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The "Stern Review" and Its Critics: Implications for the Theory and Practice of Benefit-Cost Analysis

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> "Every cost-benefit analysis is an exercise in subjective uncertainty. If, as the *Stern Review* puts it, 'climate change is the greatest externality the world has ever seen,' then a cost-benefit calculation of what to do about it is the greatest exercise in Bayesian decision theory that we economists have ever performed."
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> – Martin Weitzman (forthcoming 2007)

INTRODUCTION

In 2003, the UK's Chancellor of the Exechequer, Gordon Brown, appointed Nicholas Stern, an Oxford economist and former World Bank chief economist and senior vice president, as second permanent secretary at HM Treasury. In July of 2005, Chancellor Brown asked Secretary Stern to head up an official governmental review of the economics of climate change. More than a year later, on October 30, 2006, the UK Treasury published its Review on the Economics of Climate Change (Stern 2007). This was hardly the first benefit-cost analysis on climate change to be published (see, e.g., Cline 1992; Nordhaus and Boyer 2000). It was, however, the first analysis to be issued with the imprimatur of a major government. Consequently, the Stern Review had unusually high political salience and potential to influence

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policy.

Unsurprisingly, politicians and NGOs that favor rapid and strong action to mitigate greenhouse gas emissions greeted the nearly 600-page "Stern Review" with uncritical adulation, while climate-change skeptics summarily bashed it.[1] More serious academic critiques of the Review were not long in coming. Several well-respected economists (including Nordhaus 2006; Dasgupta 2006; Maddison 2006; Mendelsohn 2006-7; Tol 2006; Yohe 2006; and Yohe and Tol 2007) have argued that the Review's assumptions, arguments and recommendations are seriously flawed, even biased. Others (including Quiggin 2006) have given the Review more generous readings. In perhaps the most nuanced and interesting of all the reviews, Weitzman (forthcoming 2007) deftly criticizes the Stern Review's assumptions and analysis, but goes on to suggest why, ultimately, the Review's conclusions might be sound. Weitzman's analysis, in particular, has important implications for the practice of BCA, especially as it applies to policies, like climate change, involving long time horizons and very high levels of uncertainty.

The purpose of this paper is to explain that the disagreements over the quality of the Stern Review (among serious scholars without ostensible political or ideological agendas) largely reflect disagreements about how BCAs generally should (and should not) be done. To that end, it focuses on the Stern Review's benefit-cost analysis from a process-oriented perspective. What can producers and consumers of benefit-cost analyses learn from the Stern Review and its critics about the theory and practice of benefit-cost analysis? The lessons are several, including many negative lessons about how benefit-cost analyses should *not* be done. Less obvious, but just as important, are one or two positive lessons from the Stern Review. The lessons, both positive and negative, relate predominantly to one aspect of the Stern Review: the choice of a social discount rate.

[1] Early responses to the Stern Review are compiled by Riboń (Dec. 2006) and The Center for Science and Public Policy (Nov. 2006).

A SUMMARY OF THE STERN REVIEW

The Stern Review does three things: (1) it assesses the likely costs of climate change up to 2200 under a "business as usual" (BAU) policy, where nothing is done to control emissions rates or atmospheric concentrations of greenhouse gases (GHGs); (2) it assess the costs and benefits of various levels of emissions mitigation/climate stabilization; and (3) it assess policy options in light of the analyses in (1) and (2).

The PAGE2002 Model

In order to assess the costs of climate change, a special kind of model is required to integrate scientific and economic information about GHG emissions, atmospheric concentrations, and impacts. The goal is to simulate the process of climate change starting from GHG emissions through atmospheric concentrations to climate effects and ultimately the socio-economic impacts of climate change. The Stern Review uses an Integrated Assessment Model (IAM) called PAGE2002,[2] which was developed by Cambridge economist Chris Hope (2006). Chapter 6 of the Stern Review provides some (but not all[3]) relevant details of the PAGE2002 model and how it was employed to derive climate change damage functions under various GHG emission trajectories.

The PAGE2002 IAM measures socio-economic impacts of climate change in terms of differential income growth rates under various scenarios ranging from zero climate change at one extreme to BAU at the other. Those two limits are joined by a continuum of points at which GHG emissions are mitigated to one extent or another and atmospheric concentrations of CO_2 are stabilized at various levels. The precise relations between GHG emissions and socio-economic effects of climate change are, of course, subject to high levels of uncertainty, which

[2] "PAGE" stands for "Policy Analysis of the Greenhouse Effect" (see Stern 2006, p. 153).

