Rejoinder to the review of „Making or breaking climate targets – the AMPERE study on staged accession scenarios for climate policy“

April 14, 2015

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 a solid 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 Rosen (2015) in the *Journal Technological Forecasting & Social Change,* to which we will refer as simply, “the critique”, is plagued by a number of misconceptions, unsubstantiated assertions and false claims, which appear to reveal a lack of understanding of the approaches used in the AMPERE study.

The critique has been labelled as perspective article, but is in most parts a review of Kriegler et al. (2015a) and the wider results of the AMPERE project presented in a special issue on the economics of climate stabilization in *Technological Forecasting and Social Change* (see references section for a full list of articles in the special issue). In addition to our study on staged accession scenarios, the special issue included studies on the role of delayed mitigation and technology availability (Riahi et al., 2015) and model diagnostics (Kriegler et al., 2015b). We focus our rejoinder on the review content of the critique by refuting its central claims one by one below. Those are: 1. our study is motivated by the desire to advocate globally fragmented climate policy approaches, 2. the model comparison approach that we are using is not credible, 3. our estimation of mitigation costs is inadequate and misleading, 4. the integrated assessment models (IAMs) used in our study are not well documented and 5. the peer review of our study did not serve its purpose. We provide a more detailed discussion of underlying misconceptions and false claims in the annex. We will also 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. Since the critique highlights the need to clarify misconceptions about the nature of integrated assessment modeling approaches, we see our rejoinder as an opportunity to provide a clearer understanding of strengths, weaknesses, and ultimately the value of integrated assessment.

Motivation of the AMPERE study: The critique’s assumption 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. It is neither the task nor the goal of the AMPERE studies to advocate specific climate policy scenarios. The fact that our 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 (Kriegler et al., 2015c).

Model comparison approach: A major problem of the critique is its apparent confusion of model comparison studies with model sensitivity studies. We agree with Rosen (2015) that changing parameters of a model to evaluate their influence on model output is indeed a very valuable exercise to understand model sensitivities to input assumptions. Such studies have been frequently performed with individual integrated assessment models (e.g. Gritsevskij and Nakicenovic, 2000; Clarke et al., 2008; McJeon et al., 2011; Luderer et al., 2013; Rogelj et al., 2013). However, they are not transferable 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, and in contrast to what the critique seems to assume, it is usually not the question of model comparison studies, including the AMPERE studies, whether model differences are due to input or structural assumptions. Rather, model comparisons undertake controlled variations of, e.g., 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) to understand robust and sensitive features of energy-emissions pathways along the selected dimensions given the full set of differences between models along the other dimensions. This is an effective way to capture "between model" uncertainty. It also allows to better understand differences in model behavior due to the iterative process of comparing model output and discussing underlying reasons for output differences among modelling teams. Studies differ in the emphasis placed on capturing the degree of "between model" uncertainty in policy applications vs. diagnosing differences in model results in more stylized experiments, and AMPERE has undertaken both types of studies (Kriegler, 2015a and Riahi, 2015, emphasize application, while Kriegler, 2015b, model diagnostics).

The value of model comparisons is underlined by the fact that they are conducted in many 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). The model comparison approaches in these communities do not differ structurally from the approach taken in integrated assessment modelling studies.

Mitigation cost estimates: A major misconception 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 does 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 of 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, devotes separate working groups to the assessment of 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. Although it is impossible - by definition - to generate negative cost estimates due to the benefit of avoided climate damages with a cost-effectiveness approach, the critique appears to claim that such results should exist (see the annex for further discussion). 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 and the long tradition of cost effectiveness studies, it is rather astonishing that the critique neither recognized the nature of the cost estimates nor the discussion of mitigation benefits in our article.

In our study, mitigation 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 further misconceptions about economic policy analysis in the critique.

Related to this discussion is the critique's emphasis on technology cost assumptions. As discussed in the annex, it is by no means the case that these assumptions are the dominant driver of mitigation costs. The critique comes to the general conclusion that mitigation costs over the 21st century are unknowable because technology cost assumptions are. We agree with Rosen (2015) that 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. The integrated assessment modeling community is well aware about the deep uncertainty about long-term technology developments as documented by, e.g., its involvement in research on technological innovation in the energy system (e.g. Wilson et al., 2013; Grübler and Wilson, 2014). It is indeed an important debate how the tension between deep uncertainty in the long run and the need to perform century-scale analysis due the long-term nature of climate change and mitigation goals can be adequately addressed (e.g. Morgan and Keith, 2008). But this debate needs to be informed by the existing research on the topic and should not suffer from misconceptions about methodological approaches. Furthermore, it equally applies to the assessment of climate change mitigation, climate change impacts, and adaptation to climate change.

