CLIMATE CHANGE

A Stern Reply to the Reply to the Review of the *Stern Review*

Richard S. J. Tol & Gary W. Yohe

In previous work, we have argued that a sound economic argument for immediate greenhouse gas emission reduction can be made. We have applauded the *Stern Review* (Stern *et al.*, 2006) for putting this on the public agenda. At the same time, however, we have expressed concern that the *Stern Review* may be right for the wrong reasons. Indeed, we have expressed concern that questionable and essentially incomparable estimates of economic damages and abatement costs makes it easy for climate sceptics and policy critics to dismiss the *Stern Review* altogether (Tol, 2006; Tol and Yohe, 2006; Tol and Yohe, submitted; Yohe, 2006; Yohe and Tol, 2006; Yohe *et al.*, forthcoming). Dasgupta (2007), Nordhaus (forthcoming), Maddison (2006), Mendelsohn (2006), and Weitzman (forthcoming) have all come to essentially the same conclusion. Unfortunately, the recent papers by Dietz *et al.* (2007), Hamid *et al.* (2007) and Anderson (2007) add little to a sound economic basis for international climate policy. We now fear that there is even more confusion about what the *Stern Review* did and did not do.[1]

In the *Summary of Conclusions*, Stern *et al.* (2006) write that the "overall costs […] of climate change will be […] at least 5% of global GDP […] the costs of action […] can be limited to around 1% of GDP […] so prompt and strong action is clearly warranted". In contrast, Dietz *et al.* (2007) argue that the argument for emission abatement "rests neither solely nor even primarily on the aggregate valuation of climate-change impacts". Hamid *et al.* (2007) add that "it is not the social cost of carbon that drives pricing" and argue for a "quantity based target" met in a cost-effective manner. We fully agree. A cost–benefit analysis cannot be the whole argument for abatement.

Richard S. J. Tol is a Senior Research Officer at the Economic and Social Research Institute, Dublin. **Gary W. Yohe** is the Woodhouse/Sysco Professor of Economics at Wesleyan University, Middletown, CT, USA. We have discussed the merits and demerits of the *Stern Review* with many people and we are grateful to all of them. For this paper, Dennis Anderson, Simon Dietz, Chris Taylor and Dimitri Zenghelis provided useful insight, and Chris Hope had helpful comments. All errors and opinions are ours.

[1] An extensive email exchange with Dennis Anderson, Chris Hope, Chris Taylor, and Dimitri Zenghelis did clarify some minor matters, but left our larger questions unanswered.

Uncertainty, equity, and responsibility are other, perhaps better reasons to act. In fact, it has been shown time and again in the earlier literature that a strict cost–benefit analysis, using reasonable parameters, cannot justify substantial emission reduction especially in the near term (Nordhaus, 1991; Kolstad, 1998; Tol, 2005).

Dietz *et al.* (2007) and Hamid *et al.* (2007) nonetheless defend the economic impact estimates of Stern *et al.* (2006). They stand by their questionable choices for the rates of time preference,[2] risk aversion, and inequity aversion. As citizens, they are entitled to their opinions, but one would expect a civil servant to somehow represent the revealed preferences of the average citizen, at least for comparison's sake. The sensitivity analyses now presented are too little, too late. They should have been part of the initial publication so that they would have been part of the public discussion from the very start. Had the *Stern Review* been subjected to a proper peer review, sensitivity analyses would have been included and much of the controversy would have been foregone.

In Tol and Yohe (2006), we noted that the abatement cost estimates of Stern *et al.* (2006) are lower than typical estimates in the literature because the latter are based on cost-minimisation[3] and the former, we think, are not. It followed that the Anderson (2006) model would be an outlier, if it were optimised. We deduced the non-optimality of the Anderson estimates from the fact that the average abatement costs are falling over time, from \$100/tCO_2 in 2005 to \$25/tCO_2 in 2050 (Stern *et al.*, 2006, p. 260).[4] Marginal costs rise over time in an optimal abatement trajectory. Moreover, rising marginal costs typically imply rising average costs (when marginal cost exceeds average cost). Since, Anderson (2007) writes that marginal costs "are 2–4 times higher than the average costs", we see an immediate problem. How can marginal costs rise? At best, they move from \$200/tCO_2 in 2005 to \$100/tCO_2 in 2050. We thus assumed that the Anderson (2006) model was much like other abatement models but followed a non-optimal trajectory.

Let us play devil's advocate to our own argument and see whether marginal costs can rise while average costs are falling. An increasing wedge between average and marginal costs implies that short-run abatement cost curves get steeper over time, but does this make any sense? Can it be explained in terms of technological

[2] Stern *et al.* (2006) assume that there is a 10% chance of extinction of the human race in the 21st century, independent of climate change. Carlo Jaeger pointed out that therefore this problem has a higher priority than climate change.

