The Stern Review: A Dual Critique

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AUTHORS' INTRODUCTION

The twin papers that follow present a critique in two parts of the Stern Review on *The Economics of Climate Change*. Part I focuses on scientific issues and their treatment in the Review. It forms the point of departure for Part II which deals with economic aspects.

The Stern Review was commissioned in July 2005 by the UK's Chancellor of the Exchequer, Gordon Brown. It was conducted under the joint auspices of the Cabinet Office and the Treasury, and the final text was delivered to the Chancellor and the Prime Minister who both spoke at its launching at the end of October 2006. Sir Nicholas Stern is Head of the Government Economic Service in the UK and Adviser to the British government on the economics of climate change. Although the Review was commissioned and financed by Her Majesty's Government, and largely drafted by British officials, it is described as 'independent'.

The Review is a formidable document. Its main text comprises over 550 pages, and covers or refers to a vast range of issues. It reflects the work of a team of over 20 officials under the direction of Sir Nicholas, backed by a substantial number of consultants. It draws on an array of already published studies and papers, as well on a substantial number of specially commissioned outside contributions. In dealing with the economic aspects which form its main concern, it develops a closely constructed argument of is own. On the basis of what it takes to be established science, together with its own distinctive analysis of the economic issues, it draws strong and confident conclusions for policy.

The Review has been widely hailed as an authoritative guide to thinking and policy. It is seen as providing an accurate account of generally agreed and increasingly disturbing scientific conclusions, and as building on these, through solid economic reasoning, an unassailable case for far-reaching and immediate collective action to limit and reduce emissions of 'greenhouse gases' in general and CO_2 in particular. To quote the British Prime Minister, at the launch of the Review,

> … what is not in doubt is that the scientific evidence of global warming caused by greenhouse gas emissions is now overwhelming… [and] … that if the science is right, the consequences for our planet are literally disastrous… what the Stern Review shows is how the economic benefits of strong early action easily outweigh any costs.

In what follows, we take issue with such assured and unqualified verdicts. In relation to both scientific and economic issues, we question the accuracy and completeness of the Review's analysis and the objectivity of its treatment. We thus present a critique of the Review, rather than a full assessment of the argument as a whole.

The subject of the Review is the economics of climate change, and its terms of reference did not require it to cover scientific aspects. However, the text carries substantial sections on these; and it is on the basis of what scientific inquiry is taken to have established that the Review adopts as its starting point for the economic analysis that "climate change… is the greatest and widest-ranging market failure ever seen". The credibility of the Review as a whole thus depends in large part on what it says or presumes about 'the science'. Hence this critique, though it appears in an economic journal, has a scientific as well as an economic dimension.

The analysis that we present below, and the views that we express, are ours alone: they should not be attributed to any of the various institutions that we are affiliated with. We represent no interests, and we have neither sought nor received any financial or institutional support for our work. We write as independent commentators.

The Stern Review: A Dual Critique

PART I: THE SCIENCE

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Introduction

The Stern Review includes an introductory chapter that summarises the present state of climate science and, in Part II, an analysis of the physical and environmental impacts of prospective future paths of climate change. The credibility of the document as a whole thus rests in large part on how far the material presented under these two science headings is accurate and balanced.

Two distinct aspects are relevant here. First, there is the question of whether it can indeed be said, as the Review asserts in its opening sentence, that

> The scientific evidence is now overwhelming: climate change presents very serious global risks, and it demands an urgent global response.

Second, there is the related issue of how far the Stern Review, in the sections that it devotes to them, gives an accurate account of the scientific issues.

We consider that the Review is doubly deficient. The scientific evidence for dangerous change is, in fact, far from overwhelming, and the Review presents a picture of the scientific debate that is neither accurate nor objective.

We present our argument under three main headings. In Section 1 we consider the Review's treatment of basic issues of climate science, and its over-confident conclusions about the prospective course of 'greenhouse gas' concentrations and global warming. In Section 2 we turn to what the Review says about the prospective impacts of the climate changes that it envisages as possible or likely. Under both headings, we note two interrelated features of the Review: First, that it greatly understates the extent of uncertainty, for there are strict limits to what can be said with assurance about the evolution of complex systems that are not well understood. Second, that its treatment of sources and evidence is selective and biased. These twin features combine to make the Stern Review a vehicle for alarmism.

Section 3 is concerned with fundamental issues of scientific conduct and procedure that the Review fails to consider. Professional contributions to the climate change debate very largely take the form of published peer-reviewed articles and studies. It is widely assumed, in particular by governments and the Intergovernmental Panel on Climate Change (IPCC), that the peer review process provides a guarantee of quality and objectivity. This is not so. We note that the process as applied to climate science has tolerated gross failures in due disclosure and archiving, and that peer review is both too inbred and insufficiently thorough to serve any audit purpose, which we believe is now essential for science studies that are to be used to drive trillion-dollar policies.

Besides these three main sections and our summary conclusions in Part 4, we comment in an annex on some aspects of the mishandling of data in the Stern Review. Overall, our conclusion is that the Review is flawed to a degree that makes it unsuitable, if not unwise, for use in setting policy.

1. FLAWS IN THE ALARMIST PARADIGM

The alarmist view of climate science

Sir Nicholas Stern made a revealing comment in his OXONIA lecture of January 2006: "in August or July of last year, [he] had an idea what the greenhouse effect was but wasn't really sure".[1] It seems that, starting from a position of little knowledge of the issues, he has swiftly espoused the official view of the Hadley Centre for Climate Prediction and Research, on

[1] http://www.hm-treasury.gov.uk/media/695/8C/OXONIA_Oxford_31012006.pdf

whose advice the Review relies heavily. But this Hadley Centre picture of reality, though broadly in line with that of the IPCC, is by no means universally held. Many of the specific claims that are endorsed in the Review have been seriously challenged in the scientific literature, while the text plays down the great uncertainties that remain.

The Hadley message, as reflected in the Review, is an alarmist one. It presumes without question that moderate further increases in atmospheric CO_2 levels will give rise to major climatic changes and that these are likely to be seriously damaging; that the climatic changes observed over recent decades can be reliably blamed on emissions of 'greenhouse gases' in general, and CO_2 in particular; and that climate model projections and forecasts present a sufficiently accurate view of the future at relevant geographic and temporal scales to form a basis for major policy decisions.

The Stern Review itself fails to take proper account of the profound uncertainties and major gaps in knowledge of climate science, and neither does it address the many continuing debates regarding climate change mechanisms and impact assessments. Like its sources, the Review gives unwarranted credence to model projections over firmly established data and findings. By exaggerating climate alarm it focuses on implausible rather than likely outcomes, and thereby fails to provide a sound basis for policy.

Mishandling of uncertainty

The Review states on page 10 that: "The analysis of climate change requires, by its nature, that we look out over 50, 100, 200 years and more. Any such modelling requires caution and humility, and the results are specific to the model and its assumptions. They should not be endowed with a precision and certainty that is simply impossible to achieve."

Yet in this respect the Review repeatedly fails to heed its own warning. The tone is set by the Executive Summary which announces without qualification that "These concentrations [of greenhouse gases] have already caused the world to warm by more than half a degree Celsius and will lead to at least a further half degree warming over the next few decades, because of the inertia in the climate system." This is only the first of dozens of unqualified Review statements that attribute causality or state what "will" happen to climate or the biosphere.

A prime element of this unwarranted certainty is the Review's confidence in computer model outputs. Indeed, the Review gives these outputs even more credence than the IPCC, which warned in its Third Assessment Report (TAR) of 2001 that:

> In climate research and modeling, we should recognize that we are dealing with a coupled non-linear chaotic system, and therefore that *the long-term prediction of future climate states is not possible*. The most we can expect to achieve is the prediction of the probability distribution of the system's future possible states by the generation of ensembles of model solutions.[2]

The IPCC has highlighted the "process whereby uncertainty accumulates throughout the process of climate change prediction and impact assessment [which] has been variously described as a 'cascade of uncertainty' (Schneider, 1983) or the 'uncertainty explosion' (Henderson-Sellers, 1993)".[3] There are many levels of cascaded uncertainty, each one contributing to the overall uncertainty. These cascades of uncertainty extend from estimates of relevant location-specific climatic changes to their biophysical and socioeconomic impacts.

The Review attempts to deal with these uncertainties by comparing thousands of model runs under varying assumptions. The model parameterisation chosen takes no account of the possibility that carbon dioxide emissions may have minor or benign effects, and is slanted towards emphasis on larger impacts, feedbacks and damages than even the IPCC has implied to date.

In arguing that the Review has misread the state of the science, we shall challenge some of its specific assertions on climatic mechanisms. In doing so, we do not deny the possibility of future climate risks, especially from natural climate change; nor do we argue that models should only be used if they are able to meet an unrealistic standard of perfection, for their main value is heuristic, not predictive. But we do assert that it is misleading of the Review to draw so predominantly from the upper end of risk distributions and then present these as representative of the range of credible outcomes.

[2] IPCC TAR, Working Group I report, Chapter 14.2.2.2. (Emphasis added.)
[3] "Uncertainties in the IPCC TAR: Recommendations to Lead Authors for More Consistent Assessment and Reporting," cf. http://stephenschneider.stanford.edu/Publications/PDF_Papers/UncertaintiesGuidanceFinal2.pdf. (Emphasis added.)

Climate prediction: is it a mature or a new science?

Some of the unjustified confidence in the Review appears to derive from a perception that climate prediction is a mature branch of science with a pedigree of unchallenged research dating back to work by Fourier in 1827.[4] This is not so. The reality is that climate prediction, far from being a mature science, is a new area that has emerged from the science of weather forecasting, aided by the dramatic increase in power and availability of computers in the last three decades.

In its last Assessment Report, the IPCC still rated the "level of scientific understanding" of nine out of twelve identified climate forcings as "low" or "very low",[5] highlighted the limitations and short history of climate models,[6] and recognised large uncertainties about how clouds react to climate forcing.[7] Since then, major scientific papers have claimed, among other things, that the forcing of methane has been underestimated by almost half,[8] that half the warming over the twentieth century might be explained by solar changes,[9] that cosmic rays could have a large effect on climate,[10] and that the role of aerosols is more important than that of greenhouse gases.[11] Generally speaking, none of these suggestions is included in current climate models though, as mentioned later, aerosols are used, without any proper or rigorous basis, to cancel greenhouse warming which would otherwise be far in excess of what we have experienced.

Moreover, given that the estimated temperature change over the late twentieth century amounted to only a few tenths of a degree, there must be significant doubt as to whether model simulations of external forcings are even required as an explanation. Such minor fluctuations may rather be due to natural, internal, unforced variability. The primary sources of this natural variability are oceans that are never in equilibrium with the

[4] Review, page 7.
[5] IPCC, TAR, Working Group 1, Technical Summary, page 37.
[6] Ibid., pages 48–9.
[7] Ibid., page 49ff.
[8] Shindell, D. T., G. Faluvegi, N. Bell, and G. A. Schmidt (2005), 'An emissions-based view of climate forcing by methane and tropospheric ozone', *Geophysical Research Letters,* **32**, L04803, DOI:10.1029/2004GL021900.
[9] Scafetta, N., and B. J. West (2006), 'Phenomenological solar contribution to the 1900–2000 global surface warming', *Geophysical Research Letters*. DOI: 1029/2005GL025539.
[10] Henrik Svensmark, Jens Olaf P. Pedersen, Nigel D. Marsh, Martin B. Enghoff, and Ulrik I. Uggerhøj (2006), 'Experimental evidence for the role of ions in particle nucleation under atmospheric conditions', *Proceedings of the Royal Society A: Mathematical, Physical and Engineering Sciences.* DOI: 10.1098/rspa.2006.1773.
[11] Kilcik, Ali (2005), 'Regional sun-climate interaction', *Journal of Atmospheric and Solar-Terrestrial Physics,* **67** (16): 1573–1579, November 2005.

surface (because of irregular and poorly understood exchanges between the huge abyssal heat reservoir and the thermocline), together with a turbulent and heterogeneous atmosphere where changing circulation deposits heat in regions with differing infrared opacity. It may be many decades before models can account for this level of complexity, if indeed that ever proves possible.

Exaggerating warming trends

Early in the OXONIA Technical Annex, it was said with unjustified certainty that "The rate and scale of 20th century warming has been unprecedented for at least the past 1,000 years." While the Review backtracks somewhat,[12] the claim raises the issue of context. We have at most a 50-year span of accurate global measurements of temperature and greenhouse gases. Meaningful judgements about climate change and, in particular, natural variations, cannot be made based on such a trivially short time span; even 1000 years is short on the climatic time scale.

The only genuinely global records of measured temperature come from weather balloon radiosonde measurements (since 1958) and satellite microwave sounding units (since 1978). These data, for what they are worth over such short time periods, indicate a gentle warming trend of about 0.1–0.2 degrees C/decade.[13] On a century scale this is at the low end of the trends the Review considers. Moreover, much of the increase in the balloon data is associated with a single step-like event in 1976–77. In the post-1979 interval, the most recently revised satellite data show little change, especially in the tropics and Southern Hemisphere.[14] The trend, such as it is, is at least in part an artifact caused by irregularities such as volcanic eruptions and El Nino events,[15] and anyway—prima facie—it is unalarming in both rate and magnitude. Nor is there any sign of acceleration either in surface or tropospheric data, calling into question the Review's emphasis on outcomes involving decadal trends of 0.3–0.6 degrees C. Despite the accumulation of CO_2 in the Earth's atmosphere

[12] "Recent research, for example from the Ad hoc detection and attribution group (IDAG), uses a wider range of proxy data to support the broad conclusion that the rate and scale of 20th century warming is greater than in the past 1000 years (at least for the Northern Hemisphere)." Review, page 6.
[13] 'Temperature trends in the lower atmosphere: Steps for understanding and reconciling differences', (2006), US Climate Change Science Program.
[14] http://vortex.nsstc.uah.edu/public/msu/t2lt/tltglhmam_5.2
[15] Gray, V. (2006), 'Temperature trends in the lower atmosphere', *Energy & Environment*, **17**: 707–714.

since 1900, and especially since 1950, no global temperature databases exhibit temperature trends of such magnitude. The rates of modern temperature change observed fall well within the rates of minor warmings and coolings inferred for the Holocene in, e.g., the GRIP ice core.[16]

If comparison is made with the 'global average temperature' statistic since 1860 that is computed from near-surface thermometer measurements,[17] then the *late twentieth-century warming is similar in both amount and rate to an earlier (natural) warming between 1905 and 1940*. Comparisons over longer and more climatically relevant time spans have to be made using local proxy datasets. The best such datasets come from ocean seabed and polar ice cap drill cores. For example, the oxygen isotope (proxy air temperature) record from the Greenland GRIP drilling project shows that the late twentieth-century warming represents an intermittent high on a sinusoidal, millennial temperature pattern[18] of possible solar origin.[19] *This record shows that recent warming occurred at a similar rate, but was of lesser magnitude, than the earlier, millennial warmings associated with the Mediaeval, Roman and Minoan warm periods.*

Thus the Review's apodictic claim that "An overwhelming body of scientific evidence indicates that the Earth's climate is rapidly changing, predominantly as a result of increases in greenhouse gases caused by human activities"[20] is without foundation.

Reinventing climate history

Public and governmental concerns over anthropogenic global warming (AGW) soared with the intense and, until recently, continuous media use of a single graph from the IPCC's Third Assessment Report of 2001. This diagram, originally taken from papers in 1998 and 1999 by Mann *et al.*,[21]

[16] Davis, J. C., and G. C. Bohling (2001), 'The search for patterns in ice-core temperature curves', in: Gerhard, L. C. *et al.* (eds), Geological Perspectives of Global Climate Change, American Association of Petroleum Geologists, *Studies in Geology*, **47**: 213–229.
[17] Review, Figure 1.3, page 5.
[18] Grootes, P. M., M. Stuiver, J. W. C. White, S. Johnsen, and J. Jouzel (1993), 'Comparison of oxygen isotope records from the GISP2 and GRIP Greenland ice cores', *Nature*, **366**: 552–554.
[19] Bond, G., B. Kromer, J. Beer, R. Muscheler, M. N. Evans, W. Showers, S. Hoffmann, R. Lotti-Bond, I. Hajdas, and G. Bonani (2001), 'Persistent solar influence on North Atlantic climate during the Holocene', *Science*, **294**: 2130–2136.
[20] Review, page 3.
[21] Mann, M. E., R. S. Bradley, and M. K. Hughes (1998), 'Global-scale temperature patterns and climate forcing over the past six centuries', *Nature*, **392**: 779–787; Mann, M. E., R. S. Bradley, and M. K. Hughes (1999), 'Northern hemisphere temperatures during the past millennium: Inferences, uncertainties, and limitations', *Geophysical Research Letters*, **26**: 759–762.

showed nine centuries of near constant global temperatures followed by a dramatic rise in the twentieth century correlating with the rise in CO_2 concentrations. The Mediaeval Warm Period (MWP), previously believed significantly warmer than now, and the much colder Little Ice Age (LIA) did not appear on this graph, which was dubbed the 'hockey stick' (owing to the shape of its curve) soon after its publication and became the basis of claims that natural climatic variation had been very small for a thousand years.

