Response to Anonymous Referee #1

The reviewer comments are numbered for clarity and indicated in *italic*, while our responses are indicated in dark blue.

1. *I don't really know what to make of this paper—I'm not sure it's science, as it isn't clear to me how one would test it except by waiting until 2100. That puts it into the category of an engineering study—do empirical regressions, generate some small ensembles, build the bridge, and ask whether it will fall down?*

We will reply to the substance in this review, and avoid commenting on the unacceptable style of some of the reviewer's comments. We would respectfully suggest to this reviewer to follow the scientific standard etiquette on conducting reviews, i.e. substantiate claims with evidence or literature references, rather than broad-brushed attacks that are rooted in opinion and not in fact. While some of his comments (i.e., on the 'probabilistic' aspect) are valid and provide an opportunity to clarify our presentation, other comments, in particular on general aspects of climate model projections, make us wonder if the reviewer has the appropriate background knowledge to comment on the abilities of various projection techniques, including those presented in IPCC AR4 and well documented in the literature.

The reviewer seems to suggest that any long-term projections (we base our study on three classes of projections, i.e. based on a) the methods that IPCC AR4 chose for its sea level rise projections, b) the semi-empirical class of models and c) the empirically-derived projection estimates by Pfeffer et al.) are outside science, because "it is not clear to me how one would test it except by waiting until 2100". We are sure the reviewer is aware of the broad literature on long-term projections on climate science (e.g. Knutti et al., 2008). The two main, and well established, methods for how to estimate the legitimacy of long-term projections is a) the testing against historical observational data and b) to base projections on physical principles. This paper does present and partially combine a multitude of well-established projection methods, all of which have of course limitations, but all of which have been subject in length to tests, quantification of their uncertainties and description of their limitations as shown and referenced in the underlying cited literature. Our paper is not the appropriate place to summarize fundamentals of climate projection research in general.

2. *More specifically, it is really a study of the scatter in predictions by a class of models: the title of "probabilistic projection.." is misleading, because a true probabilistic projection would include the prior probability distribution of the models—something that isn't even mentioned.*

Broadly speaking, there are two common usages of the word "probabilistic": a broad one and a stricter one. The strict one would in each subsection of the uncertainty representation include observational data to formally derive likelihoods. The likelihoods, in turn, would allow to constrain the priors and to estimate the posterior distributions. In the broader definition, which we follow, uncertainties are characterized by their distribution of occurrence, at the moment best represented by the existing multi-model ensembles.

In some sections, our paper follows a strict Bayesian setting with prior-constraint-posterior (the underlying MAGICC-based temperature projections for the RCPs under all three "ice sheet" alternatives), while in other sections the state of scientific knowledge is required to present multi-model ensembles or – in the case of the IPCC projections – multiple lines of evidence. The implicit assumption is that all diagnosed models are a representative, random sample of the "true uncertainty" or that the multiple lines of evidence are interpreted as an unbiased representation of the full uncertainties. We omitted to remind this basic point in the manuscript, for brevity and because we considered our method description to be clear enough. We state however in the manuscript that the GCMs have been selected on the basis of their skills to represent present-day dynamic topography (Yin et al., 2010). Further, more general discussion on the challenges to interpret the "ensemble of opportunities" constituted by GCM ensembles and to use observation as constraints can be found in Knutti (2008, 2010), Knutti et al. (2010) and references therein. We believe that the metrics to compare regional sea-level simulations and observations in order to weight the projections has yet to be identified.

Admittedly, our treatment of the ice-sheet contribution is not probabilistic in the sense that we use three different approaches in parallel: The IPCC AR4 approach, the semi-empirical approach and the evidence-based scaling estimates by Pfeffer et al. We explicitly avoid assigning a probability to either of these fundamentally different approximations. Under a "loose" interpretation of probabilistic, every single of these three approaches could however be termed "probabilistic".