[3] This literature started with papers by Manne (1976), Nordhaus (1977), and Edmonds and Reilly (1983).

[4] Note that Tol and Yohe (2006) also deduce from this that Stern *et al.* (2006) do not consider capital stock turnover. Indeed, Anderson (2006) mentions this only in passing, and capital stock turnover times, although uncertain, are not part of the Monte Carlo analysis. More importantly, an unannounced carbon tax imposed on a sector with long-lived capital implies a substantial deadweight loss to the economy. Therefore, in the transition period from no carbon pricing to carbon pricing, the carbon tax or permit price should in fact rise faster than the discount rate. We are glad that Anderson (2007) confirms that capital stock effects are included in his model, even though we suspect that they did not feature in the choice of abatement trajectory.

progress, expectations and capital stock turnover? One may argue that in the short run, there are only a limited number of options for emission reduction before one has to turn to premature retirement of capital. In the long run, though, locked-in capital imposes less of a constraint, so the long-run average cost curve for abatement should be flatter than the short-run cost curve. Put another way, many decisions that shape current energy use were made long before climate policy was an issue. As businesses start to anticipate climate policy, however, the short-run abatement cost curve should, over time, become more like the long-run curve—that is, flatter and not steeper. Moreover, technological progress would work in the same direction because cost-saving R&D is more profitable in high-cost sectors.

Despite these problems, let us for the moment continue with the thought experiment that the short-run cost curves for abatement get steeper over time so that average costs can fall while marginal costs rise. The intuition behind this possibility is that technological progress and economic development expand the set of relatively cheap emission reduction options—a reasonable assumption—but that a shrinking share of emissions can only be avoided at prohibitive expense. More to the point, emissions must be essential as an "input" to an equally essential and significant economic activity, and the cost of their removal from the effluent stream, or sequestration elsewhere (e.g., in forests) must be very high—an odd assumption to make across enough of an economy to make a difference in aggregate cost estimates.

Even the arithmetic is difficult to swallow. Suppose that Anderson (2006) conducted a cost-effectiveness analysis, which would be the logical thing to do as Stern *et al.* (2006) do a "cost–benefit analysis" on a limited number of discrete abatement policies only. If marginal costs were to rise with a rate of discount of 2% per year and average costs were to fall by 3% per year (to achieve the quoted 75% reduction over 45 years), then the abatement cost curve must get steeper by a factor of 10 over 45 years. That is, if the short-run abatement cost curve were something like $C = a(t)R^2$ in 2005, then it would have to be something like $C = a(t)R^{20}$ in 2050, where R is emission reduction and $a(t)$ is the unit emission reduction cost in year t. In Stern *et al.* (2006, p. 259), emission reduction increases over time, from some 1% in 2005 to 90% in 2050. If it were optimal to move up an ever steeper cost curve, unit emission reduction cost $a(t)$ has to fall rapidly; with the above numbers, $a(t)$ has to fall by 85% per year, or 37 orders of magnitude in 45 years.[5] To keep marginal

[5] Falling average and rising marginal costs are easier to reconcile when the abatement cost curve does not go through the origin, say $C = b + aR^2$. This may be because there are substantial transaction costs that are independent of the level of emission reduction ($b > 0$), because substantial amounts of energy are wasted and this waste can be eliminated with a minimal effort ($b < 0$), or because there are distortions in the tax system that will be lessened or worsened by abatement policy independent of the level of emission reduction ($b \neq 0$). These are particular assumptions. Furthermore, b would need to fall over time to generate a pattern of increasing abatement with falling average and rising marginal costs.

(footnote continued overleaf)

costs constant, unit emission reduction costs would have to fall by 51% per year, or 15 orders of magnitude in 45 years.[6] As this does not make any sense, we therefore think it is safe to assume that the Stern *et al.* (2006) abatement cost estimates are not minimised.[7] As they are smaller than typical, minimised estimates, it follows that there is a substantial downward bias in the Stern estimates.

Confusingly, Dietz *et al.* (2007) write that "marginal [abatement] costs […] rise through time, in line with a rising marginal damage cost of carbon",[8] which suggests that a cost–benefit analysis with emission reduction as a continuous variable was done. This suggestion is misleading, however. Stern *et al.* (2006) estimate a marginal damage cost of \$85/tCO_2 in 2005, and an average abatement cost of \$100/tCO_2. As the marginal abatement cost is larger than the average abatement costs (by at least a factor of two, according to Anderson, 2007), Stern *et al.* (2006) cannot have done a cost–benefit analysis with marginal damages avoided being set equal to marginal costs of abatement.[9] Regardless of this, marginal abatement costs are said to be *rising*, while the reported numbers suggests that they are falling.