Other scientists have undertaken temperature reconstructions that are claimed in the Review to corroborate the 'hockey stick', but overlap in the proxies and methods used in these reconstructions casts doubt on their independence. For many, from various disciplines, from the outset the implications of the 'hockey stick' appeared unlikely. Historians and other scientists had documented the LIA, with its frozen Thames, and the flowering of civilizations in the MWP. Taken at face value, these lines of evidence[22] suggest that natural factors played a far more significant role in climate changes than the 'hockey stick' reconstruction suggested. They put in question claims that recent warmth can only be explained by human-induced increases in greenhouse gases.

Despite implying that the debate on the science of climate change is now settled, the Review had no choice but to admit that major doubts exist over the 'hockey stick'. Two recent US reports, one by the National Research Council (NRC) and one by Edward Wegman, Chair of the National Academy of Sciences Committee on Applied and Theoretical Statistics, have invalidated the 'hockey stick' conclusion.[23] These reports have confirmed earlier findings that the hockey-stick shape is an artifact resulting from a combination of defective statistical methods and inclusion

[22] The Medieval Warm Period Project summarises scores of scientific papers on this subject and sets out the resulting temperature histories: see www.co2science.org/scripts/CO2ScienceB2C/data/mwp/mwpp.jsp. The Project's analysis suggests that about 80 per cent of areal studies estimate that peak MWP temperatures exceeded recent warmth.

[23] Wegman concludes that "Overall, our committee believes that Mann's assessments that the decade of the 1990s was the hottest decade of the millennium and that 1998 was the hottest year of the millennium cannot be supported by his analysis." http://energycommerce.house.gov/108/home/07142006_Wegman_Report.pdf. The NRC panel concluded that "uncertainties of the published reconstructions have been underestimated", and confirmed flaws in Mann's methodology: see http://www.house.gov/science/hot/climate%20dispute/NAS%20full%20report.pdf.

of data on bristlecone pine tree-rings, which have been demonstrated to be unreliable as temperature proxies.[24]

While previously the 'hockey stick' study was represented as proof of human-induced climate change, the Review now says in Box 1.1 (our emphasis) "Climate change arguments do not rest on 'proving' that the warming trend is unprecedented over the past Millennium. *Whether or not this debate is now settled*, this is only one in a number of lines of evidence for human induced climate change." However, page 6 then adds that (our emphasis) "Much of the debate over the attribution of climate change *has now been settled* as new evidence has emerged to reconcile outstanding issues." The Review fails to specify this "new evidence" but in any case, attribution studies can never be 'evidence': they are heuristic thought experiments designed to explore possibilities, not provide definitive explanations. Some further problems with such studies are discussed below.

While earlier Stern Review documents cited the 'hockey stick' as valid evidence[25]—which it is not—the Review now treats it as irrelevant. But this also is not a tenable position. Climate models are tuned to the low estimate of natural climate variability put forward by the IPCC in 2001. Were it proved that the world was much warmer in mediaeval times, the models could not replicate this without giving more weight to natural variability and, perforce, their ability to identify anthropogenic forcing would be decreased.

Attribution studies: circular reasoning

The Review's confidence that greenhouse gases are likely to give rise to major, deleterious climate change appears to be based in large measure on the results of a single Hadley Centre paper prominently used in the IPCC

[24] McIntyre, S., and R. McKitrick (2003), 'Corrections to the Mann et al. (1998) Proxy Data Base and Northern Hemisphere Average Temperature Series', *Environment and Energy*, 14 (6): 751–771; McIntyre, S., and R. McKitrick (2005), 'The M&M critique of the MBH98 Northern Hemisphere Climate Index: update and implications', *Energy and Environment*, **16** (1): 69–100.

[25] "So I should say while I had temperature in the previous slide starting in the 19th century, if you send that one a long way back as far as we know, if you send it back another 8 or 9 hundred years it would look pretty flat with oscillations around the level. So that's what has been happening to the stock of carbon dioxide and you can see that it is very suggestive in relation to the story of the temperature and of the science. The relation to human activity: this is the stock of carbon dioxide, this is the flow of carbon dioxide simply from the burning of the fossil fuels, so that is the direct link with the human activity." OXONIA Lecture, op. cit.

WG1 Third Assessment Report.[26] However, as can be seen from the Assessment Report, in order to simulate observed trends in global mean surface temperature, the Hadley Centre had to eliminate about two-thirds of the anthropogenic greenhouse forcing with countervailing aerosols (the net result being referred to as anthropogenic forcing). That is to say, the model—like others of its kind—exaggerates the actual warming which was only a few tenths of a degree. Further, as leading researchers in aerosol science reported in *Science*,[27] the aerosol forcing is so poorly known that they felt that calculating how much aerosol forcing is needed to cancel greenhouse forcing is as good a way of estimating the aerosol forcing as any. At the same time, the IPCC's use of this level of uncertainty to claim that the model had simulated observations is self-evidently circular. In actuality, even the sign of aerosol forcing is unknown. In a more rational and less politicized environment, one would at least entertain the simplest resolution of the problem: namely, that the models are exaggerating the response to anthropogenic greenhouse forcing.

The circular reasoning that characterizes attribution studies based on deterministic modeling of presumed forcings undermines claims that they prove warming could only be caused by those forcings. The former Director of Research at the Royal Netherlands Meteorological Institute, Dr Hendrik Tennekes[28] recently pointed out that:

> [T]hose that advocate the idea that the response of the real climate to radiative forcing is adequately represented in climate models *have an obligation to prove that they have not overlooked a single nonlinear, possibly chaotic feedback mechanism that Nature itself employs*….[T]he task of finding all nonlinear feedback mechanisms in the microstructure of the radiation balance probably is at least as daunting as the task of finding the proverbial needle in the haystack.

Even the IPCC cautioned in relation to the Hadley attribution study that "These results show that the forcings included are sufficient to

[26] See Figure A1 in the OXONIA Technical Annex available at www.hm-treasury.gov.uk/media/695/0E/OXONIA_Technical_Annex_FINAL.pdf, where the source is given only as "Hadley Centre (as reported in IPCC 2001)". The original paper was Stott P. A., S. F. B. Tett, G. S. Jones, M. R. Allen, J. F. B. Mitchell, and G. J. Jenkins (2000), 'External control of twentieth century temperature by natural and anthropogenic forcings', *Science*, **290**: 2133–2137.

[27] Anderson, T. L., R. J. Charlson, S. E. Schwartz, R. Knutti, O. Bucher, H. Rhode, and J. Heitzenberg (2003), 'Climate forcing by aerosols—a hazy picture', *Science*, **300**: 1103–1104.

[28] Published on the Roger Pielke, Sr. Research Group Weblog at: http://climatesci.atmos.colostate.edu/2006/01/06/guest-weblog-reflections-of-a-climate-skeptic-henk-tennekes/. Dr Hendrik Tennekes, prior to retirement has been Director of Research, Royal Netherlands Meteorological Institute; Professor of Aerospace Engineering at Pennsylvania State University; and Professor of Meteorology at the Free University, Amsterdam. (Emphasis added.)

explain the observed changes, *but do not exclude the possibility that other forcings may also have contributed*."[29] The Review, however, disregards these warnings and flatly asserts that "more than a decade of research and discussion…has reached the conclusion there is *no other plausible* explanation for the observed warming for at least the past 50 years".[30]

Though the Review neither mentions nor discusses them, several other plausible explanations of recent warming have been advanced in the professional literature. One line of research has correlated recent temperature trends with local heating caused by urbanization and industrialization.[31] Other studies using longer-term geological evidence also suggest minimal impacts from greenhouse gas forcing. One of these concludes that:

> …the global warming observed during the latest 150 years is just a short episode in the geologic history. The current global warming is most likely a combined effect of increased solar and tectonic activities and *cannot be attributed to the increased anthropogenic impact on the atmosphere*. Humans may be responsible for less than 0.01°C (of approximately 0.56°C total average atmospheric heating during the last century).[32]

The Review fails to refer to any of this research, the very existence of which contradicts claims that the science is settled or that GHG forcing is needed to explain current warming. It also fails to notice that models trained to emulate climate using both the instrumental record and long-term geological evidence—e.g. the last 140 years of surface temperature measurements,[33] the last 5,000 years of proxy climate data from a Caribbean marine core and a South African speleothem,[34] or the 100,000 year-long GRIP ice core[35]—are not only successful in 'predicting' the current warming phase, but also suggest cooling over the next few decades. This conclusion has also recently been strengthened on a more analytical

[29] IPCC, TAR, Working Group 1, Summary for Policymakers, page 10. (Emphasis added.)
[30] Review, page 3. (Emphasis added.)
[31] de Laat, A. T. J., and A. N. Maurellis (2004), 'Industrial CO_2 emissions as a proxy for anthropogenic influence on lower tropospheric temperature trends', *Geophysical Research Letters*, **31**, L05204, DOI:10.1029/2003GL019024; Kalnay, E., and M. Cai (2003), 'Impact of urbanization and land use change on climate', *Nature*, **423**: 528–531; Hale, R. C., K. P. Gallo, T. W. Owen, and T. R. Loveland (2006), 'Land use/land cover change effects on temperature trends at U.S. Climate Normals stations', *Geophysical Research Letters*, **33**, L11703.
[32] Khilyuk, L. F., and G. V. Chilingar (2006), 'On global forces of nature driving the Earth's climate. Are humans involved?', *Environmental Geology*, **50**: 899–910.
[33] Klyashtorin, L. B., and A. A. Lyubushin (2003), 'On the coherence between dynamics of the world fuel consumption & global temperature anomaly', *Energy & Environment*, **14**: 733–782. (Emphasis added.)
[34] Loehle, C. (2004), 'Climate change: detection and attribution of trends from long-term geologic data', *Ecological Modelling*, **171**: 433–450.
[35] Kotov, S. R. (2001), 'Near-term climate prediction using ice-core data from Greenland', in: Gerhard, L. C. *et al.* (eds), Geological Perspectives of Global Climate Change, American Association of Petroleum Geologists, *Studies in Geology*, **47**: 305–315.

basis by NASA and the Russian Academy of Sciences, both of which have issued predictions that cooling will occur early in the twenty-first century as solar activity decreases.

Carbon dioxide in perspective

It is important to distinguish CO_2 emission levels, CO_2 concentrations in the atmosphere, and climate forcing. It is the last that is directly relevant to the purported problem of warming. Emission reductions proposed by the Kyoto Protocol would have only a minuscule effect on atmospheric concentrations, while increments in these concentrations would anyway have a diminishing impact on climate forcing. A doubling of CO_2 is used as a benchmark for climate sensitivity and represents a forcing of about 3.7 Watts per square meter. Since anthropogenic greenhouse forcing is already estimated at about 2.7 Watts per square meter—a little over half due to CO_2, with about half of the rest to methane—then in terms of climate forcing, we are already about three quarters of the way to an effective doubling of CO_2, yet we have experienced much less warming than such forcing would suggest. *The Review assumes, against all empirical evidence and physical reasoning, that future increments of CO_2 will have substantially greater effects than those in the past.*

Changes in the CO_2 concentration are not well correlated with the 0.6 degree C increase exhibited by the surface thermometer 'global average temperature' estimates during the twentieth century. First, the phase of temperature increase between 1905 and 1940 occurred before any greatly increased industrial emissions of CO_2. Second, the rapid post-1940 increase in CO_2 emissions was accompanied by a falling temperature between 1945 and 1965. The hockey-stick curve had the striking property that its heavy smoothing and axis-scaling visually diminished these matching problems, and led to a much more plausible-looking match between the alleged temperature changes and actual CO_2 curves. Even the direction of causality is open to question. Data from ice cores indicate that, during ancient climate changes, increases in temperature preceded parallel increases in CO_2 by at least hundreds of years.[36]

[36] Mudelsee, M. (2001), 'The phase relations among atmospheric CO_2 content, temperature and global ice volume over the past 420 ka. quaternary', *Science Reviews*, **20**: 583–589; Siegenthaler, U., T. Stocker, E. Monnin, D. Luthi, J. Schwander, B. Stauffer, D. Raynaud, J.-M. Barnola, H. Fischer, V. Masson-Delmotte, and J. Jouzel (2005), 'Stable carbon cycle–climate relationship during the late Pleistocene', *Science*, **310**: 1313–1317.

This brings us to the matter of feedbacks. It is generally calculated that a doubling of CO_2 would, other factors kept constant, result in a global mean warming of about 1 degree C. Alarming predictions all require that water vapour and clouds act so as to greatly amplify the impact of CO_2. But it is freely acknowledged, including by the IPCC, that water vapour and especially clouds are poorly modelled, while the underlying physics for determining their behaviour is missing or even unknown. The governing equations of fluid dynamics (Navier-Stokes) have resisted solution for over 100 years; indeed the Clay Institute is offering a $1 million prize to anyone who can merely prove a solution exists. The Review's glib treatment of this fundamental issue again spotlights its failure to grasp the uncertainty of climate research.

The Review's only substantive remarks on water vapour feedback[37] turn out to be irrelevant. These relate to Lindzen's 1990 suggestion for a mechanism whereby a warmer surface might lead to a drier tropopause region, even though it has long been shown that changes in water vapour at these levels would have marginal impact on climate.[38] To be sure, water vapour near the surface (where the bulk of the atmosphere's water vapour is found) is also relatively unimportant. Rather, it turns out that water vapour near the middle of the troposphere dominates this feedback. Thus, the 2005 Soden reanalysis of trends in upper atmosphere water vapour,[39] which the Review advances as a definitive refutation of Lindzen's 1990 suggestion, does not relate to any important feedback. More important, it has long been noted that the water vapour and the related cirrus cloud distribution are extremely spatially heterogeneous with distinct moist/cloudy and dry/clear regions. The restriction to clear regions (as is, in fact, done in Soden's study) is unlikely to be meaningful on this count either. For some time now it has been recognized that the real feedback in the atmosphere likely consists in simply changing the relative areas of moist/cloudy and dry/clear regions.[40] Much recent work supports the existence of such a

[37] Review, page 7, footnote 17. This misidentifies Lindzen's paper as "Lindzen 2005". The references section misidentifies it as Lindzen's 2001 paper on the Iris Effect. The actual suggestion addressed by Soden's analysis was contained in Lindzen, R. S. (1990), 'Some coolness concerning global warming', *Bull. Am. Met. Soc.*, **71**: 288–299.

[38] See, for example, Shine, K. P., and A. Sinha (1991), 'Sensitivity of the Earth's climate to height dependent changes in the water vapor mixing ratio', *Nature,* **354**: 382–384; and Sun, D.-Z., and R. S. Lindzen (1993).

[39] Soden, B. J., D. L. Jackson, V. Ramaswamy, M. D. Schwarzkopf, and X. Huang (2005), 'The radiative signature of upper troposphere moistening', *Science,* **310** (5749): 841–844.

[40] Udelhofen, P. M., and D. L. Hartmann (1995), 'Influence of tropical cloud systems on the relative humidity in the upper troposphere', *J. Geophys. Res.,* **100**: 7423–7440; Lindzen, R. S. (1997), 'Can increasing atmospheric CO_2 affect global climate?', *Proc. Natl Acad. Sci. USA,* **94**: 8335–8342; Lindzen, R. S., M.-D. Chou, and A. Y. Hou (2001), 'Does the Earth have an adaptive infrared iris?', *Bull. Amer. Met. Soc.,* **82**: 417–432.

mechanism, the strength of such a mechanism, and the failure of current models to replicate the data from which such conclusions emerge.[41] Much new research is currently in progress. The process (sometimes referred to as the Iris Effect), it should be noted, would reduce sensitivity to a doubling of CO_2 to less than 0.5 degrees C—rather more consistent with observations.

The Review is too confident and unqualified in assigning an overriding role to greenhouse gases in determining climate. Its approach ignores observational facts and cherry-picks among papers that promote alarm.

2. OVERSTATING CLIMATE IMPACTS

The same pattern of alarmism is apparent in the Review's treatment of climate impacts, for these impacts are made to appear dire by the introduction of two systematic biases. The first is the choice of scenarios. The studies of impacts used in the Review are based largely on four of the 40 scenarios developed by the IPCC.[42] They thus omit two of the six "illustrative" scenarios chosen by the IPCC as "equally sound".[43] The missing scenarios are both from the A1 "very high growth" family: A1B (Balanced) and A1T (predominantly non-fossil fuels). The only A1 scenario used by the Review is the extreme A1FI (fossil fuel intensive) scenario,[44] which yields a central estimate of warming in the twenty-first century of 4.33°C, compared to 2.79°C for scenario A1B and 2.38°C for A1T.[45]

In addition to focusing on the highest of three emissions scenarios that assume rapid global economic growth and ignoring the other "very high" economic growth scenarios that yield much lower warming projections, the Review selects IPCC scenario A2 as its base case.[46] This scenario

[41] Clement, A. C., and B. Soden (2005), 'The sensitivity of the tropical-mean radiation budget', *J. Clim.*, **18**: 3189–3203; Choi, Yong-Sang, and Chang-Hoi Ho (2006), 'Radiative effect of cirrus with different optical properties over the tropics in MODIS and CERES observations', *Geophys. Res. Ltrs.*, in press; Chou, M.-D., and R. S. Lindzen (2005), 'Comments on "Examination of the Decadal Tropical Mean ERBS Nonscanner Radiation Data for the Iris Hypothesis"', *J. Clim.*, **18**: 2123–2127.
[42] IPCC Special Report on Emissions Scenarios, 2000; summary available at www.ipcc.ch/pub/sres-e.pdf.
[43] Ibid, p. 4.
[44] Review, page 61.
[45] IPCC WG1 TAR, page 552, available here: www.grida.no/climate/ipcc_tar/wg1/552.htm.
[46] Review, Box 6.1, page 154.

projects global population in 2100 at 15 billion.[47] But according to the International Institute for Applied Systems Analysis, there is only a 2.5% probability that world population will exceed 14.4 billion in 2100.[48] Thus, the A2 population projection is considered highly unlikely by the research institute that prepared it. This is not surprising, since the A2 estimate for 2100 is more than 50% above the UN's latest medium population scenario and 7% above its high scenario.[49] This inflated population estimate inflates emissions and, more important, the numbers at risk for each of the climate-sensitive hazards examined in the Review, and hence the consequences and costs of dealing with them.