In regard of the multiple usages of the term "probabilistic" in the literature, we propose to a) change the title to "Projections of sea-level change along the world's coastlines and its uncertainties.", b) restrict the use of the term "probabilistic" and replace it by something like "broad representation of uncertainties" when applicable, and c) amend the paper with a more detailed discussion on the description of the presented uncertainty ranges.

3. *So for example, what is the probability the Rahmstorf model is skillful? It ignores entirely the known physics of continental glaciers. Since some of the Antarctic ice is nearly 1 million years old, surely there would have to be terms reflecting the lag times between temperature changes 1 million years ago (and everything in between) and modern melt rates. Thus one might argue that the probability of it being correct is nil!*

These criticisms have been covered at length in the literature: The semi-empirical model has been designed for short-term (century-long) projection, as clearly stated by the author (Rahmstorf et al., 2007). So the million-year history of the Antarctic ice-sheet is not relevant here. Its skill at simulating the past century sea-level rise is evidence for this. It is precisely because of the lack of knowledge in the physics of continental glaciers that the model has attracted attention in the scientific community (but by no mean consensus). Rather than "ignoring the known physics", it would be more appropriate to state that the model attempts to "bridge" the lack of knowledge in the physics – until better models are available. That said, the use of a simple, linear model to project future sea-level rise evidently comes with drawbacks. This discussion has been summarized in our paper (section 2.3.1):

"*These methods typically yield future sea-level projections that can reach more than one metre of rise by 2100, which is significantly higher and in sharp contrast with the IPCC AR4 projections. One caveat in their application is that the semi-empirical relationships between temperature and sea-level variations is calibrated over a relatively narrow range of global mean temperature variation compared to the projected warming by 2100 (Lowe and Gregory, 2010), but in the absence of robust physical models that can reliably and explicitly simulate ice sheet response to warming based on first principles, semi-empirical methods still provide a useful, plausible alternative estimate (Rahmstorf, 2010)*"

Moreover, our paper attempts to go beyond the debate of the total ice-sheet contribution by 2100 and focuses on comparing the sea-level patterns under several approaches for the ice sheets (including IPCCAR4-like projections), rather than claiming that any single approach is superior to the others. The fact alone that semi-empirical projections are still part of the debate (see above-cited literature), and are sometimes used in the planning for adaptation, justifies having it presented in our paper as one of several approaches.

4. *A true probabilistic prediction would have the posterior probabilities depend upon the model priors.*

We have already discussed the terminology of "probabilistic projections" above.

5. *They say P. 364 that they are spanning the range of possibilities—but how is that known? It seems to be only the range of the given models. What is the probability they really do span the possibilities?*

We will reformulate that statement and others in the text to make the interpretation of our uncertainty estimates clearer, in line with the above discussion.

6. *Perhaps the authors can deal with this: (1) How does one falsify the conclusions of this study without waiting 90 years?*

This is a challenge for any kind of long-term projections, and it is in the strictest sense impossible. The old adage 'all models are wrong' comes to mind, but this argument is a bit of a straw man. The models are built upon the basic laws of physics. Also, historical and paleo-simulations show that climate models are indeed skillful in capturing the essential behavior of Earth's climate system (e.g. Yin et al., 2010, for GCM skills at representing present-day dynamic sea-level topography, after which we selected the GCMs used for regional sea-level projections in our study). Thus, one can infer (without rigorously showing so) that there is skill in these projections. Exactly how much is certainly difficult to assess.

7. *(2) If they cannot or won't assign prior probabilities to the models, they can reformulate the paper as a study of empirical ensembles run on various classes of model.*

The interpretation of "probabilistic" has been discussed above. We propose a new title: "Projections of sea-level change along the world's coastlines and its uncertainties" which addresses the related critiques of the reviewer. See above for other proposed changes.

8. *But I wouldn't want to do an expected cost calculation based upon the results.*

That is a personal opinion, to which the reviewer is of course entitled to, but this criticism has no place in a scientific review.