Try as we might, we cannot decipher a coherent story from the reported details that would lead us to conclude, with Dietz *et al.* (2007), that "[m]arginal abatement costs are not falling through time in the Anderson study, they are rising." If marginal costs were rising, Anderson (2006) must have made very peculiar assumptions.[10] If marginal costs were not rising, the trajectory is not optimised. Either way, the cost estimates are suspect.

Moving on, Tol and Yohe (2006) argue that vulnerability to climate change is not constant, and that Stern *et al.* (2006) may therefore have overestimated the impacts of climate change. There are a number of reasons why vulnerability would change: technological progress, a change in the

(Footnote 5 continued)

For example, one may assume that because energy waste is reduced today, more will be wasted tomorrow. We therefore dismiss the option that $b \neq 0$. Note that a cost function of the shape $C = bR + aR^2$ could have averages and marginals moving in different directions, but only for small emission reduction; for large emission reduction, averages and marginals rise and fall together. Reyer Gerlagh pointed out that baseline emissions may not be independent of emission reduction; this may help explain the pattern observed in Stern *et al.* (2006), but then average costs and percent emission reduction are no longer properly defined, as the baseline is not fixed. Strong path dependence and a low discount rate would pose a high premium on early abatement, and may imply that marginal abatement costs fall over time. However, there is little empirical evidence of path dependence of this size. Anderson (2006) does assume learning-by-doing, with an average learning rate of 12%. Riahi *et al.* (2004) make a similar assumption, but still have rising marginal abatement costs (Weyant, 2004).

[6] Note that Dennis Anderson (personal communication, 2007) claims to have used a 10% consumption discount rate. This would be inconsistent with the rest of the *Stern Review*. The assumed rate of technological progress in unit costs would be very large, in fact beyond the numerical precision of our simple model.

[7] The writings of the *Stern* team are ambiguous on this point, and our personal exchanges with them are equally contradictory.

[8] Note that Hamid *et al.* (2007) write the opposite: "it is not the social cost of carbon that drives pricing".

[9] In a personal communication (2007), Dimitri Zenghelis claims that a "non-marginal cost–benefit analysis" was done, but we do not know what that means. Chapter 13 of the *Stern Review* offers only graphs and tables, but no insight into non-Leibnizian calculus.

[10] Note that we unsuccessfully requested Dennis Anderson to show the short- and long-run marginal and average abatement cost curves over time.

composition of the economy, a shift in values, and different options for adaptation. All of these reasons are related to economic growth, which is abundant in the *Stern Review*, especially in the developing world where adaptive capacity has so much room to expand. Dietz *et al.* (2007) only present a sensitivity analysis for different adaptation options that are not tied to underlying socio–economic development. We do not know much about the capacity to adapt to temperature increases above 5 degrees. However, we think that improvements in adaptive capacity, especially in the developing world over the next decades, and certainly before 2100, will play a large role in ameliorating damages. A low discount rate exaggerates the importance of the assumptions about adaptive capacity in the distant future.

Tol and Yohe (2006) argue that catastrophic risks are counted twice. Stern *et al.* (2006, p. 153) write that "[c]atastrophic impacts are modelled in a manner similar to the approach used by Nordhaus and Boyer". Nordhaus and Boyer (2000) estimate a certainty-equivalent catastrophe, using a high rate of risk aversion, while in Hope (2006) "catastrophic risk" consists of three uncertain parameters in a Monte Carlo analysis used to calculate a certainty-equivalent. Dietz *et al.* (2007) argue that Hope (2006) was not based on Nordhaus and Boyer (2000), and closer inspection of Hope (2006) shows indeed little similarity. Instead, Hope (2006) claims that his work is based on Smith *et al.* (2001)[11] even though that paper does not present any economic estimates (only temperature thresholds). It seems, therefore, that the *Stern Review* did not double-count catastrophic risk; but we do not know where the damage estimates came from.

To sum up, we welcome that Dietz *et al.* (2007) and Hamid *et al.* (2007) seem to have abandoned the cost–benefit analysis of Stern *et al.* (2006). Before pushing too hard to advance the argument for immediate action in their revised context, however, they may want to revisit their analysis. Nothing written in Dietz *et al.* (2007), Hamid *et al.* (2007) and Anderson (2007) has enhanced our confidence in the academic quality of the *Stern Review*. We continue to think that the *Stern Review* is right for the wrong reasons, and we would have more faith in a climate policy that is right for the right reasons. We think that there are ample bits of evidence offered in the academic literature and indeed the underlying documentation of the *Stern Review* to make such a case—one that is "bullet-proof" and incontrovertible.

[11] Note that both Tol and Yohe are among the *alii*.

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