A second systematic bias in the Review's consideration of climate impacts is its reliance on papers that assume either that human beings will take no countermeasures to combat adverse impacts of climate change, or that any measures they do take will utilize existing technologies. In fact, we can confidently expect improved technologies in the wealthier and more technologically advanced worlds that will eventuate, and are indeed depicted by IPCC's scenarios.

In these and other ways, the Review's consideration of various climate impacts is biased towards damaging or disastrous outcomes. Some specific examples follow.

Hunger and agricultural productivity

The studies cited by the Review under this heading can be traced mainly to a paper by Parry *et al.*[50] This study allows for some adaptations and increased use of existing technology that would improve productivity. But it explicitly excludes any technologies that may be developed specifically to cope with negative impacts of climate change.[51] This is not a sound procedure. The potential for future technologies, including biotechnology, to cope with climate change is large even in developing countries, especially

[47] Review, Box 3.2, page 61.
[48] See http://www.iiasa.ac.at/Research/POP/proj01/index.html?sb=5; Lutz, W., W. C. Sanderson, and S. Scherbov (eds) (2004), *The End of World Population Growth in the 21st Century: New Challenges for Human Capital Formation and Sustainable Development* (London: Earthscan).
[49] UN Population Division (2004), *World Population to 2300* (New York: United Nations).
[50] Parry, M. L., C. Rosenzweig, I. Iglesias, M. Livermore, and G. Fischer (2004), 'Effects of climate change on global food production under SRES emissions and socio–economic scenarios', *Global Environmental Change*, **14** (1): 53–67.
[51] Ibid., page 57.

given the prospective continuing increases in their per capita income. Thus, the abrupt declines in yields predicted by the Review once certain temperature thresholds are reached are unlikely given appropriate breeding, crop switching and other adaptations in the decades during which temperature might be rising towards these thresholds.[52] Most other threats to agriculture and food supply, e.g., waterlogging, drought, and salinity, have also to be weighed in the light of the obvious possibilities for adaptation.

The approach used in Parry *et al.* to estimate the impacts of climate change decades from now is, in essence, tantamount to estimating today's level of hunger (and agricultural production) based on the technology of fifty years ago. Past prognostications made along these lines have proven to be spectacularly wrong precisely because they omitted from consideration developments in agricultural technology that occurred in subsequent decades.[53]

Another source of the Review's overestimates of future levels of hunger is its treatment of the prospective fertilisation of crops by additional carbon dioxide. The Review says that, following Parry, it assumes that carbon fertilisation is "weak" and "smaller than previously thought".[54] Close scrutiny of the Review's footnotes is required to descry the fact that the actual assumption is not weak fertilisation but "*no fertilisation effect*".[55] The basis for this assumption, which flies in the face of numerous papers on the reality of carbon fertilisation, is a recent paper (Long *et al.*, 2006), which suggests only that under field conditions, carbon fertilisation may be a third to less than half of what is suggested by experiments using growth chambers.[56] The Review's effective assumption of no carbon fertilisation, which is wholly unrealistic, allows it to make a headline projection that "250–550 million additional people may be at risk"[57] of hunger, whereas, on its own figures, an assumption of strong fertilisation would have

[52] See Goklany, I. M. (2001), *The Precautionary Principle: A Critical Appraisal of Environmental Risk Assessment* (Washington, DC: Cato Institute).
[53] Recall, for example, the inaccuracy of the catastrophic warnings in Paul Ehrlich's *The Population Bomb* (1968).
[54] Review, pages 67–8; Box 3.4 (page 70). Figure 3.6 on page 73 shows the huge impact of this assumption of weak fertilisation on projected numbers of hungry people. The numbers under the A2 scenario, used by the Review as a base case, are also far higher than under any other scenario.
[55] Review, page 72, footnote 43.
[56] Review, page 67, footnote 35; Long, S. P., E. A. Ainsworth, A. D. B. Leakey, *et al.* (2006), 'Food for thought: lower-than-expected crop yield stimulation with rising CO_2 concentrations', *Science*, **312**: 1918–1921.
[57] Review, page 72.

suggested declining numbers of hungry people, even for a temperature increase of up to 3.5 degrees C.[58]

Ecosystems and extinction risks

The Review acknowledges that much of the "information" furnished with regard to impacts on ecosystems and extinction risks that it quotes originates with Thomas *et al.* (2004) and concedes that there is a "great deal of uncertainty inherent in such estimates".[59] This acknowledgement, however, is offered only several pages after the results of the Thomas *et al.* study have been highlighted in the Executive Summary, and in Key Messages for Part II and Chapter 3. Moreover, the Review uses these estimates repeatedly and often without any qualification. For example, Figure 2 of the Executive Summary notes "Many species face risk (20–50% in one study)," but it fails to note the uncertainties associated with that "one study". Similarly, the Executive Summary states that "Ecosystems will be particularly vulnerable to climate change, with around 15–40% of species potentially facing extinction after only 2°C of warming."[60] Here, as elsewhere, the reader is not warned that this statement is based on a single study, which, moreover, is fraught with uncertainties.[61]

After finally acknowledging the substantial uncertainty associated with the Thomas *et al.* (2004) study, the Review attempts to justify its use by saying that "other studies looking at climate suitability also predict high levels of extinction".[62] But many of the problems inherent in the Thomas *et al.* study are also endemic to these other studies. A basic issue is whether such climate suitability studies are even able to predict extinction risks under different climatic regimes. For each such regime, atmospheric concentrations of CO_2, rates of plant growth, water use efficiency, the energy requirements of species and their predator–prey relationships would all be different from what they are today.[63] As noted by Schwartz *et al.* (2006),

[58] Figure 3.6 on page 73.
[59] Review, page 80, footnote 79.
[60] Review, page vi.
[61] For other notable examples in the Review of failure to identify the single-study basis of these conclusions, see the cover page and Key Messages of Part II (pp. 55 and 56), and Table 3.1 on page 57.
[62] Review, page 80, footnote 79.
[63] See Pearson R. G., and T. P. Dawson (2003), 'Predicting the impacts of climate change on the distribution of species: Are bioclimatic envelope models useful?', *Global Ecology and Biogeography*, **12**: 361–371; Guisan, A., and W. Thuiller (2005), 'Predicting species distributions: Offering more than simple habitat models', *Ecology Letters*, **8**: 993–1009.

"the efficacy of using bioclimatic models to assess the possible extinction potential of climate change, particularly among species with small distributions, requires empirical assessment", while claiming that climate change puts a particular endemic species at risk of extinction "requires a detailed understanding of the responsiveness to climate of the target species, as well as that of species with which it is likely to interact".[64]

The Review also ignores what has been written about the likelihood that carbon fertilisation, and other factors likely to extend secular increases in agricultural productivity, will reduce habitat loss and increase water use efficiency of plants, thereby reducing pressures on ecosystems and biodiversity.[65] Lower habitat loss would also conserve migration corridors, something that has been advanced as a mechanism to aid species adapt to changed circumstances. Changes in forest productivity (because of higher CO_2 concentrations, for instance) would similarly promote biodiversity. Thus it is conceivable, indeed probable, that at low to moderate levels of climate change, the overall pressure on biodiversity, ecosystems and species would on balance be lower.[66] In sum, the Review's assessment of ecosystem and extinction risks are a worse-than-worst-case scenario, based on a naïve and one-sided appeal to the literature.

Water availability and water shortages

With respect to water supplies and water availability, the Review's information is based mainly on Arnell's studies which indicate that although aggregate populations under water stress through the 2080s—the period considered—may decline, people in some regions could have greater water shortages, while others may have too much water during the rainy season which could lead to both flooding and water shortages during other seasons.[67]

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[64] Schwartz, M. W., L. R. Iverson, A. M. Prasad, S. N. Matthews, and R. J. O'Connor (2006), 'Predicting extinctions as a result of climate change', *Ecology*, **87**: 1611–1615.
[65] Idso, S. B., and A. J. Brazel (1984), 'Rising atmospheric carbon dioxide concentrations may increase streamflow', *Nature*, **312**: 51–53; Gedney, N., P. M. Cox, R. A. Betts, O. Boucher, C. Huntingford, and P. A. Stott (2006), 'Detection of a direct carbon dioxide effect in continental river runoff records', *Nature*, **439**: 835–838; Goklany, I. M. (1998), 'Saving habitat and conserving biodiversity on a crowded planet', *BioScience*, **48**: 941–953.
[66] Goklany, I. M. (2001), *The Precautionary Principle: A Critical Appraisal of Environmental Risk Assessment* (Washington, DC: Cato Institute); Goklany, I. M. (2003), 'Relative contributions of global warming to various climate sensitive risks, and their implications for adaptation and mitigation', *Energy & Environment*, **14**: 797–822.
[67] Arnell, N. W. (2004), 'Climate change and global water resources: SRES emissions and socio–economic scenarios', *Global Environmental Change*, **14** (1): 31–52; Arnell, N. W. (2006), 'Climate change and water resources: A global perspective', in: Schellnhuber, H. J., *et al.*, *Avoiding Dangerous Climate Change* (Cambridge: Cambridge University Press, pp. 167–175).

But the magnitude of these adverse outcomes is exaggerated, since Arnell's papers ignore even the adaptation possible with existing technologies, let alone possibilities from new and improved technologies.[68] No account is taken of the fact that human beings have had a long, and mainly successful, history of combating floods as well as dealing with erratic water flows through a variety of supply and demand side adaptations.[69]

Melting ice sheets

The Review's comments concerning Greenland ice melt are similarly slanted. The text repeatedly emphasizes "significant melting and an acceleration of ice flows near the coast"[70] and hammers the possibility of "irreversible" melting of the Greenland ice sheet.[71] Yet, of the four papers relied on, two, based on satellite altimetry, show a slight net gain in the mass of the Greenland ice sheet (over 1992–2002 and 1992–2003), since although the ice margins of Greenland are shrinking, ice is building up inland due to higher snowfall.[72] A third paper, using data from 1996 to 2005, indicates a net loss of ice mass.[73] The fourth study, which uses meteorological models to estimate the overall mass balance of the ice sheet, finds no significant trend from 1961 to 2003.[74] None of these data has been gathered for a sufficiently long period to enable us to discern whether they constitute short-term fluctuations or long-term trends, let alone for us to identify their causes. We note, however, that papers based on longer data series have found that the temperature around the Greenland coast, while it may have risen just in the last few years, is still lower than it was around

[68] Page 16 in: Warren, R., N. Arnell, R. Nicholls, P. Levy, and J. Price (2006), 'Understanding the regional impacts of climate change', research report prepared for the Stern Review, Tyndall Centre Working Paper 90 (Norwich, UK: Tyndall Centre, available from http://www.tyndall.ac.uk/publications/working_papers/twp90.pdf).
[69] Goklany, I. M. (2003), 'Relative contributions of global warming to various climate sensitive risks, and their implications for adaptation and mitigation', *Energy & Environment*, **14**: 797–822; Tol, R. S. J. (2005), 'Adaptation and mitigation: trade-offs in substance and methods', *Environmental Science & Policy*, **8**: 572–578.
[70] Review, page 16; also pages v, 2, 14, 56, 57, 59, 81, 82, 84, etc.
[71] Review, pages v, 81, 82, etc.
[72] Zwally, H. J., M. B. Giovanetto, J. Li, H. G. Cornejo, M. A. Beckley, A. C. Brenner, J. L. Saba, and D. Yi (2005), 'Mass changes of the Greenland and Antarctic ice sheets and shelves and contributions to sea-level rise: 1992–2002', *Journal of Glaciology*, **51** (175): 590–527; Johannessen, O. M., K. Khvorostovsky, M. W. Miles, and L. P. Bobylev (2005), 'Recent ice-sheet growth in the interior of Greenland', *Sciencexpress*: www.sciencexpress.org, 20 October 2005.
[73] Rignot, E., and P. Kanagaratnam (2005), 'Changes in the velocity structure of the Greenland Ice Sheet', *Science*, **311**: 986–990.
[74] Hanna, E., P. Huybrechts, I. Janssens, J. Cappelin, K. Steffen, and A. Stephens (2005), *Journal of Geophysical Research*, **110**: 10.1029/2004JD005641.

1940,[75] and little changed from the very first instrumental measurements in the 1780s.[76]

The Review also fails to mention that temperatures in the Arctic as a whole are only as warm now as they were in the 1930s,[77] or that the much larger Antarctic ice sheet is growing.[78] A continual build-up of snow and ice on the continent will have a tendency to lower mean global sea level.

General health impacts

The estimates presented in the Review for the present day health impacts of climate change and increases in such impacts through 2030 due to a 1 degree C increase in temperature[79] can be traced directly, or indirectly through Patz *et al.* (2005), to McMichael *et al.* (2004).

Evidence of bias can be seen in McMichael's explanation of his method:

> …climate change occurs against a background of substantial natural climate variability, and its health effects are confounded by simultaneous changes in many other influences on population health….Empirical observation of the health consequences of long-term climate change, followed by formulation, testing and then modification of hypotheses would therefore require long time-series (probably several decades) of careful monitoring. *While this process may accord with the canons of empirical science, it would not provide the timely information needed to inform current policy decisions on GHG emission abatement*, so as to offset possible health consequences in the future. *Nor would it allow early implementation of policies for adaptation to climate changes.*[80]

In other words, the estimates in this paper are based not on robust science but on a desire to be policy-relevant. The unquestioning use of the

[75] See, for example, Chylek P., J. E. Box, and G. Lesins (2004), 'Global warming and the Greenland ice sheet', *Climatic Change*, **63**: 201–221.
[76] Vinther, B. M., K. K. Andersen, P. D. Jones, K. R. Briffa, and J. Cappelen (2006), 'Extending Greenland temperature records into the late eighteenth century', *Journal of Geophysical Research*, **111**, 10.1029/2005JD006810.
[77] Polyakov, I. V., G. V. Alekseev, R. V. Bekryaev, U. Bhatt, R. L. Colony, M. A. Johnson, V. P. Karklin, A. P. Makshtas, D. Walsh, and A. V. Yulin (2002), 'Observationally based assessment of polar amplification of global warming', *Geophysical Research Letters*, **29**: 10.1029/2001GL011111.
[78] Wingham, D. J., A. Shepherd, A. Muir, and G. J. Marshall (2006), 'Mass balance of the Antarctic ice sheet', *Philosophical Transactions of the Royal Society—A*, **364**: 1627–1635; Van de Berg, W. J., M. R. van den Broeke, C. H. Reijmer, and E. van Meijgaard (2006), 'Reassessment of the Antarctic surface mass balance using calibrated output of a regional atmospheric climate model', *Journal of Geophysical Research*, **111**: 10.1029/2005JD006495; Vaughn, D. G. (2005), 'How does the Antarctic ice sheet affect sea level rise?', *Science*, **308**: 1877–1878.
[79] Review, pages 75–6.
[80] McMichael, A., *et al.* (2004), 'Global climate change', in: *Comparative Quantification of Health Risks: Global and Regional Burden of Disease due to Selected Major Risk Factors* (World Health Organization, Geneva, page 1546). (Emphases added.)

McMichael, Patz and WHO studies that have explicit policy concerns is further evidence of partiality and bias.

Malaria and dengue fever

Most of the Review's disease projections are based on Tanser *et al.* (2003), van Lieshout *et al.* (2004) and Hales (2002). Importantly, none of these authors takes account of future changes in technology and increases in adaptive capacities of developing nations as they become richer.[81] Van Lieshout *et al.*, for instance, factor in adaptive capacity as it was in 1990 but they do not allow for improvements in adaptive capacity that can be expected to occur between 1990 and 2085.[82] Notably, Tol and Dowlatabadi (2001) estimate that malaria is functionally eliminated in a society once annual per capita income reaches $3,100, which is substantially below the average that has been projected in the future for today's developing countries under the poorest (A2) scenario.[83] This is consistent with the basic fact that techniques to eradicate these diseases have been available for decades, so that they are now diseases of poverty, not of climate or climate change.[84]

Extreme weather

In his earlier response to critics in this journal,[85] Sir Nicholas Stern stated that many uncertainties had been resolved in favour of alarm, but that "one remaining controversy" existed about the "attribution of current weather events to human-induced climate change". He was wrong on both counts, since while significant uncertainty remains in many areas of climate science, it is very broadly agreed that specific weather events cannot be ascribed to global climate changes, let alone to their hypothesised

[81] Goklany, I. M. (2003), 'Relative contributions of global warming to various climate sensitive risks, and their implications for adaptation and mitigation', *Energy & Environment*, **14**: 797–822; Tol, R. S. J. (2005), 'Adaptation and mitigation: trade-offs in substance and methods', *Environmental Science & Policy*, **8**: 572–578.

