



# Copenhagen Consensus on Climate Change

## Perspective Paper – Climate Change

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### I. Background

The four papers challenge papers provide a valuable discussion of the options for addressing climate change. The Tol paper examines the possible role of economic instruments such as carbon taxes; the Galiana and Green paper emphasizes the importance of measures to promote the development of technological low carbon solutions; the Bosello et al. paper looks at the balance between adaptation and mitigation and the Bickel and Lane paper makes a case for investing in finding out more about geo-engineering.

I review each of the papers briefly and in the final section I give my overall assessment of how well they cover the issues that arise when formulating climate policy.

### II. Review of Challenge Papers

#### An Updated Analysis of Carbon Dioxide Emission Abatement as a Response to Climate Change

This paper by Tol provides an excellent review of the state of knowledge on climate economics and notes that the most efficient economic instrument to reduce GHGs is a carbon tax. Some might argue that emissions trading is equally good if not better (Aldy and Stavins, 2008) and politically has a better chance of being implemented so perhaps the case for a carbon tax is not so clear.

The paper surveys the literature on the economic impacts of climate change very well indeed and rightly concludes that: (a) these vary a lot from country to country with negative impacts falling disproportionately on the poor and (b) there is still a great deal of uncertainty about the numbers. On the costs of mitigation he is also right to note that deep cuts are possible and the costs of making these cuts is not that high if the reductions are made progressively, starting from a low base.

The paper then goes on to consider different carbon tax profiles, with a cost in welfare terms of \$75 billion over four years. These give rise to different concentrations of GHGs in 2100. His calculations show a benefit cost ratio of over one for the tax scenarios that are very low in the initial year (less than \$3/ton carbon). However these scenarios result in concentrations that are well above the current consensus level of 450ppm, while the tax scenario needed to get to that level involves a tax of around \$250/ton of carbon and has a benefit cost ratio of only 0.2.

There are several problems with the analysis and the author is aware of them. First it ignores the equity and uncertainty aspects of the problem, taking point estimates of damages through the social costs of carbon. Moreover the estimates he takes (from the FUND model) are on the low side from the range that exists in the literature.

The paper does try to allow for the equity and uncertainty aspects in a modified set of calculations, which could justify an initial tax of around \$12/ton of carbon, with a resulting concentration in 2100 of around 675ppm, but no higher tax is justified. It is not clear how the uncertainty is accounted for in these calculations. The devil really is in the detail here and we need to know the basis of that analysis.

Finally the assumption that the tax will be imposed only for four years and then stopped does not make sense: again the author is aware of that but feels he had to work within structure because that is what the Copenhagen Consensus framework required. Climate change is a longer term problem and short term analyses are not the right way to decide on policy.

### A Technology-led Climate Policy in a Changing Landscape

This paper by Galiana and Green proposes a policy for climate change based exclusively on a carbon tax that is used to fund R&D in new low carbon technologies. The tax starts out at a modest level but increases over time. They claim this one measure is enough and there is no need for any targets for reductions in emissions or for any other measures to increase existing carbon efficiencies.

The authors had put forward five arguments for this approach in earlier work. First they state that the challenge of stabilizing climate requires innovation and development of new technologies to an extent that has been underestimated. Second the current level of technology is not able to cope with the scale of the transformation in low carbon use that is required. Third, subsidies to R&D are required because the incentive from a carbon tax or other measure is not enough to provide enough resources to this activity. Fourth the costs of reducing emissions from current technologies are underestimated. And finally their proposed program has a very high benefit cost ratio.

In this paper they claim the case is as strong as it was before, perhaps even more so given some changes in the landscape for policy making in this area. The changes they cite are: (a) the continuing focus on targets to reduce emissions as a distraction from technology-led solutions, (b) the fact that emerging economies see technology as the “way forward”, (c) the adoption of low carbon options from existing technologies is hampered by the development of indigenous gas and oil from “fracking” that provide energy security at an affordable cost.

