

Review of “The Social Cost of CO2 from the PAGE09 Model” by Chris W. Hope

The paper presents the input parameters of the PAGE09 model, key outputs of the model (CO2 concentrations, temperature, impacts and social cost of carbon), an analysis of the influence of parameters on the social cost of carbon and a regional split of the social cost of carbon and shares of emissions.

The paper is well written and one can easily follow the presentation. The new version of PAGE is an important tool both in the policy world and in academic publishing and the careful and detailed presentation of the input parameters is very useful. All the changes to the model look like clear improvements over previous versions of PAGE. The presentation of the regional split of the social cost of carbon in combination with responsibilities of emissions is nice and makes for an interesting interpretation of the results.

I recommend publication of the paper, subject to the individual points outlined below.

Thank you

General comment: The paper presents many of the new parameter values used in PAGE 09, but none

of the equations. The paper states that the model equations can be found in a companion technical paper (Hope, forthcoming). I find that separation very problematic, both for a reader as well as for review. As a reader, I am presented with a large quantity of parameter values but with almost no information how those values are used in the model. But surely these parameter values are only of interest if one knows how they are actually used in the model, i.e. if one sees the equations at the same time. For review, this gets really problematic: in order to evaluate whether a specific parameter

makes sense one needs to see how it is used, but none of that is actually part of the paper. I would suggest that the model equations are either included in an appendix in this paper, or even better are presented in the same place where the parameter values are shown.

I am not averse to including the equations in an appendix. But the full equations run to 14 pages in the technical paper. So it might be better to offer them as supplementary material here.

Page 1: Why do most distributions have a triangular distribution? Weitzman (2009) and the literature

that followed argued that the real reason for concern about climate change might be fat tails. By using triangular distributions for all input parameters, the author prohibits anything like fat tails by assumption to ever show up in PAGE. Is there a good reason for that?

This is not true. Many variables in PAGE09 are made up from combinations of the input parameters. These variables can have long and fat tails. Figure 1 gives an illustration of this. The climate sensitivity is constructed from two of the input parameters, but has a long tail out to 6.5 degC and beyond. Figures 5 and 6 illustrate this even more clearly, showing the total and marginal impacts with very long tails. I can add a sentence to discuss this if that would be helpful.

Page 2: Why stop in the year 2200? Are we not expecting significant impacts after that? I would assume that at the low time preference rates that are also used in PAGE this arbitrarily ends the analysis too early.

The default final analysis year of 2200 is consistent with other models in the literature. However, it is changeable by the user. Future papers will explore the consequences of a longer analysis period.

Page 3: Climate sensitivity. Shouldn't the most likely value (mode) be 3.0, not the mean, to be in line

with IPCC?

This depends a bit on how you interpret 'best estimate' in the IPCC report. But given the long right tail, a modal value of 3 degC would imply a higher mean of about 3.5 degC which is out of line with the current literature.

Looking at Andrews and Allen (2008) I cannot understand why a triangular distribution is used, surely there is nothing in their work that suggests there is a cut off at 7 degree?

The climate sensitivity is not a triangular distribution. The default values give a long tail out to above 6.5 degC. If users want to explore even higher values, they can easily change the TCR and FRT inputs to do so.

Page 5: The text quotes Stern (2007) saying that poor countries would have higher costs of impacts (as a percent of GDP), but Table 5 and the first sentence of the last paragraph on page 5 suggest that in PAGE every other region than Europe is less vulnerable to climate change. How does that square?

*The key point about the weights is that they are defined for the same sea level and temperature rise, and **at the same GDP per capita**, as stated in the text at the bottom of page 5. So poor countries can have higher impacts than the EU even with weights of less than 1, because their GDP per capita is lower.*

Page 6: Are the weights in table 5 applied to all impacts, or is there a different set of weights for each type of impact? One set of weights for all impact types seems highly inappropriate: as the author points out earlier, some regions are less vulnerable to sea-level rise than the EU because of their coast length. But the same region might be more vulnerable to temperature increases than the EU. Given that temperature and sea-level rise have very different dynamics, lumping everything into one weight could be highly misleading.

The weights apply to all impacts. Having different weight factors for each impact sector would increase the number of inputs from 7 to 28, and introduce many difficulties to cope with correlated inputs. However, it does lead to the problems mentioned by the reviewer and is a decision that might be revised in the next version of the model.

Page 6: The text mentions that impacts are explicitly linked to GDP per capita and that there is a saturation of impacts at high temperatures. These points should be expanded, it is not clear at all from the existing text how this is done.

The text says that the impacts drop below their polynomial on a logistic path once they exceed a certain proportion of remaining GDP. There is a figure and extended discussion in the technical report, and I didn't feel it necessary to repeat this here, but more details can be included if required.

Citing Weitzman (2009) as support for a damage function that introduces a cutoff point for impacts is misleading.

No, it isn't. Weitzman explicitly recommends a cap, which he calls the statistical value of civilization, to prevent even a minute chance of impacts becoming infinite.