[82] Van Lieshout, M., R. S. Kovats, M. T. J. Livermore, and P. Marten (2004), 'Climate change and malaria: analysis of the SRES climate and socio–economic scenarios', *Global Environmental Change*, **14** (1): 87–99.

[83] Tol, R. S. J., and H. Dowlatabadi (2001), 'Vector borne diseases, development & climate change', *Integrated Assessment*, **2**: 173–181.

[84] For an extensive and insightful discussion of the chronic overemphasis of the climate factor in these diseases in IPCC Reports, see the written evidence submitted by Prof. Paul Reiter of the Institut Pasteur to the House of Lords Select Committee on Economic Affairs, available at http://www.publications.parliament.uk/pa/ld200506/ldselect/ldeconaf/12/12we21.htm.

[85] Stern, N. (2006), 'Reply to Byatt et al.', *World Economics*, 7 (2): 153–157.

human-induced component. His response, however, gave the opposite impression by selective citation and claiming, without evidence, that "The world has been experiencing more extreme weather events."[86] The latter statement is vague (no base period was stated for the comparison), and contradicts the statements in the last IPCC report that there was:

> …no compelling evidence that the characteristics of tropical and extra-tropical storms have changed… [and that]…Recent analyses of changes in severe local weather (e.g., tornadoes, thunderstorm days, and hail) in a few selected regions do not provide compelling evidence to suggest long-term changes. In general, trends in severe weather events are notoriously difficult to detect because of their relatively rare occurrence and large spatial variability.[87]

Several studies since the last IPCC report have re-confirmed these statements. For example, to evaluate projections of increased floods and droughts as a result of AGW, Svensson *et al.* (2005) examined river flow data from the Global Runoff Data Centre in Koblenz, Germany with individual record lengths from stations of between 44 to 100 years.[88] The results of this research showed no general pattern of increasing or decreasing numbers or magnitudes of floods. Andreadis and Lettenmaier (2006) examined trends in drought over the continental United States for the period 1925 to 2003 and found that "droughts have, for the most part, become shorter, less frequent, less severe, and cover a smaller portion of the country".[89] The June, 2003, issue of the scientific journal *Natural Hazards* was devoted to assessing whether extreme weather can be attributed to AGW. The editors concluded that most studies find no such connection.

Indeed, elementary considerations of meteorology lead to the conclusion that a warmer world would have less extratropical storminess and variability,[90] while the suggestion of Sir John Houghton that storminess would be abetted by increased evaporation and precipitation (considerations that might be more relevant in the tropics) is inconsistent with the observation

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[86] Ibid., page 154.
[87] Both citations from IPCC WG1 TAR, Technical Summary, page 33.
[88] Svensson, C., Z. W. Kundzewicz, and T. Maurer (2005), 'Trend detection in river flow series: 2. Flood and low-flow index series', *Hydrological Sciences Journal*, **50**: 811–824.
[89] Andreadis, K., and D. Lettenmaier (2006), 'Trends in 20th century drought over the continental United States', *Geophysical Research Letters*, **33**, 2006GL025711.
[90] The relevant process, baroclinic instability, is shown in all textbooks on dynamic meteorology to be proportional to the north–south temperature difference. viz Holton, J. R. (2004), *An Introduction to Dynamic Meteorology*, Volume 88, Fourth Edition (International Geophysics) (Hardcover).

that there has been no discernible increase in precipitation since the beginning of satellite measurements.[91]

We note in passing that, contrary to virtually all projections, the 2006 hurricane season in the North Atlantic was relatively mild, underscoring the poor knowledge the climatological community has about the processes that drive storms and extreme weather events, and the folly of giving too much credence to longer-term forecasts based on current knowledge even when forecasting tools have been "trained" intensely using past information.

To sum up, the Review's analysis of the prospective impacts of possible global warming is consistently biased and selective—and heavily tilted towards unwarranted alarm.

3. THE ISSUE OF PROFESSIONAL STANDARDS

The scandal of non-disclosure and poor archiving

Given the global impact of the 'hockey stick', referred to earlier, and similar papers based upon the statistical manipulation of proxy temperature data, one might have expected that governments would by now be insisting that due diligence be applied to all papers concerned with AGW. With the importance now attached to climate prediction, researchers should be required to follow the most stringent professional standards of archiving and disclosure, but with commendable exceptions they do not. Poor disclosure, verification, and media reporting in climate prediction are widespread and a scandal.

The volume of data involved in climate research makes verification of climate prediction impossible without the cooperation of the original workers. The 1998 Mann *et al.* 'hockey stick' paper was soon questioned, but so poor is the archiving of its data and computer programmes that it took almost eight years and direct action from the US House of Representatives for its statistical flaws and lack of robustness to be exposed. By refusing to release data or computer programmes, researchers can effectively prevent verification (which, in science, is the normal route to acceptance) and thereby argue that their thesis has not been falsified.

[91] Smith, T. M., X. Yin, and A. Gruber (2006), 'Variations in annual global precipitation (1979–2004), based on the Global Precipitation Project 2.5° analysis', *Geophysical Research Letters*, **33**, 2005GL025393.

Some climate scientists who receive generous public funding appear to be determined to maintain self-regulation solely through peer review, and they have been supported in this aim by the British Government and the IPCC.

The contemporary global temperature series as used by the IPCC plays as central a role in climatology as the Consumer Price Index plays in national economic research. The Review shows it as Figure 1.3. Yet it is not produced by a proper statistical agency working under transparent and rigorous protocols. Instead, it is produced by a small, secretive group of researchers at the Climatic Research Unit (CRU) at the University of East Anglia, an organization closely affiliated with the Hadley Centre. The CRU has an explicit policy of refusing to allow external examination of how they produce their global temperature series. In response to a request to examine the underlying data and methods, Dr Phil Jones of the CRU stated: "Why should I make the data available to you, when your aim is to try and find something wrong with it?" Since scepticism and efforts to falsify hypotheses are fundamental elements of scientific method, we find this statement remarkable. The request came from Australian researcher Warwick Hughes, who wished to examine possible Urban Heat Island (UHI) effects and other bias in the CRU instrumental temperature series. Dr Jones repeated his statement to German climatologist Prof. Hans von Storch,[92] who, in a presentation to the US National Academy of Sciences on March 2, 2006, made clear his astonishment and contempt towards this attitude.

This is by no means an isolated instance. It would be unimaginable for national statistical agencies to take a secretive position regarding the national accounts and price index data they prepare, yet the same situation is regarded as perfectly acceptable within climate science. In a *Wall Street Journal* interview,[93] asked why he would not cooperate with researchers attempting to replicate his 'hockey stick' diagram, Mann said that he would not be "intimidated" into releasing his computer programme. When US Congressman Barton later asked for this programme he replied, "It also bears emphasis that my computer program is a private piece of intellectual property."[94] This episode triggered a chorus of indignation

[92] Slide 4, http://meteo.lcd.lu/globalwarming/von_Storch/reconstruction_of_historical_temp_060302.ppt
[93] *Wall Street Journal*, Feb. 14, 2005.
[94] http://www.realclimate.org/Mann_response_to_Barton.pdf

from climate prediction scientists—not at Mann's attempt to block verification of his publicly-funded paper, but at Congressman Barton's request![95] This, however, raises the question as to whether potentially costly public policies should be based, even in part, on private pieces of intellectual property that, moreover, have not been thoroughly evaluated and replicated.

The full disclosure of all data, statistical techniques and computer code should be a requirement for science used in climate policy formulation, and the Review should have rejected any advice, or publications, for which such disclosure has not been made. The Review should also have advised the UK government to require that full disclosure be made for any future climate science advice that it receives, in line with the recommendations of both the NRC and Wegman panels, and so that the scientific process can function unimpeded by secrecy. The presently permitted secrecy is not only inconsistent with the process of science, but also retards scientific understanding and slows the search for rational policies to address climate change.

Inadequacies of peer review

Policymakers place far too much confidence in the peer review system used by journals, because they misunderstand its purpose and the process. "Throughout history, most scientists published their views without formal review and peers published their criticisms openly."[96] The peer review system was developed comparatively recently by editors of publications to maintain the quality of their journals. But while peer review aims to ensure that papers are well-framed and advance hypotheses worthy of consideration by the scientific community, it was never intended to provide a guarantee that hypotheses or recommendations advanced in papers were correct or unchallengeable. In particular, it is no safeguard against dubious assumptions, arguments and conclusions if the peers are largely drawn from the same restricted professional milieu as the authors. Moreover, as the examples above show, peer review does not even ensure that data and methods are open to scrutiny or that results are reproducible.

[95] This included a letter from the European Geosciences Union pleading to retain self-regulation. See http://pubs.acs.org/subscribe/journals/esthag-w/2005/jul/policy/figures/EGSstatement.pdf

[96] Maciej Henneberg, Peer review: the Holy Office of modern science, *Natural Science*, http://naturalscience.com/ns/articles/01-02/ns_mh.html

Bias in science is not usually intentional or even conscious, but it is especially prone to occur when consensus views are sought or expressed. Prof. von Storch, who is review editor of the "Regional Climate Projections" chapter of the IPCC's forthcoming assessment report, recently warned[97] that "exaggerat[ed] claims pass the internal quality checks of science relatively easily, whereas more reasoned and scientifically accurate claims find an unwelcome audience among scientists". He went on to argue that "The practice of scientists exaggerating threatening perspectives of anthropogenic climate change and its implications serves not only the purpose of supporting a policy perceived as 'good' but also personal agendas of career and public visibility."

A recent example of how easily flawed papers supporting the alarmist view can pass peer review is that of Chuine *et al.*,[98] who claimed that they could derive the summer temperature in Burgundy for any year back to 1370 from the dates of grape harvests. The paper concluded that 2003 was the warmest year since 1370, a dramatic conclusion which helped it gain acceptance in *Nature* and wide attention for the authors. A statistician, Douglas J. Keenan,[99] engaged in a long effort to obtain the authors' data, and eventually was able to show that while the Chuine *et al.* model treated moderate summers well, it was without statistical merit for estimating exceptionally warm years. The problem for the use of this type of science in the public arena is that far more lay people will have seen or heard media reports of the original paper than hear of its rebuttal.

Keenan says on his web page (our emphasis), "What is important here is not the truth or falsity of the assertion of Chuine et al. about Burgundy temperatures. Rather, what is important is that *a paper on what is arguably the world's most important scientific topic [global warming] was published in the world's most prestigious scientific journal with essentially no checking of the work prior to publication*."

Few papers in climate science are independently verified, often because of the difficulties in getting the original data as reported above. When the few papers that are critical of the consensus view are published

[97] von Storch, H., 'Tragedy of the Commons and Sustainability of Climate Science', presentation at the Institute for the Study of Society and Environment, Boulder, Colorado, 8 July 2005. http://w3g.gkss.de/staff/storch/ABSTRACTS/050708.boulder.pdf

[98] Chuine I., P. Yiou, N. Viovy, B. Seguin, V. Daux, and E. Le Roy Ladurie (2004), 'Grape ripening as a past climate indicator', *Nature*, **432**: 289–290. DOI: 10.1038/432289a.

[99] Keenan, D. J. (2007), 'Grape harvest dates are poor indicators of summer warmth', *Theor. Appl. Climatol.*, **87**: 255–256.

they are often met with a chorus of criticism for their lack of, or inferior, peer review, which stifles discussion of the disputed issues. The dispute over the Mann *et al.* paper is an object lesson both as to why those papers based upon large data sets and advanced statistical techniques should be verified, and why peer review alone is inadequate. From what has now been disclosed, and thoroughly investigated, we know that the criticisms of the Mann *et al.* paper that were rebuffed by many, including the British government, by repeated reference to peer review, were accurate. Those including the British government who continued to defend the 'hockey stick' work because it had been peer reviewed simply missed the point. Based on this experience, the IPCC peer review process provides no safeguard against dubious assumptions, arguments and conclusions. This is particularly so as, over time, dissenting panellists[100] have withdrawn from the IPCC process, thereby reducing it to a restricted professional milieu within which close colleagues frequently review their own work or that of close colleagues.

4. CONCLUSION

We conclude that the Stern Review is biased and alarmist in its reading of the science. In particular, it displays:

- a failure to acknowledge the scope and scale of the knowledge gaps and uncertainties in climate science
- credulous acceptance of hypothetical, model-based explanations of the causality of climate phenomena
- massive overestimation of climate impacts through an implausible population scenario and one-sided treatment of the impacts literature, including reliance on agenda-driven advocacy documents
- lack of due diligence in evaluating many pivotal research studies despite the scandalous lack of disclosure of data and methods in these studies
- lack of concern for the defects and inadequacies of the peer review process as a guarantor of quality or truth.

[100] In an open letter, Dr Chris Landsea explains his reasons for leaving the IPCC AR4 team: http://www.lavoisier.com.au/papers/articles/landsea.html. In written evidence on his work with the IPCC TAR, Professor Paul Reiter of the Institut Pasteur explains why he left the project: http://www.publications.parliament.uk/pa/ld200506/ldselect/ldeconaf/12/12we21.htm.

These and other related problems arise because the Review has relied for advice almost exclusively on a small number of people and organizations that have a long history of unbalanced alarmism on the global warming issue. Most of the research cited by the Review does not, on inspection, make a convincing case that greenhouse warming constitutes a major threat that justifies an immediate and radical policy response. Contrary research is consistently ignored, as are basic observational facts showing that alarm is unwarranted.

The Review fails to present an accurate picture of scientific understanding of climate change issues, and will reinforce ill-informed alarm about climate change among the general public, the bureaucracy and the body politic. HM Government will need to look elsewhere for a balanced, impartial and authoritative review of the current climate change debate.

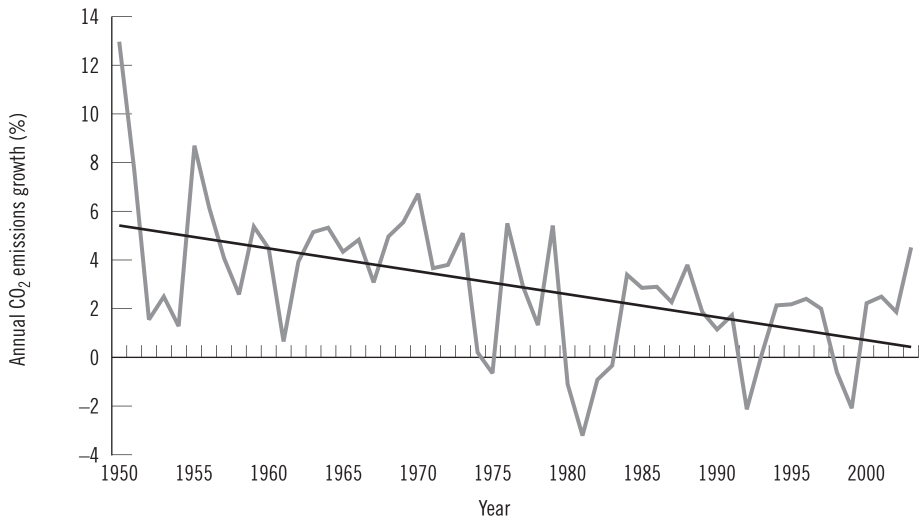
ANNEX
The Stern Review's Mishandling of Basic Observational Data

The Review's presentations of data on the key parameters of the greenhouse equation—emissions, concentrations, and forcing—are inconsistent and unreliable. For example, the Review puts the worst possible face on emission trends:

> Emissions of CO_2, which accounts for the largest share of greenhouse gases, grew at an average annual rate of around 2½% between 1950 and 2000.[101]

The statement is only true if one ignores all natural emissions, which the Review does persistently and carelessly.[102] At the same time, however, the statement obscures the more important point that the rate of emissions growth fell throughout the period, as Figure 1 shows.[103]

Figure 1: Annual growth of anthropogenic CO_2 emissions, 1950–2003



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[101] Review, page 169.

[102] Page 170 of the Review states that "Total greenhouse-gas emissions were 42 $GtCO_2e$ in 2000," but this ignores natural sources, as does the statement on the same page that "57% of emissions are from burning fossil fuels in power, transport, buildings and industry", and the remark on page 171 that "A quarter of all global greenhouse-gas emissions come from the generation of power and heat." Figure 7.1 and Figures A and B in Chapter 7 all omit natural GHG emissions (which comprise 95 per cent of the total for carbon dioxide and are substantial for both methane and nitrous oxide). There is no mention of 'natural emissions' or 'natural sources' of GHGs in Chapter 7.