There is general agreement that innovation has an important role in the package of instruments to achieve the required transition and most of the serious applied and theoretical work has recognized the need for some subsidies to R&D in this area. Where a number of researchers would disagree with the paper is on the exclusive dependence on this instrument. Here are the main reasons:

- a. Some existing low carbon technologies are becoming competitive with fossil fuels, as a result of learning by doing and by increasing production. The problems alluded to of energy storage are being addressed in technologies such as solar thermal, for example, and will be more effectively resolved through the implementation and trial and error with existing plants. The log linear relationship between unit cost and cumulative production of a good applies to these areas and we can only move down the curve if more plants are installed. This requires some incentives to overcome these learning by doing constraints and spillover externalities. Since a carbon tax is unlikely to be implemented in many countries (in the US it can hardly be mentioned in political circles), other instruments are needed to make progress in this direction.
- b. A sharp increase in government funding for any good or service generally results, in the short term, in an increase in the price of that good or service, more than an increase in the output.

The same applies to R&D. If you push for major increases in R&D the first thing that will happen is an increase in the salaries of researchers and not an increase in the total output. Programs that seek to increase R&D have to be introduced over time to avoid this impact.

- c. The authors are too dismissive of the range of measures introduced to promote low carbon energy so far. Even the Kyoto Protocol resulted in an increase in innovations (see below). The different programs for promoting energy efficiency have resulted in gains and some countries have increased their non-carbon energy shares. One has to compare the present emissions of CO<sub>2</sub> against the counterfactual of what they would have been without any of the measures being introduced, not against what they were 20 years ago.
- d. A carbon tax is probably the best measure to address climate change (although some analysts prefer emissions trading – see my comment on the previous paper<sup>1</sup>). Models that seek to achieve targets based on existing technology **plus** likely developments in technology suggest slightly higher rates than this paper – e.g. the POLES model comes up with around \$8/ton CO<sub>2</sub> in 2010, \$35 in 2020 and \$110 in 2030 – (Criqui et al, 1999, EC, 2003). This is similar to the tax quoted in the paper from the Harvard Belfer Centre on which the authors state:

“The Belfer survey’s need for a high and rapidly rising carbon price to induce the commercialization and development of low carbon energy technologies suggests a lack of confidence that R&D alone can sufficiently reduce the cost of low carbon technologies. In our view, this result is a direct consequence of a stringent emissions target whose timeframe (83% reduction from 2005 by 2050) does not necessarily coincide with the development of scalable technologies.”

I do not think that is right. Certainly in the POLES model (and from what I could see also in the Belfer model) new and unproven technologies are assumed to come in. The fact that there are stringent targets implies that in their absence, and with the taxes the authors propose (they start at \$5/ton of CO<sub>2</sub> in 2010, they would go up to \$20 in 2020 and \$40 in 2030), we would get much lower levels of emissions reduction. This would then result in not meeting a stabilization target that is widely regarded as desirable.

- e. To summarise, the authors are more optimistic about the power of the tax and indifferent to meeting any kind of target. This is a risk many of us are not willing to take. In the meantime, given such taxes are not on the cards, we need to use other instruments and in this regard some are better than others.

### Specific Comments

- a. On the issue of the oil in the ground, I believe that optimal growth models such as DICE do take account of this. The optimisation is carried out subject to the available resources and fossil resources such as oil are included. The estimate social cost of carbon from the model is therefore calculated based on an optimal extraction of remaining stocks.

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<sup>1</sup> If emissions are auctioned the revenues could be used to support R&D research so the desired program could be financed in that way,

- b. The discussion on adaptation is rather casual. In what ways would R&D contribute to the infrastructure solution to adaptation? So far it is not new technologies that are the problem but the uncertainty in knowing when to implement the technologies we have.
- c. I do agree that a consumption based calculation of emissions is the better method of making any national allocations and we should move to that basis.

### Market and Policy Driven Adaptation

This is a very comprehensive and wide ranging paper covering the relative roles of mitigation and adaptation policies to address climate change. It is not so much a case for spending a given amount to achieve the highest return, as an exploration of the factors that determine the forms of intervention for climate policy. Some benefit cost ratios are presented but these are not really returns on a given expenditure over a short period of time; rather they report the discounted present value of reduced damages following a long term investment program. In the case of adaptation much of the investment will be made much later as the paper rightly concludes.

I broadly agree with most of the conclusions of the paper. To summarise they are:

- a. The optimal response to climate change is a combination of mitigation and adaptation – there are no “corner” solutions.
- b. Market-induced adaptation can ease the burden of public adaptation but it will not eliminate it.
- c. The burden of adaptation will fall more heavily in developing countries
- d. In most cases mitigation actions need to be undertaken earlier and adaptation can be undertaken a later. This does not apply, however to all areas of adaptation.

