology costs, as it appears to see them as a panacea to explain all kinds of model output. For example, it asserts: "*Importantly, different assumptions by different modeling teams regarding the cost of mitigation options are very likely to be a key determinant, if not the key determinant, of the very different CO2 prices, because input costs generally determine output prices in such models.*"

(Rosen, 2015, pg. 3). This is an overstatement for several reasons. First, levelized costs of energy emerge endogenously in the models, and usually reflect more than just cost input parameters, e.g. integration requirements and carbon pricing (i.e. they are scenario dependent). Moreover, not the absolute cost of energy produced by a technology, but the relative costs between technologies influence the technology deployment in the energy sector[4]. Finally, technology deployment may be equally or more strongly affected by additional constraints, e.g. concerning energy resource availability and diffusion rates. Thus, it is by no means a "*logically obvious point that different assumptions about cost inputs might account for much of the different price results for CO2.*", as claimed in the review (Rosen, 2015, pg. 3). On the contrary, as stated in our paper, we think that differences in emissions reduction requirements (due to different emissions baselines) and substitutability of energy technologies (as determined by model structure and availability of mitigation options) have a much larger impact on differences in CO2 prices. This is supported by our model diagnostics study published as part of the AMPERE special issue (Kriegler et al. 2015b). In this study, one could have found some of the answers that the critique is calling for when writing: "*One key, but unanswered question, is to what extent are these differences in carbon prices due to different model structures and the different assumptions as to mitigation option availability. One key question that also should have been addressed is what particular features of the model structures lead to such big CO2 price differences.*" (Rosen, 2015, pg. 2). The critique and the related review of mitigation cost estimates by Rosen and Guenther (2015) comes to the general conclusion that mitigation costs over the 21st century are unknowable because technology cost assumptions are. Of course, no robust prediction of economic outcomes can be made even over much shorter time spans. But this is not the point, as no unconditional predictions are attempted. Rather, it is the careful framing of mitigation cost estimates relative to a dynamic baseline that allows a structured exploration of economic impacts conditional on a range of different, and uncertain, scenarios.

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