[103] Marland, G., T. A. Boden, and R. J. Andres (2006), 'Global, regional, and national CO_2 emissions', in: *Trends: A Compendium of Data on Global Change* (Carbon Dioxide Information Analysis Center, Oak Ridge National Laboratory, US Department of Energy, Oak Ridge, Tenn., USA; available at http://cdiac.ornl.gov/trends/emis/tre_glob.htm).

The Review's handling of current CO_2 equivalent (CO_2e) levels is incompetent. Its first mention of the concept is the following:

> The warming effect due to all (Kyoto) greenhouse gases emitted by human activities is now equivalent to around 430 ppm of carbon dioxide.[104]

This is wrong. If the current CO_2e level is 430 ppm, then the warming effect due to all (Kyoto) greenhouse gases emitted by human activities is actually equivalent to only 150 ppm of carbon dioxide, since 280 ppm of carbon dioxide was already in the atmosphere in the pre-industrial era.[105]

Note, however, that even with this correction, the statement still glides too easily over the difference between emissions from human activities and concentrations. CO_2e levels are concentrations, and concentrations do not simply increase by the amount of emissions from human activities. In fact, most GHGs emitted by human activities have been either reabsorbed by the biosphere (this is the case for about 60% of total man-made CO_2 emissions to date) or destroyed by chemical reactions in the atmosphere (as is the case for methane, nitrous oxide, etc.).

The Review also quotes inconsistent figures for CO_2e levels. The OXONIA Lecture gives 425 ppm. The Review generally quotes 430 ppm, but this excludes CFCs solely because they are regulated by the Montreal Protocol rather than the Kyoto Protocol. Including the CFCs, the Review states the figure would be 445 ppm.[106] Yet Box 8.2 on page 202 gives a current level of 450 ppm for Kyoto gases only, implying a total, including CFCs, of ~465 ppm. The true figure may be higher still, as recent papers suggest that the radiative forcing of methane has been underestimated.[107]

The Review says that "The rate of annual increase in greenhouse gas levels is variable year-on-year, but is increasing."[108] This is not true, as examination of the data behind the graph presented to back this statement shows.[109] There has been a clear fall in the rate of increase of total GHGs (including CFCs) since the mid-1980s. The fall would have been clearer still if the graph had been on a logarithmic scale, which it should have been in order to reflect the true increase in forcing.

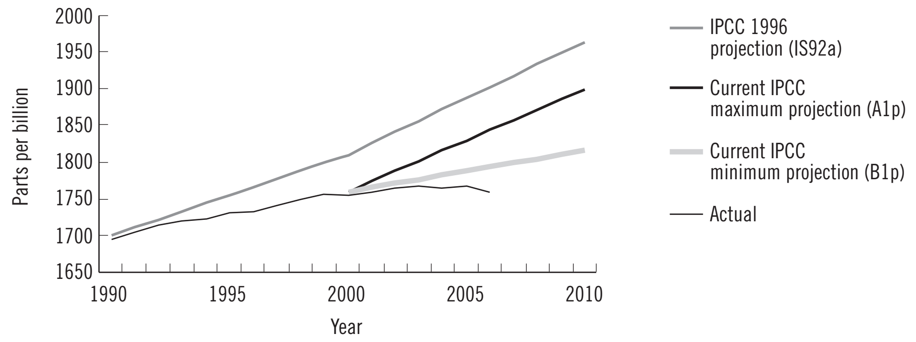
This skews the treatment of *likely future increases in GHGs* towards a worst-case scenario. Page 176 of the Review says, "Emissions are rising. But suppose they

[104] Review, page 3.
[105] The Review plainly misunderstands the meaning of CO_2e levels. What these actually express is current CO_2 levels plus the amount of extra CO_2 that would have the same radiative effect as total observed *increases* in other GHGs. Thus, CO_2e figures do not reflect the total warming effect of GHGs, since they do not include the warming effect of pre-industrial concentrations of non-CO_2 gases. Nor do they reflect the relative warming effect of increases in GHGs since pre-industrial times, since they include the pre-industrial level of CO_2.
[106] Review, page 4.
[107] For example, Shindell, D. T., G. Faluvegi, N. Bell, and G. A. Schmidt (2005), 'An emissions-based view of climate forcing by methane and tropospheric ozone', *Geophys. Res. Lett.*, **32**, L04803, DOI: 10.1029/2004GL021900, observes that "The emissions-based view indicates that methane emissions have contributed a forcing of ~0.8–0.9 W/m^2, nearly double the abundance-based value."
[108] Review, page 176.
[109] Figure 1.1, Review, page 4. For a clearer graph of the growth rate, see NASA's *Growth Rates of Greenhouse Gas Forcing (5–year mean)*, available at http://www.giss.nasa.gov/data/simodel/ghgases/. The accumulation rate has fallen further since 2003; the latest data are available at http://www.cmdl.noaa.gov/ccgg/iadv/index.php.

continue to add to GHG concentrations by only 3ppm a year…" This implies both that 3 ppm is the current rate, and that it is a reasonable minimum rate for the future. Neither proposition is true. Other parts of the Review give the current rate of increase at "about 2.7ppm CO_2e per year",[110] "roughly 2.5 ppm every year",[111] and "around 2.3 ppm per year".[112] In fact, over the last 10 years it only averaged 2.2 ppm, and the trend seems downwards, with 1.7 ppm the likely outcome for 2006.[113] Taking 3 ppm as a minimum future value is thus excessively pessimistic. Yet the Review goes even further when it proposes that "In a plausible 'business as usual' scenario, they [concentrations] will reach 550ppm CO_2e by 2035."[114] As this is based on the Review's assumption that current concentrations are only 430 ppm, it requires an increase of 120 ppm in 30 years, an average of 4 ppm per year. This is unrealistic: it is double the current rate and higher even than the record average level achieved in the peak years of 1976–1988.

The excessive projections derive from ignoring hard data on concentration trends, and instead using carbon cycle models to predict concentrations from projected emissions. A good test of the reliability of this approach is to compare model predictions for methane with actual observations. Since methane has a shorter atmospheric lifetime than CO_2, it shows the reliability of modelling more quickly. As Figure 2 illustrates, modelling concentrations from emissions is still a very inexact science.[115]

Figure 2: Atmospheric abundance of methane



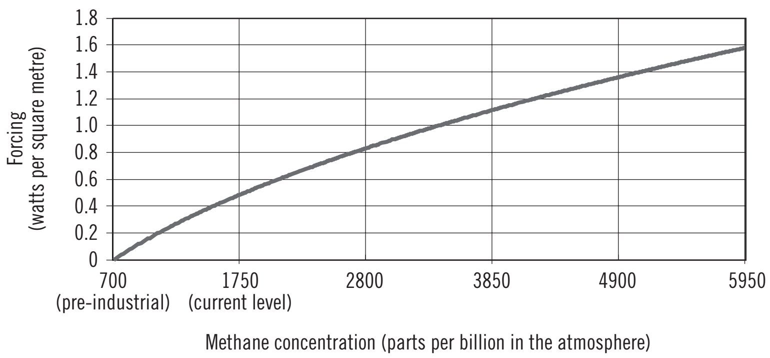
[110] Review, page 169.
[111] Review, page 193.
[112] Review, page 3.
[113] As of early November, 2006, Mauna Loa, Cape Grim and the South Pole are all showing trend increases for 2006 implying an annual rise of ~1.65 ppm. The contribution of other GHGs will be negligible. For the latest data, see http://www.cmdl.noaa.gov/ccgg/iadv/index.php.
[114] Review, page 169.
[115] Note, for example, that in the case of CO_2, the difference in the two estimates quoted by the IPCC for the rate of absorption by tropical forests alone is greater than total estimated global fossil-fuel emissions. See IPCC TAR, Working Group 1, page 99, Table 3.2; and Marland, op. cit.

The real, observed concentration of methane has not increased for the last 7 years, contrary to all IPCC modelling and scenarios.[116] While the first chapter of the Review mentions methane more than 20 times and repeatedly emphasises the possibilities for massive escape of the gas from thawing permafrost or ocean hydrates, it fails to observe this important change in atmospheric forcing, let alone discuss possible explanations.[117]

The Review correctly states that "the warming effect of carbon dioxide rises approximately logarithmically with its concentration in the atmosphere", but then immediately adds, wrongly, that *methane and nitrous oxide concentrations have a linear relationship to radiative forcing*.[118] In fact, forcing declines with concentration increments, as shown in Figure 3 for methane using the IPCC formula.[119]

Leaving aside the Review's mistake in describing CO_2e levels, all its misstatements of data on emissions, concentrations and forcing follow a consistent pattern. In each case, total change to date—which has been substantial, but harmless—is minimised. By contrast, present and likely future rates of change—which are presented as having dire consequences—are exaggerated. The Review's data distortions are systematically biased towards alarm.

Figure 3: The radiative forcing of methane*



*IPCC Third Assessment Report formula, relative to pre-industrial level

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116 Methane trends at measuring sites around the world are shown here: http://cdiac.esd.ornl.gov/trends/atm_meth/csiro/csiro_gaslabch4.html. Provisional data indicate that, as at October 2006, the trend level of methane at the benchmark site at Mauna Loa, Hawaii, had fallen by 10 parts per billion from its peak in late 2003. These data are continuously updated at http://www.cmdl.noaa.gov/ccgg/iadv/index.php. The chief compiler of these data, Dlugokencky, recently observed that "even as the reduction was happening, people doing emission scenarios weren't accounting for it." (http://www.americanscientist.org/template/AssetDetail/assetid/54097).

117 One recent paper suggests that it may be a temporary phenomenon resulting from reduced precipitation in some wetlands—which had not, however, been predicted by models. See Bousquet *et al.* (2006), 'Contribution of anthropogenic and natural sources to atmospheric methane variability', *Nature*, **443**: 439–43.

118 "Note that other greenhouse gases, such as methane and nitrous oxide, have a linear relationship." Review, page 7, footnote 16.

119 The formula is given in Section 6.3.5 of IPCC Working Group 1 TAR, available here: http://www.grida.no/climate/ipcc_tar/wg1/222.htm#635

The Stern Review: A Dual Critique

PART II: ECONOMIC ASPECTS

Ian Byatt, Ian Castles, Indur M. Goklany, David Henderson, Nigel Lawson, Ross McKitrick, Julian Morris, Alan Peacock, Colin Robinson and Robert Skidelsky

Introduction

The starting point of the Stern Review is that 'The scientific evidence is now overwhelming: climate change is a serious global threat...'. For reasons that are set out in Part I above, we believe that this assertion is not correct, and that the Review's treatment of scientific issues is open to serious question. Here we go on to question its treatment of economic issues.

This is no straightforward task, because of the lack of clarity which characterises much of the Review's analysis. This has been noted by others: in the article of theirs that follows, and which likewise comments on the Review, Richard Tol and Gary Yohe make the point that 'It is impossible for a reader to understand precisely what is in the calculations that underlie' the Review; and in the same vein, William Nordhaus has written that 'It is virtually impossible for mortals outside the group that did the modeling to understand the detailed results of the Review'. In an after-the-event attempt to clarify matters, a Postscript to the Review, accompanied by a Technical Annex on modelling issues, was published just before this article went to press. But much remains unclear, placing an undue burden on readers to excavate the actual structure of the Review's argument.

Our treatment below falls under six headings. We start in Section 1 by considering the Review's valuation of the possible *impacts* of global warming. Here our point of departure is Section 2 of Part I above, where our scientific colleagues have assessed what the Stern Review says about prospective biophysical impacts. With their conclusions as a basis, we move on to consider, and to put in question, the figures that the Review derives for the prospective costs of these various impacts, and hence for the benefits that would supposedly flow from policies to reduce emissions.

From the projected benefits of mitigation, we turn in Section 2 to consider the prospective costs involved. We think that the Stern Review has understated these, probably by a wide margin. The combination of projected benefits that are pitched too high and projected costs that are pitched too low has led to a seriously unbalanced presentation of policy alternatives.

In Section 3, we consider the central issue of discounting the future. Here again we give reasons to question the Review's treatment. Critical issues are not fully explored, the bias towards immediate and far-reaching actions to reduce emissions is reinforced, and the risks and problems that would arise from following the Review's prescriptions for policy are not faced.

Under all these headings, a recurrent theme is that the Review positions itself well outside the mainstream of published economic writings on these subjects: in relation to the professional debate, it appears as an outlier.

In Section 4, we consider the choice of policy instruments in the context of climate change, and comment on the treatment of these issues in the Review. Section 5 deals with further major omissions from the Review—issues, and contributions to the subject, which the document fails to consider. Some of the points that we make here form a counterpart and extension of the argument in Section 3 of Part I above: we draw attention, as our scientific colleagues have done, to an established and officially-approved process of inquiry which is not professionally up to the mark. Section 6 summarises our conclusions.

The Review shows serious weaknesses in its treatment and presentation of basic data. The Annex to Part I comments on one aspect of this failing, namely, the mishandling of basic observational data relating to climate change and the factors that bear on it. Here, we present a counterpart annex of a similar kind. It deals with the Review's faulty handling of sources which are themselves flawed. The sources in question are the emissions scenarios which form the starting point for the Third Assessment Report of the Intergovernmental Panel on Climate Change (IPCC).

1. VALUING POSSIBLE IMPACTS

Biased alarmism

The Review presents a dark and dramatic picture of the possible consequences of global warming. The main message is conveyed in the following excerpts, already much quoted by commentators, from the Summary of Conclusions (p. vi):

> Using the results from formal economic models, the Review estimates that if we don't act, the overall costs and risks of climate change will be equivalent to losing at least 5% of global GDP each year, now and forever. If a wider range of risks and impacts is taken into account, the estimates of damage could rise to 20% of GDP or more.
>
> Our actions now and over the coming decades could create risks of major disruption to economic and social activity, on a scale similar to those associated with the great wars and the economic depression of the first half of the 20th century.

Such conjectures—for they are no more than that—are built up in two stages: first, the possible biophysical impacts over time are listed and reviewed; and second, values are attached to these in order to derive measures of their possible effect on human well-being, as in the numbers just quoted.

For both stages, the results presented in the Review refer to possible future developments *over a period of two centuries or more*. This fact alone gives grounds for caution. Both theory and past experience suggest that 'results from formal economic models' are a highly unreliable guide to what may happen so far ahead, while similar doubts can be entertained about the scientific inputs which in this instance form the point of departure for the models.

The Review's treatment of projected biophysical impacts of global warming has been analysed above in Part I. Drawing on a wide range of published sources, the authors review the evidence relating to hunger and agricultural productivity; ecosystems and extinction risks; water availability and shortages; melting ice sheets; general health impacts; malaria and dengue fever; and extreme weather events. They demonstrate that 'the Review's analysis of the prospective impacts of possible global warming is consistently biased and selective—and heavily tilted towards unwarranted alarm'. This conclusion bears on the dramatic claims that the Review makes about the prospective *values* to be attached to these impacts, which consequently appear as greatly overstated.

The arguments set out in Part I are not confined to purely biophysical outcomes: the two aspects, scientific and economic, are partly overlapping. The authors rightly note that the studies which the Review relies on take inadequate account, or no account at all, of the fact that people, enterprises and institutions generally can be expected to adapt their conduct, and the forms which their investment for the future takes, in response to both the experience and the prospect of global warming: now as in the past, they would not just be passive and helpless spectators of climate change. The Review also downplays the possibilities for adaptation arising from future technical progress, the more so since (1) the emergence or prospect of global warming as a problem would increase the incentive for such progress to be directed towards ways of adapting to it, and (2) the time horizon under review is so extended. To disregard or underplay both adaptive behaviour and technical progress is not an acceptable way of defining 'business as usual'.[1]

In weighing the prospects for adaptation, the Review presents a picture of the prospects for developing countries in particular which is in part misleading. It emphasises that adaptation is harder in countries with low levels of GDP per head. But it takes no account of the fact that, in the scenarios that it quotes from the Special Report on Emissions Scenarios (SRES) which point towards high levels of global warming, the projections of GDP per head yield the result that developing countries in general are no longer poor by absolute standards by the time that seriously damaging impacts from warming are seen as emerging.[2] Given such projections of their long-term growth, and the possibilities for resourceful action that this increasing prosperity would help to open up, it is not reasonable to portray the developing countries over the longer term as hapless victims of change.[3]

[1] Elsewhere, the Review is ready to make heroic assumptions about the extent to which technological innovation will reduce or eliminate costs of reducing emissions in the future.
[2] The SRES, published in 2000, produced emissions projections over the period 1990–2100. These formed the point of departure not only for the IPCC's Third Assessment Report, but also for its successor. For the 'OECD 90' group of countries, the SRES gives a figure of $19,100 for GDP per head in 1990. In all but one of the six 'illustrative' scenarios that it focuses on, the GDP per head in developing countries in 2100 substantially or greatly exceeds this figure.
[3] In deploying an argument similar to that of these two last paragraphs, Tol and Yohe write that in the Review, 'vulnerability is assumed to be constant over… two or more centuries'.