Documentation of modelling tools: The critique that the modeling tools and assumptions used in the AMPERE study were not adequately documented 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 without grounds.

Peer review process: The critique that reviewers of the AMPERE special issue were unable to assess the merit of the models and the study approach is baseless. The guest editorial board was fully committed to a thorough review process to ensure a high quality product, and the AMPERE project itself was supervised by a scientific advisory board throughout its lifetime. The two reviewers per paper were carefully selected experts in the field of integrated assessment modelling with deep knowledge about the modelling tools and approaches (as was the case for the guest editors who provided additional review comments were needed). Furthermore, numerous studies based on the models in the AMPERE study have been published in the peer-reviewed literature before, 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. Thus, the notion put forward in the critique that the models have never been peer reviewed is wrong. It did not fall on the review process for the AMPERE special issue to review these models from scratch. The critique's recommendation to zero-base the review of modelling tools in each study that uses them contradicts the basic notion of the scientific enterprise to build on published work.

While it is true that introducing large-scale numerical models to the peer-reviewed literature and reviewing subsequent studies based on them can be very demanding, the challenge is neither new nor unique to integrated assessment modeling. Large-scale numerical models are used in environmental and economic research since decades. Of course, one can always ask for more in documenting such models with hundreds or thousands of equations. We agree with Rosen (2015) that improving comprehensibility and comprehensiveness of model documentations have large benefits for transparency and model evaluation (see also Schwanitz, 2013). To this end, 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).

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[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).

[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).

We conclude that the central points of the critique of our AMPERE study are not tenable. We provide a further discussion of these and other points in the annex, highlighting a number of additional misconceptions and false claims in the critique. The critique appears to suffer from a limited understanding of climate change economics and integrated assessment modeling. We hope that our rejoinder has been able to clarify a number of items concerning the purpose of IAM studies, model comparisons, mitigation costs, model documentation and peer review, and thus will contribute to an improved understanding of the approach taken in the AMPERE study, and many integrated assessment modeling studies at large.

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[3] The views expressed in this article are purely those of the author and may not in any circumstances be regarded as stating an official position of the European Commission.

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Annex to the rejoinder

This annex serves a threefold purpose: to pinpoint false claims in the critique by contrasting quotations from it with elements of our original paper, to provide a more detailed discussion of misconceptions and further clarification of them, and to address several additional smaller claims in the critique that are misconceived or exaggerated.

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 critique 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 critique's claim that "*major baseline assumptions are not provided.*" (Rosen 2015, pg. 3) is also false.

Sensitivity analysis: The critique 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 of the AMPERE special issue (e.g. Bertram et al. 2015; Bibas et al. 2015). 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).

Mitigation cost estimates: As pointed out above, several deep 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[4]. The critique goes on to 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

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[4] 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 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.

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 of 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 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 misconception 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.

1st and 2nd 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 beyond the level of the critique, we think it is useful to discuss it here because it is part of an active scientific debate about mitigation costs (see Clarke et al, 2014, Section 6.3.6.5, for an overview). 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 gross economic costs (before accounting for the benefits of avoided climate damages). 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 gross 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 accounting for the direct economic benefits of avoided climate damages) is an empirical question, and depends on the formulation of the baseline and the second best policy (Fullerton and Metcalf, 1997). These important points have been highlighted repeatedly in the literature, including the 5th Assessment Report of the IPCC (Kolstad et al., 2014; Clarke et al., 2014).

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 went a good deal beyond the standard approach of considering first best baselines and carbon pricing only policies. And it included one model, IMACLIM, that showed large enough co-benefits of the added infrastructure policy to yield aggregate 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). Of course, more research on economic co-benefits and adverse side effects of mitigation policies will be needed in the future.

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[5]. 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

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[5] This also implies that changing cost parameters for only a single technology may lead to inconsistencies if the underlying argument for this change would also affect other technology cost parameters.

claimed in the critique (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 and mitigation costs. This is supported by our model diagnostics study published as part of the AMPERE special issue (Kriegler et al. 2015b).

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