I am not convinced of the conclusion that developed countries should undertake more anticipatory adaptation and developing countries more reactive adaptation. One of the reasons given in the paper is:

“OECD countries are richer. Thus they can give up relatively more easily their present consumption to invest in adaptation measures that will become productive in the future. By contrast non-OECD countries are compelled by resource scarcity to act in emergency”

If the solution being sought is a global cooperative equilibrium it should not matter how poor or rich a country is. The solution should be determined by where the net benefits are greatest.

The modelling of mitigation and adaptation is impressive. Yet there are questions that remain to be addressed at different levels. I will comment on some of these and indicate where I think we need to go to help answer them.

- a. Modelling Adaptation Impacts. It is now common practice to assume an “adaptation function” linking the reduction in damages to expenditure and AD-WITCH using a nested CES, with components consisting of reactive and anticipatory adaptation. Yet the empirical basis for the parameters of this function is very weak. We are highly uncertain about the damages caused by climate change in the first place and we compound that uncertainty when we deal with the changes in damages following any given level of adaptation expenditure. These uncertainties cannot be ignored; they are at the heart of the problem and although they are briefly

mentioned in the paper they are not directly addressed<sup>2</sup>. There are some attempts in the literature to address them. For example see the chapter by Chris Hope on the use of the Page model in Parry et al (2009) in which he looks at adaptation benefits using probability distributions, which is helpful. The authors criticise the Page model on the grounds that the adaptation is not endogenous to the solution in his model. However, the version referred to above does look at the optimal level of adaptation for different mitigation policies and while this does not give an optimal solution to both it does show the trade-offs quite well. If I had to pick between shedding more light on the uncertainties and going for a full optimal control model I would go for the former.

- b. The timing issue is important for the reasons given but also because of a changing knowledge base. As noted, most of the adaptation can be left to later because the lag between the timing of the investment and the impacts is measured in years and not in decades. This also allows us to take advantage of the fact that more information will be forthcoming. The analysis presented here does not build in that aspect. Were it to do so I suspect it would make it even more desirable to go for the low regret options now and postpone action by “buying” an option to act more aggressively in the future. Some work has pointed in that direction (Ranger et al., 2010).
- c. One area where early adaptation action is warranted is land use planning. The model and analysis cannot show that (although the authors do pick this up based on their good common sense). It should be possible to show formally that not addressing this issue now will create patterns of land use in areas more prone to flooding and extreme events, which in turn will entail significantly higher adaptation costs in the future.
- d. Returning to issue of detailed impacts at the sectoral level, a CGE model (ICES) is used to allow for market driven adaptation, which is a valuable contribution to the assessment of climate policies. The results, however, are highly dependent on the structure of that model (many of the parameters of which we do not know) and on the evolution of future trade regimes. The work of Parry and others emphasises how much these scenarios matter (Parry, 2007). Broadly speaking I would expect that a world with a more liberalized trade regime would allow for a higher level of effective market driven adaptation and therefore place a lower overall burden. The distributional effects, however, could be more severe in some regions. In general more information on the ICES model and how it links to AD-WITCH would be helpful.
- e. Another important aspect of adaptation expenditures is equity. We may not be able to justify some proposed actions on benefit cost grounds, but failure to act to address agricultural yield losses in some places could, for example, leave some vulnerable people in a state of destitution. Should this not be taken into account?

#### Specific Comments

- i. In the adaptation versus mitigation debate a question that comes up is how much effect does an effective adaptation program have on the desired mitigation program? If we institute the optimal adaptation plans it should allow us to be more lenient on the GHG reduction targets. In the paper Figure 10 shows the difference in future emissions with and without adaptation: up to 2030 there is little difference but after that the “with adaptation” program allows emissions to

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<sup>2</sup> The authors made some additions in response to comments I sent, including addressing this issue but I could only find a short comment.

rise more so that by 2100 they are about 7 gigatons higher. It would be of considerable interest to see the implications of these differences in terms of CO<sub>2</sub> stabilisation targets and projected temperature increases.

- ii. With coastal zones adaptation the central issues are timing of the investment and uncertainty
- iii. Sea level rise is an important factor in several developing countries
- iv. In the case of health adaptation cost effectiveness analysis is used partly because it is the preferred tool for health sector decisions anyway.
- v. The agricultural adaptation choices are very much influenced by global trade scenarios. These effects need to be studied further.