In this connection, a point worth noting is that in industrial economies climate has little effect on economic activity. Most of the world's economic activity today takes place indoors: generally speaking, the outputs of both manufacturing and services are unaffected by outdoor conditions. Again, resource extraction also carries on under widely varying climatic conditions, since its location is determined by the resource deposit. In developed countries, only agriculture and forestry can realistically be considered vulnerable to climate change, while for the mid-latitudes, available projections suggest that warming may in fact be beneficial. Only in those lower-latitude countries where the primary sector occupies a large fraction of GDP, and in particular poor tropical countries, does warming as such appear as a possibly significant direct threat to the conduct of economic activity. While the Review rather grudgingly admits that this is the case, it does not make the point that on generally accepted projections of future growth in GDP per head, which it does not put in doubt, the share of these vulnerable sectors can be expected to decline to a relatively low level.

Model-based speculations

The Review spends considerable time discussing Integrated Assessment Model results from the economics literature. Figure 6.2 in the Review shows, for what they are worth, long-term projections of the economic costs associated with global warming scenarios from zero to about 6 degrees C, as computed by some of the most prominent authors in the field. As noted in Part I, the situation as currently understood points to modest warming trends at most. Up to the 2C level, the model simulations as presented suggest zero or negative expected net costs from climate change. Beyond 2C, two of the three models show moderate global costs of less than 2 per cent of GDP; and furthermore, they indicate that the costs level off quickly, even out to a 6C warming scenario. Only the Nordhaus and Boyer analysis appears to suggest increasing marginal costs. But this property of their model arises from the same kind of methodological departure that features in the Stern Review—namely, adding in very speculative non-economic costs with little empirical guidance. The Review acknowledges (p. 152) that, in the Nordhaus–Boyer model, the conventional direct economic costs are only one-tenth of those shown in Figure 6.2, the remainder being speculative 'multiplier effects' operating through investment; and even then, as the Review notes, policy analysis based on the Nordhaus model does not support aggressive emission reductions (see Section 4 below).

Thus, looking at the economics information presented in the Review itself, neither the Integrated Assessment Models nor the IPCC scenarios provide a credible basis for expecting dramatic economic damages from global warming. This can fairly be described as the consensus position in the economics literature. Yet the Review summarily sets it aside. Instead, beginning on p. 149, it appeals to new insights of its own:

> Existing estimates of the monetary cost of climate change, although very useful, leave many questions unanswered and omit potentially very important impacts. Taking omitted impacts into account will increase cost estimates, and probably strongly.

The Review then positions itself as an outlier by referring to two working papers (cited as Watkiss 2005; Warren et al. 2006) as the basis for dramatically ramping up estimates of damages due to extreme weather, 'social and political instability', and 'knock-on effects'. Of these three, the Review's treatment of extreme weather is questioned in Part I; some experts in the field are more severe in their criticism.[4] The latter two influences are not at all clearly defined: the reader can consult the Review (pp. 151–152) to try to make headway. Later they are grouped into 'non-market impact' and 'risk of catastrophe' effects, though with little further definition provided. According to the Review, they account for some 80–90 per cent of the projected damages due to global warming, and yet everybody else seems to have missed them.

These speculations have two effects: they bump up the projected climate warming outcomes (see Box 6.1, p. 154), and they add (massively) to the expected costs in the model runs from the PAGE 2002 model on which the Review places heavy reliance. Table 6.1 (p. 163) shows that from the PAGE model one obtains a span of economic costs from the business-as-usual climate change simulation, 90 per cent of which fall between 0.3 and 7.5 per cent (of total current consumption), depending on whether the regular model or the 'high climate' amplified version is used. This is already high compared to the mainstream distribution, but the Review is

[4] For example, http://sciencepolicy.colorado.edu/prometheus/archives/climate_change/index.html#000973

only getting started—and the later Technical Annex serves to amplify the effects even further. Once the vaguely-defined 'non-market impacts' and 'risk of catastrophe' categories are added in, the economic costs come to span 2.2 to 32.6 per cent of total consumption. These additional elements thus amplify the impacts by factors ranging from 4.3 to nine.

To sum up: from 80 to 90 per cent of the impacts of climate change estimated by the Review comprise novel and conjectural cost categories that are not used by the large majority of experts who have studied this issue up to now; that rely on arbitrary amplifications to regular climate model processes; and which, most crucially, have not received proper critical attention in the peer-reviewed economics literature.

This is not an acceptable procedure. It might have been defensible to include such speculative extensions in a second round of estimates, after having first presented results based on the existing published assessments of economic damages as recognised in the economics literature to date. But to present these novel, outlier concepts as the central results of the Review betrays a lack of balance.

From projected physical impacts to the figures quoted above, of damages which amount to 'at least 5% of global GDP [per head]' and possibly '20% or more', 'now and forever', there is in fact a sequence of argument by which, to take over a phrase from Nordhaus, 'a few more gloomy ingredients are stirred in'. It is via this poorly explained and highly coloured process of accretion that the Review finally derives its startlingly high conjectural figures for the damages that it sees as resulting from the continued pursuit of what it misleadingly portrays as 'business as usual'. Since the treatment of projected damages and disasters is so flawed, these final results cannot be taken at face value: they reflect a bias towards speculative alarmism.

Behind the high damage estimates are emissions estimates that seem themselves to be pessimistic as regards economic pressures for conservation. As relative costs and prices change, new technologies will be adopted because they are profitable: energy saving is an obvious example. As the Review notes, there has been a very big improvement in the fuel efficiency of electricity generation over time; and indeed there is a long history in most developed countries of decline in the energy intensity of GDP. Experience after the 'oil shocks' of the 1970s and early 1980s demonstrated the responsiveness of energy consumption to energy price increases. The elasticity of energy demand with respect to price is low in the short run because the presence of an inherited stock of energy-using equipment limits the extent of switching and conservation (Robinson, 1988). But the Review takes a very long view, and in the medium and long terms, the elasticity is much higher as the stock changes in response to changes in the price of energy relative to other goods and the relative prices of different energy sources. World energy consumption, which had increased at a compound rate of over 5 per cent per annum between 1950 and 1973, continued to rise for a few years after the first oil shock in 1973–74 but then stopped increasing in the first half of the 1980s (BP, 2006). Recent increases in oil and other energy prices are also likely, after a time lag, to bring about a similar response.

In other words, a realistic 'business as usual' (BAU) scenario is itself likely to contain significant energy-saving technological advances that will reduce carbon emissions. This is a further reason why the damage resulting from carbon emissions under BAU may well be significantly less than the Review projects.

2. THE ESTIMATED COSTS OF MITIGATION

Downward bias

Just as the Review exaggerates damages, so it produces surprisingly low estimates of the costs of abatement. Since it is not clear what the extent of carbon reductions would be under BAU, trying to estimate the costs of further reductions beyond this unknown base becomes a highly speculative exercise. There is a long history of 'appraisal optimism' in attempts to estimate the costs of energy sources which would not come to market without some form of government subsidisation or other form of promotion. The massive under-estimation of future costs in Britain's successive government-promoted nuclear power programmes from the 1950s onwards is the example nearest to home (Helm, 2003), but there has been a general tendency to underestimate the costs of energy sources that might replace fossil fuels.

One reason why mitigation costs appear low relative to damage costs is because the Review applies its own relatively low rate of interest in discounting projected future costs and benefits: we consider this aspect in Section 3 below. However, other influences also enter into the result.

In chapter 9 of the Review, an analysis of technologies that would help reduce carbon emissions, and their possible costs, results in mitigation cost estimates of –1 per cent to +3.5 per cent of GDP by 2050, with an average of around 1 per cent. The list of carbon-reducing technologies is one about which there is some consensus among energy specialists (though that is not to say that it will turn out correct, since technological forecasting has a very poor record). But there is considerable doubt about the cost of forcing the adoption of such technologies over and above what would occur without such forcing.

Chapter 9 gives some indication of the uncertainty surrounding its mitigation cost estimates. These depend to a large extent on the work of Dennis Anderson, who has drawn on a number of studies, often by official bodies. Anderson puts the average cost of carbon abatement in 2005 at £225/tonC; but this figure is projected to fall, as a result of incentives, innovation and technical progress to £145 by 2015, to £85 by 2020 and £60 by 2050.[5] The Review (p. 231, Figure 9.5) translates the £225/tonC into \$100/CO_2, which exceeds Stern's own estimate, of (\$85/tonCO_2e), which itself is high in comparison with other studies.

The Review estimates (p. 233) a technology uncertainty of 4.3% of world GDP, far bigger than the energy price uncertainty of 2.2% (both by 2050). When writing about carbon capture, Anderson says: 'even in the near to medium term, the uncertainties are very large.' Two examples that he gives are:

> [carbon capture and storage] (CCS) is expected to play a crucial role…the range of cost estimates will be narrow when CCS technologies have been demonstrated but, until this occurs, the estimates remain speculative.
>
> The costs of carbon abatement are expected to decline by half over the next 20 years, and then by a third further by 2050. But the longer term estimates of shifting to a low-carbon energy system span a very broad range as indicated…and may even be broader than estimated here.

Anderson also makes the important point that in optimisation models, the results change kaleidoscopally with small changes in relative cost assumptions.

[5] Stern Review support papers: *Costs and Financing of Abating Carbon Emissions in the Energy Sector*, 20 October 2006, p. 28.

This emphasis on uncertainty is appropriate. However, here as in other parts of the Review, the qualifications made in the body of the document receive little attention when conclusions are drawn. By the end of chapter 9, it is concluded that mitigation costs are likely to be 1 ± 2.5 per cent of annual GDP—which seems a very small range compared with the highly speculative nature of the estimates; and the Executive Summary (p. xiii) removes all reference to a range of uncertainty, giving the 'upper bound' for the annual cost of emission reductions as 1 per cent of GDP.

Chapter 10 of the Review goes on to discuss mitigation cost estimates derived from macro-economic modelling exercises, with supporting discussion in Chapter 12. The Chapter 10 estimates are generally consistent with those in Chapter 9, concluding that estimates of mitigation costs in 2050 centre on 1 per cent of GDP, with a range of –2 to +5 per cent of GDP. While reference is made to the work of many mainstream analysts, heavy reliance is placed on a single meta analysis (cited in the Review as Barker et al. 2006).

The Review's Table 10.1 summarises the span of surveyed cost estimates for mitigation policy packages adequate to cap atmospheric CO_2 at 450 ppm. The basic cost is 3.4 per cent of global output. This is then whittled away by invoking a number of assumptions, until the 3.4 per cent cost of mitigation becomes a 3.9 per cent economic gain—a very large free lunch.

The revenue-cycling aspect

The largest single cost reduction (1.9 per cent of global output) is arrived at by assuming 'active revenue recycling'. Revenue recycling refers to the fact that some emission pricing policies (taxes, auctioned permits) generate revenue for the government, and this added revenue could be used to finance a cut in other tax rates. In order to model the effects of revenue recycling, however, the cost estimation must be done in a model that includes a full treatment of the tax system. Table 10.1 applies a large cost reduction to all the models surveyed, but notes in a footnote (fn. 4, p. 243) that revenue recycling was a feature only of one model examined.

There is a problem with arbitrarily deducting the benefits of revenue recycling from mitigation cost estimates computed in models without a full treatment of the tax system. The problem is that adding in a proper treatment of the system increases the estimated mitigation costs through 'tax interaction' effects. In studies that have examined this issue, tax interaction costs are typically as large as or larger than revenue recycling effects, so that it is invalid to assume that revenue recycling can be counted against the cost estimates shown in Table 10.1.

Numerous well known studies, not mentioned in the Review, have concluded that in order to measure the recycling benefit in a theoretically sound way, tax interaction costs must also be modelled (e.g. Bovenberg and de Mooij 1994; Fullerton 1997). Tax interaction effects arise from consideration of the conventional deadweight costs of taxation. A tax drives a wedge between the buyer price and the seller price, destroying more consumer and producer surplus than the tax revenue created. This 'excess burden' is a function of the tax rates and the parameters of demand and supply in the market affected. The cross-price effects of introducing a new tax in one market will affect the excess burden in other markets; and in specific circumstances they will increase the excess burden in related markets. Empirical examination by economists (e.g. Parry 1995; Bovenberg and Goulder 1996) has indicated that emissions taxes will typically interact with factor markets (labour and capital) in such a way as to increase the pre-existing excess burdens, generating positive costs due to tax interaction effects. These effects grow in step with—and indeed slightly faster than—the potential benefits from revenue recycling. This result confirms an early theoretical argument by Agnar Sandmo (1975). Rather than this item bringing a net reduction to modelled costs, therefore, it should be viewed as tending to increase them.

There is also a time dimension here. Insofar as carbon taxes are progressively effective in reducing emissions, their revenue yield will fall accordingly, and this will limit the possibilities for revenue recycling. The Review relies on a model without a tax system and hence does not take into account the changing public finance aspects over time.

The domain of conjecture

Besides the questionable gains from 'recycling', Table 10.1 in the Review also allows for arbitrary, free lunch-style 'induced technology' benefits, and for gains due to ancillary reductions in conventional pollution. These influences, which are far from well defined, bring down the projected costs by a further 0.5 per cent of global GDP. They are elaborated in Chapter 12, where, however, the cited literature is notably heavy on unpublished NGO discussion papers and industry promotional brochures. Another significant effect (0.4 per cent) comes under the heading of 'climate benefit', which however remains undefined.

This whole analysis largely relates to a conjectural future: little attention is given to actual past experience. Measures and programmes to reduce CO_2 emissions have been in place for some years, in Britain and elsewhere. The costs and effects of these could have been reviewed, with an eye to the evidence they provide and the lessons to be drawn from them. Such a survey, impartially conducted, would have been a useful contribution to knowledge. Four of us (Byatt, Henderson, Peacock and Robinson) made this point in submitting evidence at the outset of the Review: we suggested that the costs of British mitigation policies, current and prospective, should be identified and documented. This suggestion was not acted on: here as elsewhere, the Review appears as more focused on hypothetical futures than on the evidence and experience of the past.

Much depends on the kinds of measures that are adopted by way of mitigation. Insofar as reliance is placed on regulatory instruments, costs are likely to be appreciably higher. (Here again there may already be useful lessons to be drawn from actual experience to date). Concerned about 'market imperfections', the Review questions the capacity of market-led technological change to adapt to the climate change 'threat'. On the other hand, it seems remarkably optimistic, in the face of past evidence, about the ability of governments to pick technological 'winners' and bring them successfully into the market.

Weighing costs and benefits

The treatment of costs and benefits in the Stern Review is deeply flawed. First, the Review either overlooks or sets aside important elements of the professional literature in favour of its own views, which read as outliers by comparison. Second, whereas the Review is biased towards technological pessimism when assessing the costs of climate change, it is equally (and inconsistently) biased towards technological optimism concerning large-scale mitigation efforts, alternate energy, and so forth. Its treatment of the issues is neither balanced nor credible.

3. DISCOUNTING AND INTERGENERATIONAL EQUITY

Discounting the future

The comparison of early costs with longer-term benefits is crucial to the conclusion that there is a strong economic case for immediate action on the scale recommended. The Review's conclusions largely derive from the use of social time preference theory, which suggests a discount rate based on (1) pure intergenerational time preference, (2) an assumption as to the future growth of consumption, and (3) a figure for the elasticity of marginal utility with respect to consumption. The numbers chosen by the Review are all open to question and, as the later Technical Annex shows, the results are not robust. What is more, the Review takes no account of the opportunity cost of crowding out other forms of future-directed expenditure.

Welfare economists have proposed that the issue of allocating consumption across generations can be analysed using a discount rate that separates into three components in such a way as to allow the welfare of those now living to be compared with that of future generations, taking into account the fact that because of consumption growth the latter can be expected to be more prosperous. The Review goes over the standard discount rate decomposition, which yields:

$$\rho = \eta \frac{\dot{C}}{C} + \delta \qquad (1)$$

where C is consumption per head, $\dot{C}/C$ is its projected rate of change, δ (delta) is the pure rate of time preference, η (eta) is the rate of change of marginal utility as consumption increases ($C \times U''/U'$, where U is the utility function) and ρ is the resulting discount rate to be applied to public sector projects. To derive the appropriate social time preference rate, values thus have to be assigned to all three of the parameters involved.

Choosing parameters

The choice of values depends on assessments and evaluations which are inherently open to debate. Differing views can be held about the future growth of consumption per head, and different positions can be taken as to the ethical considerations that bear on the values assigned to the other two parameters. Since the issues here are both inescapable and unsettled, no short cuts are permissible. A serious treatment should be both balanced and transparent; and it should explore, through careful sensitivity analysis, the implications of taking different combinations of values. It is against this background that the treatment in the Review has to be weighed.

For the parameter delta, the Review explicitly adopts a value of 0.1 per cent per annum, which is of course a very low figure. To say this is not to reject it. The choice of a low pure time preference rate, as with other parameter values, could be defended if presented as illustrative and plausible, rather than definitive, and if the reader was shown, through the medium of a sensitivity analysis, the implications of other possible choices.

As to the other two parameters, the Review does not specify the values that it has taken, so that its recommended social time reference rate likewise remains undisclosed. This is not a transparent procedure. Further, the Review *provides no sensitivity analysis*. These twin omissions add up to a serious lapse.[6]

Since the appearance of the Review, some progress has been made in making good these deficiencies. First, it has been revealed that the Review sets the value for eta at unity, and that it takes the growth rate of world consumption per head over the next three centuries to be, respectively, 2.0, 1.8, and 1.3 per cent per annum. (The latter rate is assumed to hold perpetually thereafter).[7] Allowing for pure time preference, this implies discount rates, century by century, of 2.1, 1.9, and 1.4 per cent per annum.