Page 7: Can PAGE also produce results that don't use equity weights? I think presenting equity weighted results as done is perfectly fine, but it should be accompanied by results that don't use

equity weighting. Equity weights are based on very strong assumptions about distributional preferences of the social planner, it would be good to have results that are based on efficiency considerations only (and thus ignore distributional issues between regions) accompany the equity weighted results. That way one could tell to what degree the strong distributional preferences of the social planner drive the social cost of carbon estimates.

Equity weighting is included in PAGE09 via the EMUC parameter. It is possible to set this parameter to zero and so produce non-equity-weighted results, and this often makes sense for results in particular years. But the implied discount rate will then just be the PTP rate, which will distort the NPV results.

Page 7: I have serious problems with the fact that the pure time preference rate and EMUC are probabilistic in PAGE. EMUC also is the relative risk aversion parameter in the expected utility framework on which PAGE seems to build, and it just entirely eludes me what it means that the parameter of relative risk aversion is in itself uncertain. It seems to me that by making it uncertain the exercise is most definitely no longer based on expected utility theory. I would also expect that making the pure time preference rate uncertain violates one of the conditions of the expected discounted utility framework. Both choices (making EMUC and the time preference rate uncertain) have the effect that the analysis is no longer based on the well understood and studied framework of expected discounted utility theory, but rather on something else that is not discussed nor clear from the paper.

This is a rather deep issue. In each run of PAGE09, a single value is chosen for the PTP rate and EMUC, and the results for that run are appropriate for that value of PTP and EMUC. But there is debate and disagreement amongst economists about the appropriate values for these parameters. I take the view that it is better to reflect this debate in the model than to arbitrarily choose single values for the parameters. Amongst other things, it allows the influence of the debate about these parameters to be calculated and compared with the influence of uncertainty about other parameters such as the transient climate response, as is done in figure 7. This is a useful result for policy makers.

The expected utility framework is only theoretically watertight for a single decision maker, who would be taken to have a single view about the value of EMUC, and also possibly PTP. But here we are attempting to apply it to a societal problem. So the result for the SCCO₂, for instance, can be thought of as representing the range of values that come from a range of individuals who each have a unique value for PTP and EMUC, none of which can be proven to be incorrect, and each of which a policy maker might want to take into account when deciding upon a climate change tax.

Page 8: An emission scenario that peaks in 2016 seems entirely unrealistic. The scenario should either be removed or backed up by a peer reviewed study.

The scenario is a well-established part of the research effort from the Met office and the Committee for Climate Change in the UK. A paper describing this effort has now been published in Nature Climate Change (Rogelj et al, 2011). There are also other papers going through peer review at Science and Nature Climate Change. I can add these to the references. But this is rather beside the point. The scenario is just one illustrative example of an aggressive mitigation scenario that attempts to limit the rise in global mean temperature to 2 degC. There is no implication that it is optimal.

Page 8: I don't understand the discussion of adaptation. Is adaptation costly in PAGE? If yes, I would like to see a discussion of the cost of adaptation. If not, I don't understand why adaptation is a policy variable, would we not expect it to be always be at the maximum adaptation level then?

Adaptation is costly in PAGE09 as is abatement. But this paper is dealing with the impacts and SCCO2, so the adaptation and abatement costs are not reported.

Page 8: Further on adaptation. If I understand the calibration of PAGE's damage functions correctly it is largely based on a kind of meta review of existing impact studies. Many, if not most, of these studies make assumptions about adaptation already, often they present impacts assuming that agents have adapted optimally already. Does PAGE take that into account in its calibration, or is it taking impact assessments that already assume adaptation, and then adds more adaptation to that?

The possibility of this kind of double-counting does exist. But the impact studies on which the PAGE09 impact functions are based do not include the kind of pro-active adaptation that is described here.

Page 11-2: Unless I misunderstand, both the results in the section "Impacts" and in "NPV of Impacts" are actually NPV estimates. The difference between the two sections is that the first presents impacts per year, whereas the second presents the sum of the impacts in all years. But both sections seem to be NPV figures, so the headline and text should reflect that.

No, the values in the 'impacts' section are not NPV figures, they are undiscounted equity-weighted values.

Page 11-12: Both the "Impacts" and the "NPV Impacts" section compare the equity weighted damage estimates with estimates of the global world product in specific years. That comparison does not make any sense because two different metrics are used to compute the impacts and the global world product. In the computation of the impacts a dollar to a poor person is greatly inflated by the equity weight. But in the computation of the global world product a dollar gets the same weight regardless of who has it. Comparing the impacts with world totals is fine for impact estimates that are not equity weighted. But when equity weighted impact estimates are presented, such a presentation is not valid and a more appropriate metric like a change in the equally distributed equivalent income (Atkinson 1970) is needed.

This is a valid point. The comparison with gross world product is only done to provide some indication of the scale of the impacts; it is not part of the calculation of SCCO2. But it would make more sense to compare to an equity weighted measure of gross world product, not an unweighted one.