#### Climate Change: Climate Engineering Research

This paper makes a cogent case for climate engineering as part of the solution for global warming. Paradoxically a strong part of the case is based on the fact that, even though its success is highly uncertain, it can help improve the outcome of an emissions reductions program when climate sensitivity is high (i.e. when the climate outcomes are at their outlying extremes).

The problem with the assessment is that we are dealing with a very uncertain technology, with significant risks. The authors reject the option of evaluating an R&D program to find out more about its potential by using a value of information approach (probably rightly). Instead they assume that investment to scope out one of the climate engineering options (solar radiation management or SRM) will generate a workable technology after ten years. The technology may turn out to have risks and damages but these can be summarized as amounting to damages in the range of 0-3 percent of the world's GDP. The technology is then included in the Integrated Assessment Model (DICE) to see how it affects the optimal solution. The conclusions are that including SRM reduces the cost of the "consensus" solution of stabilization at 2°C considerably. It does this by reducing the costs of abatement (less emissions reductions are needed), while implementing it is relatively a low cost option.

Yet there are unanswered questions that need to be considered. Here are some of them:

- a. The R&D program for SRM may conclude that it is not a viable option. This may be for technical reasons or for global institutional ones. In that even the investment will have no return and this has not been taken into account.
- b. Likewise it may take longer to establish SRM as feasible, in which case its implementation may be delayed. This possibility will reduce the rate of return on the investment.
- c. An important factor to consider is the fact that such injections of sulfur would need global agreement. Given the difficulty we are having on reducing GHGs emissions, it is fanciful to think the political issues could be resolved for an unknown technology such SRM.

### Specific Comments

- a. DICE is not a bad model to use for the comparison but it is conservative in its damage estimates: it does not take account of regional differences and it ends up with less control on radiative forcing and a global higher temperature profile than other Integrated Assessment Models<sup>3</sup>. These will affect the base values for a 2°C stabilization and then the additional gains from SRM, but they should still show that SRM reduces abatement costs.
- b. I would not refer to the final column of Tables 1 and 2 as “Total Damages” but rather as Net Damages, since the abatement costs are undertaken as a policy to reduce the total damages.
- c. It may be worth looking at SRM as an option, which you are buying through the investment proposed and then evaluate its benefit in the case where traditional technologies and prospective innovations do not deliver. This sees SRM as a “last resort” solution, which may be politically more feasible. This would require a different kind of analysis than the ones conducted here.

### **III. Conclusions**

These papers enrich our understanding of the options for addressing the climate change challenge and cover the main forms of intervention. Their main shortcoming is their failure to address two of the most important aspects: public concern about possible major negative impacts and the fact that even modest damages would have serious consequences for the vulnerable and poor members of society. Cost benefit analysis has always tended to be weak on these issues of uncertainty and equity, yet tools have been developed to address them but these tools have either not been used in the challenge papers or if they have, their treatment is unclear.

The consensus for a target stabilization of GHGs at 450ppm is largely based on the precautionary principle – this is the level at which risks of major damage are kept low while the costs of the target are not unduly high. That was the argument in the Stern report, for example and I do not see the challenge papers rebutting it. The Tol paper assumes damage levels that are too low and does not give enough importance to the equity and uncertainty issues. The Galiana and Green paper focuses on an important instrument for reducing GHGs but focusing exclusively on R&D does not pay enough attention to the role of incentives in reducing emissions from energy efficiency improvement and the risk that the technologies may take longer to come up with the required solutions. The Bickel and Lane paper makes a case for R&D on climate engineering but the assessment does not allow enough for the risks that the investment will produce a non-viable technology. Finally the Bosello et al assessment of

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<sup>3</sup> See for example a recent model based on DICE, but which is more disaggregated and is more accurate in its modeling of radiative forcing and emissions is DICER. See Ortiz et al., 2012

adaptation versus mitigation does not take account of the poor state of knowledge on the returns to investment in both these areas.

Knowledge on impacts is improving and when we appraise the introduction of new policies and measures, it would be worth our while to see what we can learn from the success and failure of the instruments that are currently being implemented, instruments such as carbon taxes and emissions trading schemes, R&D subsidies, non-economic measures to promote energy efficiency and the like. They will not work equally well or badly in all places where they are tried and details of how they implemented will influence the estimated benefit cost ratios.

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