The Review argues that the presence of uncertainty should reduce the discount rate used. However, many would argue that, because our knowledge of future events becomes more uncertain as the time horizon is extended, discount rates should if anything increase rather than diminish with time.

The Review's failure to provide sensitivity analysis has been partially remedied in the later Technical Annex. Different values have been run there, through the PAGE 2002 model, for the pure time discount rate (delta) and for the elasticity of the marginal utility of consumption (eta).

[6] Where other models are discussed—e.g., in Table 13.3—the rates are given and the effect of varying the discount rate is explored.

[7] These values were obtained by Christopher Monckton, in a personal communication from HM Treasury.

However, these variations have been treated separately and not in conjunction, while no complementary sensitivity analysis has been performed with respect to the growth rate of consumption per head. Further, the Annex obscures the discount rate sensitivity analysis by simultaneously increasing the damage function parameter: it offers a wholly implausible set of simulations in which the already-exaggerated damage costs are further amplified. Its procedures are neither thorough nor transparent, and appear designed to persuade the reader that sensitivity analysis leaves intact the Review's alarmist projections.

Despite its limitations, this belated sensitivity analysis yields some illuminating results. First, the pure time preference rate. In Table PA-3 of the Annex, the average monetary cost of what is taken as a 'business as usual' scenario falls by nearly three-quarters, from 5.0 per cent of global GDP to 1.4 per cent, when the Review's preferred rate of 0.1 per cent per annum is replaced by 1.5 per cent, thus raising the recommended discount rate from 2.1 per cent per annum to 3.5 per cent which cannot be viewed as an especially high figure.[8]

Second, in the case of eta, the Annex analyses the result of taking a value of 1.5 rather than 1.0: such a figure would not be inconsistent with the distributional concerns in the Review.[9] Here the effect is to reduce prospective damage (as defined above) from 5.0 per cent of global GDP to 2.9 per cent. In combination with the 0.1 per cent pure time preference rate, this value of 1.5 yields a discount rate of 3.1 per cent per annum.

Unfortunately, the two sensitivities are not combined in the Annex; and we still await a proper sensitivity analysis on all three parameters, possibly in the form of the Monte Carlo analysis used elsewhere in the Review. Nevertheless, the scale of the potential effect on damage projections, already revealed by this incomplete sensitivity analysis, shows that when different values are assigned very different results emerge, pointing to very different policy conclusions.

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[8] It is in fact the rate recommended for public sector projects in the British Treasury's Green Book—which (it is worth noting) stresses the need to conduct full sensitivity analysis.

[9] A value of 1.5 implies that we value the utility per head of future generations, who are expected to be many times wealthier than we are, at half the rate of our own. It is the figure suggested as appropriate in the Treasury's Green Book.

Weighing the present against the future

This is not the place to consider the much-debated issue of just how the welfare of those living today is to be weighed and assessed in relation to that of future generations.[10] But it should be noted that the particular combination of values that the Review favours, of 0.1 per cent for delta and unity for eta, and the low rate of discount which goes with them, point to very high rates of saving for the current generation.

This fact is brought out in a paper by Partha Dasgupta commenting on the Stern Review. He notes that 'in a deterministic economy where the social rate of return on investment is, say, 4% a year', building in the above values for delta and eta leads to the conclusion that '*the current generation in that model economy ought to save a full 97.5% of its GDP for the future!*' (italics in the original). The Review briefly alludes (p. 47) to the argument that low values of eta yield implausibly high implied savings rates, but waves away Arrow's well known exposure of the problem by saying that it is not convincing. This is not a serious treatment of the issue.

To prescribe such high rates of current saving appears to give too little weight to the interests of the world's poor today and in the near to medium future. The Review makes much of the need to transfer resources now from developed to developing countries. But this concern with poverty today is not easy to square with the use of such a low discount rate, which *inter alia* implies that the present generation of poor people ought to transfer, via a much higher savings rate than now, a substantially greater part of its income to future generations who will be, on the Review's own assumptions, much wealthier. A way of meeting this objection is to prescribe that the extra burden of reduced consumption and higher savings today should be borne by the rich countries alone; and this seems to be the position that the Review takes. It does not, however, consider how far the imposition of such a considerable extra burden on these countries would be consistent with its surprisingly low estimate of the costs of mitigation.

It is a peculiar feature of the Review that while forecasting that people in the future will be vastly richer than today, it also proposes that the present generation should make substantial new sacrifices on behalf of these more prosperous generations. It is as though, looking back two hundred

[10] Some of us would question whether this should be for scholars to decide, with little reference to what people in general want, believe, and are ready to accept.

years (a period comparable to the one the Review purports to cover), we claimed that people living in the early days of industrialisation ought to have made sacrifices on behalf of those living today, even though we are rich beyond the dreams of anyone in those distant times.

The problem of dual standards

The recommendation of the Review is that all future-directed expenditures which are oriented towards reducing future emissions, often if not always with effects that are seen as long term or remote in time, should be evaluated at the real (social time preference) rates of discount that were quoted above. The highest of these, for the whole of this century, is 2.1 per cent per annum. The Review does not dwell on the fact that, everywhere in the world, such relatively low real rates of return are not now characteristic of other investments. While it is true that the cut-off internal rates of return for investment projects across the world are not known with any precision, and may well differ considerably, there is no doubt that they are typically much higher in the private sector; and even for public sector projects, many public enterprises and governments would probably look for higher real returns on expenditure than 2.1 per cent. The British Treasury, as noted above, recommends using a rate of 3.5 per cent with a full sensitivity analysis.

When the marginal rate of return on investments exceeds some officially specified social time preference rate of discount, as in this case, there is a strong argument for using in public expenditure projects the higher of the two rates, since the use of dual criteria opens up the possibility that investments with relatively low returns will crowd out others that would be more beneficial. The risk is all the greater if, as is the case with the Stern Review's recommended course of action, the specially favoured measures, projects and programmes are worldwide and large scale.

This problem of dual criteria has been recognised by William Cline, in a study which is in many ways a precursor of the Review. Like the Review, he advocates a low social time preference rate of discount for evaluating climate-change-related expenditures; but unlike the Review, he faces up to the issue of crowding out. His solution is to apply a 'shadow price of capital', so that insofar as mitigation expenditures are thought to displace higher-yielding investments, their initial costs are adjusted upwards:

he suggests a mark-up of 60 per cent.[11] Any such procedure, if accepted as valid, would of course serve to push up significantly the true estimated costs of mitigation. Although such a result is arguably implied by its own advocacy of dual expenditure criteria, it is not mentioned in the Review.

4. THE CHOICE OF POLICY INSTRUMENTS

The Review raises a great many issues of policy, one of which we have just referred to. Here we focus mainly on the choice of policy instruments, an aspect which the Review considers at length. We end the section with a brief comment on what one might term the policy orientation of the Review.

Prices versus quantities

Moving the discussion to means, rather than ends, brings up another example in which the Review positions itself as an implausible outlier against the specialist literature. Section 14.4 ('Efficiency under uncertainty') presents a standard treatment of the question of instrument choice in the presence of uncertainty over damages and abatement costs. The Report correctly points out that, for the case of carbon dioxide, the marginal damages curve is relatively flat and the marginal abatement cost curve is relatively steep, and the Weitzman-type analysis indicates that emissions pricing yields a smaller expected welfare loss than tradable quotas.

Combined with the literature on the low monetary value of damages, the available expert literature therefore implies that the optimal carbon policy would be, at most, a small charge on each unit of CO_2 emissions. This in turn would imply a small initial but progressively increasing reduction in emissions below the business-as-usual case. When the second-order costs and benefits ('active revenue recycling') associated with factor market distortions induced by the new carbon tax are also taken into

[11] Cline's original study, entitled *The Economics of Global Warming*, was published in 1992. It is not referred to in the Stern Review—a strange omission indeed—but gets a belated mention in the later Postscript. Cline returned to the subject in Chapter 1 of *Global Crises, Global Solutions*, edited by Bjørn Lomborg and published in 2004, where his arguments are followed by interesting expert comments. This book is likewise not referred to in the Review (or the Postscript).

account, even small departures from business-as-usual carbon emissions appear as welfare-reducing (Parry, Williams and Goulder 1999; Bovenberg and Goulder 1996).

These arguments would lead to the conclusion that picking a carbon price is economically more sensible than picking a quantity, and that such a price would initially be likely to be relatively low. Such a conclusion, however—and bearing in mind the difficulty in achieving international agreement on carbon taxation—is not compatible with the 'need to take strong action now' asserted in the first sentence of the Review, and the implication that regulators should set a hard cap on emissions well below current levels.

Perhaps aware that the logic leads away from emission caps, the Review mounts a novel argument, based on a single, recently published conjecture that, in the future, what is currently believed about the relative slopes of the marginal damage and marginal abatement cost curves will be reversed. Figure B in Box 14.1 asserts that while marginal costs of emission reductions will become very low, marginal damages due to carbon dioxide emissions will suddenly become very steep. The Review defends the idea that marginal costs will radically decline by invoking a vague notion that technology will change. The argument that the marginal damages curve will become steep is not defended: instead, on p. 314 the reader is referred to Chapter 13 for the discussion. In that chapter (p. 293) there is a list of conjectured horrors—hundreds of millions dead, social upheaval, etc.—leading on to the assertion that

> The expected impacts of climate change on well-being in the broadest sense are likely to accelerate as the stock of greenhouse gases increases, as argued in Chapter 3. The expected benefits of extra mitigation will therefore increase with the stabilisation level.[7]

Yet the footnote here leads to text which contradicts the point being made:

> One characteristic of the climate physics works in the opposite direction: the expected rise in temperature is a function of the *proportional* increase in the stock of greenhouse gases, not its *absolute* increase.

In other words, additional units of CO_2 in the atmosphere have an effect that goes with the logarithm of the level of CO_2, so that constant increments of

CO_2 have diminishing marginal effect. This in turn implies that annual emissions have diminishing marginal impact, even in the long run.

We conclude, therefore, that the premise of the policy conclusions in Chapter 14 is false even on the Review's own reading of the evidence. The Review conjectures that the relative slopes of the marginal damages and marginal abatement cost curves will reverse, even while acknowledging that this is at odds with the available evidence. We would add that if the Review is correct, that foreseeable technologies will radically reduce the cost of carbon emission abatement in the near future, this is an argument for *delaying* abatement, not hurrying into it.

The Review appears to favour carbon trading, in part because it could involve transfers to developing counties. But very little account has been taken of the practical problems of implementing satisfactory systems, in particular setting up auctions or dealing properly with the initial allocation of emissions caps. These problems would be particularly acute at international level.

In principle, there is a place for 'market instruments' such as carbon taxes or carbon trading. Carbon taxes, for example, are transparent. It is relatively easy to ensure that they are levied widely—on individuals as well as companies. They have the merit that levels can be changed in response to improved knowledge. Their initial level would inevitably be arbitrary, but they could be introduced at a relatively low rate and raised as knowledge of carbon damage and the effectiveness of taxes accrues. Provided that proper explanations are given for changes, appropriate expectations can be created. And as noted in Section 2 above, carbon taxes would provide revenue for the public finances and make it possible to reduce other taxes or, say, to provide resources for other 'green' policies.

Carbon trading likewise requires initial arbitrary decisions—in its case, on the 'desirable' levels of emissions to be achieved and their allocation to emitters. It is one thing to apply limits to a relatively small level of emitters, say large carbon using companies, and is another to apply them to all emitters, including the personal sector. Yet if limits are applied arbitrarily or unevenly, much of the benefit of using an economic instrument is lost. Both rules and administrative mechanisms need to be devised for the working of any market for trading permits; and if there is to be international trading, all the governments concerned need to act objectively and fairly, and to be seen to be acting objectively and fairly.

There could well be political resistance to carbon taxes—such as the blockages and motorway 'go-slows' in France and the UK in 2000; but acceptance or not of such taxes is a proper test of the willingness of people to support the policies that would lead to lower emissions.

Trading today is very far from being universal: it is being applied only to a limited number of emitters. For example, the EU Emissions Trading Scheme (ETS), in its phase one, covers less than 40% of relevant emissions. In the present state of knowledge, there is no way of setting the right levels, at either the national or the European level—and if they are subsequently changed, this creates uncertainty about arrangements that work only because of their longer-term incentives.

Furthermore, until governments start to auction or otherwise charge for the initial level, allocations will typically involve presenting substantial benefits to existing emitters or their suppliers. The Review advocates the use of auctions to allocate the 'desirable' amount of emissions, but the design of an auction for a large number of emitters would be complex and contentious. Until auctions are in place, carbon trading scores badly on transparency. The overall economic costs may be high, albeit disguised. Or the allocations may be so generous that costs are low, but so are the overall reductions in emissions after taking account of the gains that individuals may make by trading what is allocated to them.

There is some empirical evidence on the performance of trading schemes. In relation to the UK Emissions Trading Scheme, the world's first large-scale greenhouse gas trading scheme, that began in 2002, Smith & Swierzbinski (2006) argue that the initial setting of targets for emissions can be the Achilles' heel of emissions trading. The authorities are at an informational disadvantage and the price of making trading arrangements acceptable is to start in generous mode, giving substantial benefits to existing high emitters. They further conclude that adjustment of initial error is both difficult and potentially costly. Efficient functioning of the market requires stability and confidence about current and future property rights, and the repurchasing of rights once allocated can be costly.

The optimal policy target

In Section 13.7 of the Review, the issue is raised of identifying an optimal concentration of 'greenhouse gases'. The Review cites a group of studies (by Nordhaus and Boyer; Tol and Manne et al.) and concedes that they all lead to the same conclusion (p. 298):

> These studies recommend that greenhouse gas emissions be reduced below business-as-usual forecasts, but the reductions suggested have been modest.

But once again, the expert literature is then set aside on the basis of the Review's own contrary opinion (p. 298):

> However, the optimal amount of mitigation may in fact be greater than these studies have suggested.

In this context, as elsewhere, conjectural grounds are given why the experts who have studied the issue hitherto have all missed the salient features to which only the Review is privy, and which yield an entirely different conclusion, namely that deep emissions cuts are optimal. But the peer-reviewed literature, even that portion surveyed in the Review, suggests that an emissions charge equal to marginal damages, at most, say, US$10 per ton of carbon, is the most aggressive aggregate emissions control policy that could be justified. Because of the steepness of the marginal abatement cost curve, this implies that most countries implementing such a policy would initially reduce emissions only slightly—although the cumulative effect over the longer term would be much greater. Of course, if the path of abatement costs is not as steep as is currently thought, a small CO_2 tax might actually induce large emission reductions. However, to propose deep emissions cuts on that conjecture alone would be to make the mistake associated with the prices-versus-quantities analysis described above. In the case of carbon emissions, the social costs associated with policy uncertainty are minimized by choosing an emissions price and letting the market determine the quantity.

The role of government

While the Review makes many allusions to imperfections and failures in markets, it makes no mention even of the possibility of government failure: in this connection, no reference is made to the arguments and findings of public choice theory. The consequence of ignoring the limits and failings of political action is serious, because the Review points to the need for such action to be undertaken on a grand scale, both nationally and internationally.

While prescribing a greatly expanded role for governments, the Review has failed to think through the considerable problems of defining that role and carrying it into effect. A leading instance is to be found in its recommendation, noted above, that a special and much lower rate of discount should be used for mitigation projects alone. The clear implication of using such low discount rates, relative to those used in the private sector, is that governments would find themselves faced with an array of potential investments that arguably 'should' be undertaken but which the private sector would not find worthwhile. In such situations, ensuring that the investments were made would require heavy state involvement. Governments would be compelled either to assign the responsibility of carrying out climate change investment projects to public authorities or to assume the task of designing and putting in place the necessary incentives for private businesses to undertake them. In either case, enforcing a discount rate much lower than market rates would lead to what could be a very large expansion of the public sector. This troubling consequence of a dual discount rate is an important issue, and one on which the Review is inappropriately silent.

5. MISSING ELEMENTS

Despite its considerable bulk, the Stern Review is far from being a complete and well-rounded survey of its subject. The main reason for this is the pervasive bias which we and our scientific colleagues have both noted, and which has led to the disregard or undervaluing of sources which suggest a different view of those aspects of its subject- matter that the Review considers. But a further limitation of the Review is that there are aspects which it fails to cover, or even to recognise as pertinent. One such aspect, just noted, is its failure to face up to the problems that may arise from 'government failure'. But this is by no means the only instance where relevant topics and concerns are passed over.

A serious omission concerns an issue which goes beyond economics, and has been raised and discussed in Part I above. Our scientific colleagues have noted there the failures of due disclosure, still unacknowledged and unremedied, that have characterised published and peer-reviewed work which the IPCC and its member governments have drawn on. Neither the failures themselves nor the publications which have exposed them are mentioned in the Review: it simply turns a blind eye to evidence that might put in question any elements of 'the science'.[12] The procedural flaws which it thus disregards put in question the IPCC process as a whole, and further undermine any claim that 'the scientific evidence is now overwhelming'.