9. *Other issues: The published heat uptake calculations are taken at face-value. But as the authors note, they are mostly based upon the upper 700m, and with most of the data from the northern hemisphere. Shouldn't a probabilistic calculation account for the likely errors in these estimates? At least discuss it. It's argued that the model-data discrepancy is due to the models using the whole water column. Does that not mean there has been no net heating below 700m? Is that probable?*

This point is not correct. MAGICC makes projection of the whole water column, constrained with the observations that are available over the upper 700 m. MAGICC does an excellent job at simulating heat uptake and the uncertainty in the data is fully taken into account as shown in Meinshausen et al (2009). Also, the scaling with the GCMs use the whole water column heat update versus thermal expansion. There is definitely net heating below 700m in MAGICC simulations themselves. That GCMs show a higher heat update than the data for a unit of sea-level rise is consistent with deep ocean warming (in the calculation of meter sea-level per energy absorbed, the data-based estimate under-estimates the amount of deep warming, while the models take the whole water column into account). The critique is wholly unfounded.

10. *Do Pfeffer et al. state that their ranges should be interpreted as uniform probabilities?An unusual result.*

This is not clear, but this is the way we chose to interpret it. This does not have any real impact on our conclusions.

11. *Coastal changes are surely going to be influenced very strongly by regional wind variations (thought to produce much of the present regional variations). Isn't the wind field something that needs to be discussed? Predictability of the wind field? Is this effect negligible?*

Again, we are confused about the reviewer's remark. From introductory physical oceanography it is well known that the wind forcing is an elementary component of the ocean's dynamics, as reflected in sea surface height variations. The GCMs do indeed simulate changes in the large-scale wind fields, and this does cause a significant part of the regional dynamic sea level change, that we discuss extensively in the paper. But why would it follow that we need to 'discuss the wind field' itself? This paper is about regional sea level changes. Wind extremes, such as storm surges, can of course also change over time and present another threat from sea level change, but we do not discuss synoptic events or changes in the extremes. This has been discussed elsewhere in the literature.

References:

Knutti, R. (2008). Should we believe model predictions of future climate change? Philosophical transactions. Series A, Mathematical, physical, and engineering sciences, 366(1885), 4647-64. doi:10.1098/rsta.2008.0169

Knutti, R. (2010). The end of model democracy? Climatic Change, 102(3-4), 395-404. doi:10.1007/s10584-010-9800-2

Knutti, R., Furrer, R., Tebaldi, C., Cermak, J., & Meehl, G. a. (2010). Challenges in Combining Projections from Multiple Climate Models. Journal of Climate, 23(10), 2739-2758. doi:10.1175/2009JCLI3361.1

Lowe, J. A., & Gregory, J. M. (2010). A sea of uncertainty. Nature Reports Climate Change, (1004), 42-43. AGU. doi:10.1038/climate.2010.30

Meinshausen, M., Meinshausen, N., Hare, W., Raper, S. C. B., Frieler, K., Knutti, R., Frame, D. J., et al. (2009). Greenhouse-gas emission targets for limiting global warming to 2 degrees C. Nature, 458(7242), 1158-1162. doi:10.1038/nature08017

Rahmstorf, S. (2007). A semi-empirical approach to projecting future sea-level rise. Science, 315(5810), 368-370. doi:10.1126/science.1135456

Rahmstorf, S. (2010). A new view on sea level rise. Nature Reports Climate Change, 4(1004), 44-45. doi:10.1038/climate.2010.29

Rahmstorf, S., Perrette, M., & Vermeer, M. (2011). Testing the robustness of semi-empirical sea level projections. Climate Dynamics, in press, 1-15. doi:10.1007/s00382-011-1226-7

Yin, J., Griffies, S. M., & Stouffer, R. J. (2010). Spatial Variability of Sea Level Rise in Twenty-First Century Projections. Journal of Climate, 23(17), 4585-4607. doi:10.1175/2010JCLI3533.1