Another respect in which the IPCC process is open to question is the treatment within it of economic issues. In this connection, two of us (Castles and Henderson) have pointed to flaws both in the SRES and more broadly. These arguments receive only passing and misleading mention in the Review. Contrary to what is said or implied in the text (pp. 182 and 188), this critique of the SRES is by no means confined to the emissions projections made in the report, while what it says about the IPCC—as also the United Nations Environment Programme, which is one of the IPCC's two parent agencies—extends well beyond the scenarios. Further—and here again there is a link with Part I—these authors have made the point, in the context of the IPCC process, that peer review offers no safeguard against dubious assumptions, arguments and conclusions if the peers are largely drawn from the same restricted professional milieu. This aspect also is not touched on in the Review. An article in which the whole of this particular debate was reviewed and taken further (Henderson 2005) is not mentioned in the Review or included in its list of references.[13]

Both these topics—the question of disclosure, and the treatment of economic issues within the IPCC process—were considered in the wide-ranging report, likewise entitled 'The economics of climate change', which was prepared by the House of Lords Select Committee on Economic

[12] In particular, no reference is made to the work of McIntyre and McKitrick (2003, 2005, and 2006), nor to the important Wegman report of July 2006 to the Energy and Commerce Committee of the US House of Representatives. The latter document is referred to in footnote 23 of Part I above, and briefly summarised in an annex to Henderson (2006).

[13] One of the issues raised by Castles and Henderson was the faulty procedure, used in the SRES and elsewhere in IPCC-related documents, by which cross-country comparisons of real GDP were made using market exchange rates rather than on the basis of purchasing power parity (PPP) comparisons. Box 7.2 of the Review, where this issue is taken up, makes two basic errors. First, it says that PPP converters '[compare] the ability to purchase a standard basket of goods and services', when in fact the comparisons extend in principle to all goods and services that enter into GDP. Second, it refers to 'PPP exchange rates', when in fact PPP converters are price index numbers: except in the minds of some modellers, there is no such thing as a 'PPP exchange rate'.

Affairs and published in July 2005.[14] The report was accompanied by a separate and substantial volume containing the written and oral evidence submitted to the Committee. Despite its having treated the identical subject at length, and in a way that evoked widespread attention, the Select Committee report does not find a place among the 1,100 or so references that are listed in the Review. This is an extraordinary omission.

A notable feature of the Select Committee report was the concerns that it expressed about the IPCC. Given the general credibility which the Panel has acquired, it is remarkable that a group of eminent, experienced and responsible persons, drawn from a national legislative body and spanning the political spectrum, with the help of an internationally recognised expert adviser, and after taking and weighing evidence, should have published a considered and unanimous report in which such concerns are prominently voiced.

The Stern Review makes no reference to the issues thus raised. It takes the established official process of inquiry and assessment, including the contribution of the IPCC, as given and fully trustworthy. The possibility that the process could be improved is not entertained. This missing dimension severely limits the usefulness of the Review as a guide to policy. Its uncritical acceptance of officially sponsored sources helps to explain its strong and pervasive bias, since much the same areas and instances of bias, though often in less extreme and unqualified form, are to be seen on the part of its mentors.

We believe—and our scientific colleagues concur—that the House of Lords Select Committee was right to raise these questions, and the Stern Review is wrong to ignore them. There is a serious problem here. Although it provides for substantial, well organised and worldwide expert participation, the IPCC process is far from being a model of rigour, inclusiveness and impartiality: it is in fact deeply flawed. Its member governments either fail to notice the flaws or view them with a tolerant eye. There is an urgent need today to build up a sounder basis than now exists for reviewing and assessing issues relating to climate change.[15]

[14] The Select Committee included four former cabinet ministers, two of whom had been Chancellors of the Exchequer; two other members with ministerial experience; a former Governor of the Bank of England; and two noted professors. Its Special Adviser was an outstanding British environmental economist.
[15] This subject is further explored in Henderson (2006).

6. CONCLUSIONS

Our main conclusions coincide with, and serve to confirm and reinforce, those reached by our scientific colleagues in Part I above. Like them, we would emphasise in particular two interrelated features of the Stern Review:

- it greatly understates the extent of uncertainty as to possible developments, in highly complex systems that are not well understood, over a period of two centuries or more
- its treatment of sources and evidence is persistently selective and biased.

These twin features have combined to make the Review a vehicle for speculative alarmism.

We also endorse, from our own analysis, the judgement of our colleagues that the Review:

- mishandles data
- gives too little attention to actual observation and evidence, as distinct from the results of model-based exercises
- takes no account of the failures of due disclosure, and the chronic limitations of peer reviewing, that have been characteristic of work relating to climate change which governments have commissioned and drawn on.

As to specifically economic aspects, we have noted among other weaknesses that the Review:

- systematically overstates projected costs of climate change, partly though by no means wholly as a result of its failure to acknowledge the scope for long-term adaptation to possible global warming
- underestimates the likely cost—including to the world's poor—of the drastic global mitigation programme that it calls for
- proposes worldwide adoption of a specially low rate of interest for discounting the costs and benefits of mitigation, on the basis of inadequate analysis and without regard for the problems and risks that would result.

So far from being an authoritative guide to the economics of climate change, the Review is deeply flawed. It does not provide a basis for informed and responsible policies.

ANNEX
The Stern Review and IPCC Scenarios

In this Annex, we examine the Stern Review's uncritical use of the IPCC's scenarios of future emissions of greenhouse gases, as published in the Panel's *Special Report on Emissions Scenarios* (SRES).

The detailed analysis in the Review's assessments of the potential impacts of climate change relies upon 'a series of papers prepared by Prof. Martin Parry and colleagues ("FastTrack")' which, according to the Review, represents 'one of the few that clearly sets out the assumptions used and explores different sources of uncertainty' (p. 61).

In choosing to use only four of the SRES scenarios in their analysis, Professor Parry and his colleagues disregarded one of the most important sources of uncertainty in the assessment of climate change impacts: the differing possibilities for the developments of energy technologies. The need to take these alternatives into account had been stressed in the Summary for Policymakers of the SRES:

> The *six* scenario groups – the three scenario families A2, B1, and B2, plus three groups within the A1 scenario family, A1B, A1FI, and A1T—and four cumulative emissions categories were developed as *the smallest subsets of SRES scenarios that capture the range of uncertainties* associated with driving forces and emissions. (SRES, p. 11, emphases added.)

Both the 'FastTrack' exercise and the Stern Review ignore two of the three groups within the A1 scenario family, and present the A1FI scenario as *the* emissions scenario in that family: see, for example, the tabulation of the demographic and economic data relating to the A1 scenario in Box 3.2 (p. 61) of the Review, and the presentation of more than 200 additional millions as at risk of hunger under a hypothetical temperature increase for 'A1' of over 4°C in Figure 3.6 (b) on p. 73. If the A1T scenario had been used instead of the A1FI scenario, the temperature increase on the horizontal scale and the 'additional millions at risk' on the vertical scale would both have been much smaller.

Importantly, the Terms of Reference of the SRES required that '*none* of the scenarios in the set includes any future policies that explicitly address additional climate change initiatives', and that 'For example, no scenarios are included that explicitly assume implementation of the emissions targets in the UNFCCC and the Kyoto Protocol' (SRES, p. 23, emphasis in original).

By choosing to analyse the impacts of the 'very high' economic growth scenario using only the A1FI (fossil fuel intensive) scenario, and disregarding other scenarios that share similar economic growth assumptions but have much lower levels of emissions, the 'FastTrack' studies and the Stern Review portray a fundamentally distorted view of the prospective impacts of climate change in the absence of mitigation policies.

This can be seen most readily by noting that the omitted A1T emissions scenario assumes a higher rate of economic growth, and a higher level of global GDP in 2100, than any of the four scenarios used in the 'FastTrack' studies; but that the cumulative level of emissions under this scenario, and the projected increase in global-mean temperatures that goes with it, are *lower* than under the B2 scenario—even though the latter scenario assumes the lowest rate of economic growth, and the lowest global GDP in 2100, of the four scenarios that are used in the 'FastTrack' analyses.[16] By relying entirely upon the A1FI variant of the A1 scenario family and ignoring the A1T variant of the same family, the Stern Review presents it as inevitable that, if rapid economic growth continues, emissions will continue to escalate in the absence of climate policies.

This view does not sit easily with the following statement in the SRES Summary for Policymakers:

> [T]here are scenarios with high per capita incomes in all regions that lead to high CO_2 emissions (e.g., in the high-growth, fossil fuel intensive scenario group A1FI)… [And] there are scenarios with high per capita incomes that lead to low emissions (e.g., the A1T scenario group or the B1 scenario family). (p. 11)

Further, the Review's interpretation is certainly inconsistent with the argument by 15 members of the SRES writing team in their initial response to the Castles and Henderson critique:

> The fact that 17 out of the 40 SRES scenarios explore alternative technological development pathways under a high growth … scenario family A1 does not constitute a statement that such scenarios should be considered more likely than others with a less dynamic technological and economic development outlook, nor that a similar large number of technological 'bifurcation' scenarios would not be possible in any of the other three scenario families … The special value of the criticized A1 and B1 scenarios resides precisely in the insight that such an income gap closure [between average incomes in developing and developed countries] might not necessarily be associated with extremely high GHG emissions *but could also evolve even in the absence of climate policies with comparatively low emissions* (as for instance in the technologically optimistic A1T and B1T scenarios). (Nakicenovic *et al.*, 2003, 'IPCC SRES Revisited: A Response', *Energy & Environment*, **14** (2 & 3): 195–96, emphasis added.)

[16] Cumulative projected levels of global CO_2 emissions under the A1T MESSAGE illustrative scenario are given on p. 446 of the SRES, and the corresponding total under the B2 MESSAGE marker scenario is given on p. 561. The projected increases in global-mean-temperatures under the two scenarios are given in IPCC, *Climate Change 2001: The Scientific Basis*, Appendix II, Table II.4 at http://www.grida.no/climate/ipcc_tar/wg1/552.htm.

It follows that the Review's claim that 'All but one SRES *storyline* envisage a concentration level [of greenhouse gases] well in excess of 650 ppm CO_2e by [the end of the century]' (p. 177, emphasis added) reveals a fundamental misreading of the SRES. The storylines presented in the Report do not in themselves envisage specific concentration levels at particular times in the future: these levels are also a function of the assumed technological development pathway.[17]

By focusing on the fossil fuel intensive variant of the A1 scenario, and ignoring the technologically optimistic variants or possible variants of the other scenario families, the Review neglects to consider the possibility that continuing growth in global emissions is not inevitable, even in the absence of climate policies.

The Review asserts that 'the likelihood of economic growth slowing sufficiently to reverse emissions growth by itself is small' (p. 182). This again reveals a misunderstanding of the SRES scenarios, all of which are presented as 'equally valid with no assigned probabilities of occurrence' (SRES, Box SPM-1, p. 4). Many of the scenarios project a reversal in emissions growth in the course of the century.

Besides presenting a distorted view, the Review is slipshod in its reporting of the SRES results. For example, the statement that the growth in world GDP under the SRES scenarios is projected 'to continue at between 2 and 3% per year' (p. 182 of the Review) cannot be reconciled with the growth rate of '3.5% p.a.' reported for the A1FI scenario in the table in Box 3.2 (p. 61). The difference is not trivial: over the 110-year time span of the SRES projections, growth at an average rate of 3.5% annually yields a GDP level in 2100 which is 70% greater than the level resulting from an average growth rate of 3.0% annually over the same period. The difference between the projected GDP in 2100 under a 3.5% growth rate from 1990 onwards and the GDP in 2100 resulting from a 3.0% growth rate over the same period is equivalent to nearly 20 times the level of global GDP in the base year of 1990.

The table in the Review's Box 3.2 reports a projected level of GDP under the A1FI scenario of $550 trillion in 1990 US $. The correct figure, as reported in the SRES, is $525 trillion (SRES, p. 436).

Finally, all of the estimates and projections of regional and global GDP in the SRES are distorted as a result of the use of exchange-rate-based conversions as if they measured differences in output across countries. The use of these flawed estimates and projections in the 'FastTracks' project raises in itself serious questions about the validity of the assessments of climate change impacts both in that exercise and in the Stern Review.

[17] It is worth noting that the specific role of the SRES is to project emissions, not concentrations.

References

Anderson, Dennis (2006), 'Costs and finance of carbon abatement in the energy sector', paper for the Stern Review, available at www.sternreview.org.uk

Barker, T., M. S. Qureshi, and J. Kohler (2006), 'The costs of greenhouse gas mitigation with induced technological change: A meta-analysis of estimates in the literature', 4CMR, Cambridge Centre for Climate Change Mitigation Research.

Bovenberg, A. Lans, and Lawrence H. Goulder (1996), 'Optimal environmental taxation in the presence of other taxes: General-equilibrium analyses', *American Economic Review,* **86** (4): 985–1000.

Bovenberg, A. Lans, and Ruud A. de Mooij (1994), 'Environmental levies and distortionary taxation', *American Economic Review,* **84**: 1085–9.

British Petroleum (2006), *Statistical Review of World Energy,* June, and www.bp.com/statisticalreview

Castles, Ian, and David Henderson (2003), 'The IPCC emissions scenarios: An economic–statistical critique', *Energy & Environment,* **14** (2 & 3): 173.

Cline, William R. (1992), *The Economics of Global Warming*, Washington, DC, Institute of International Economics.

Cline, William R. (2004), 'Climate change', chapter 1 of Bjørn Lomborg (ed.), *Global Crises, Global Solutions*, Cambridge University Press.

Dasgupta, P. (2006), 'Comments on the Stern Review's Economics of Climate Change', Presentation at the Foundation for Science and Technology at the Royal Society, London.

Fullerton, Don (1997), 'Environmental levies and distortionary taxation: Comment', *American Economic Review*, **87** (1): 245–251.

Helm, Dieter (2003), *Energy, The State and The Market,* Oxford University Press.

Henderson, David (2005), 'SRES, IPCC, and the treatment of economic issues: What has emerged?', *Energy and Environment*, **16** (3 & 4).

Henderson, David (2006), 'Governments and climate change issues: The case for a new approach', *Energy and Environment*, **17** (4).

House of Lords Select Committee on Economic Affairs (2005), *The Economics of Climate Change,* Volume I: Report; Volume II: Evidence, London, The Stationery Office.

McIntyre, Stephen, and Ross McKitrick (2003), 'Corrections to the Mann et al. (1998) Proxy Data Base and Northern Hemisphere Average Temperature Series', *Energy and Environment,* **14** (6): 751–771.

McIntyre, Stephen, and Ross McKitrick (2005), 'The M&M critique of the MBH98 Northern Hemisphere Climate Index: Update and Implications', *Energy and Environment,* **16** (1): 69–100.

McIntyre, Stephen, and Ross McKitrick (2005), 'Hockey sticks, principal components and spurious significance', *Geophysical Research Letters,* **32** (3), L03710 10.1029/2004GL021750, 12 February 2005.

McKitrick, Ross R. (2006), 'Bringing balance, disclosure and due diligence into science-based policymaking', in Porter, Jene (ed.), *Public Science in Liberal Democracy: The Challenge to Science and Democracy*, University of Toronto Press.

Manne, A., and R. Richels (1995), 'The greenhouse debate: Economic efficiency, burden-sharing and hedging strategies', *The Energy Journal*, **16** (4).

Nordhaus, William (2006), 'The *Stern Review* on the Economics of Climate Change', http://nordhaus.econ.yale.edu/SternReviewD2.pdf

Nordhaus, William, and J. G. Boyer (2000), *Warming the World: The Economics of the Greenhouse Effect*, MIT Press.

Parry, Ian W. H. (1995), 'Pollution taxes and revenue recycling', *Journal of Environmental Economics and Management,* **29**: S64–77.

Parry, Ian, Roberton C. Williams III, and Lawrence H. Goulder (1999), 'When can carbon abatement policies increase welfare? The fundamental role of distorted factor markets', *Journal of Environmental Economics and Management,* **37**: 52–84.

Robinson, Colin (1988), 'Britain's energy market', *The Economic Review,* **5** (3), January.

Sandmo, Agnar (1975), 'Optimal taxation in the presence of externalities', *Swedish Journal of Economics*, **77** (1): 86–98.

Smith, Stephen, and Joseph Swierzbinski (2006), 'Assessing the performance of the UK emissions trading scheme', revised submission to *Environmental and Resource Economics*, October.

Tol, Richard S. J. (2005), 'The marginal damage costs of carbon dioxide emissions: An assessment of the uncertainties', *Energy Policy*, **33**.

Tol, Richard S. J., and Gary Yohe (2006), 'A Review of the *Stern Review*', *World Economics,* **7** (4), October–December.

Warren, R., et al. (2006), 'Spotlighting impacts functions in integrated assessment models', Norwich, Tyndall Centre for Climate Change Research Working Paper 91.

Watkiss, P., et al. (2005), 'Methodological approaches for using social cost of carbon estimates in policy assessment', Final Report, AEA Technology Environment, Culham.

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