[3] The failure to discuss all relevant details of the model, itself, can be considered a possible violation of "best practices" for BCA, especially given the overall importance of the choice of model to the outcome of the BCA. However, all relevant details about the model are available in Hope (2006), on which this summary of the Stern Review relies extensively.

increase with time. Among other uncertainties is the prospect of a nonlinear damage function resulting from extreme climate events, climate feedback mechanisms, and other large-scale discontinuities. The Intergovernmental Panel on Climate Change's (IPCC) Third Assessment Report (TAR) (2001) expressed serious concern over such impacts, and Chris Hope designed PAGE2002 precisely to incorporate them.

The PAGE2002 IAM factors uncertainty into the analysis by incorporating a Monte Carlo analysis that repeatedly solves the model using different sets of 79 random variables for key parameters (Hope 2006, Appendix 1, pp. 36-7). Those variables include (among others) emissions of primary GHGs, atmospheric concentrations of GHGs, concentrations of sulphate aerosols that cool the atmosphere, regional temperature effects for eight regions of the world, nonlinear and transient damages, regional economic growth, investments in climate change adaptation measures, and the possibility of some future large-scale discontinuity (Hope 2006, pp. 20-1). The model is run 1000 times to generate "a range of possible trajectories for GDP per capita net of climate change related damage costs" (Maddison 2006, p. 4). It then produces a probability density function of economic damage outcomes for various GHG emission levels over time.

Evidently, it was precisely this ability of the PAGE2002 IAM to incorporate nonlinear damage functions, including potential catastrophic events, and nonmarket costs that led Sir Nicholas Stern to adopt that model rather than alternative IAMs.[4] Various statements throughout the Stern Review signal an intention to estimate not only the mean expected-values of climate change damage, but higher harm-based scenarios. For example, Stern writes that "the science and economics are particularly sparse precisely where the stakes are highest – at the high temperatures we now know may be possible" (Stern 2006, p. 34). Later, he states that "the knowledge base on which the cost of climate change is calibrated – specialized studies on impacts on agriculture, ecosystems and so on – is particularly patchy at high temperatures. In principle, the gaps that remain may lead to underestimates or overestimates of global impacts. In

[4] In contrast to PAGE2002, Mendelsohn et als' (1998) model does not account for nonmarket impacts or catastrophic events. Tol's (2002) model accounts for some nonmarket impacts, but does not account for catastrophic events. Nordhaus and Boyer's (2000) model accounts for catastrophic events but not nonmarket impacts. See Stern (2006, p. 150 fig. 6.3).

practice, however, most of the unresolved issues will increase damage estimates" (Stern 2006, p. 149). Stern then criticizes earlier studies for limiting their analyses "to a small subset of the most well understood, but least damaging, impacts" (Stern 2006, p. 150). In effect, earlier analysts focused only where the light was better, ignoring impacts "that have the potential to inflict the greatest damage" simply because those impacts were "surrounded by the greatest scientific uncertainty" (Stern 2005, pp. 150-1). Thus, the Stern Review seems to have been intended, at least in part, as a BCA of "*worse*-case" scenarios for climate change.[5]

In addition to facilitating the incorporation of large-scale discontinuities in the damages function, the PAGE2002 IAM had the advantage of closely tracking the scientific projections of the IPCC's TAR, falling short by only about 40 ppm of CO_2 concentration at the IPCC's mean projection under BAU of 700 to 800 ppm for the year 2100. PAGE2002's 5th percentile and 95th percentile projections were also very close to the outer bounds of the IPCC's TAR projections.[6]

Among the important projections of the IPCC's TAR, which profoundly influenced both the PAGE2002 IAM and the Stern Review's outcome was the finding that the impact of a large-scale discontinuity might exceed ordinary disasters by orders of magnitude. The chance of large-scale discontinuities is estimated to be significant only after mean temperature has risen 5ºC over pre-industrial levels, but for every 1ºC rise above that point the chance of a large-scale discontinuity increases by 10% (IPCC 2001, p. 947). PAGE2002 presumes a 10% loss to EU gross domestic product (GDP) from any large-scale discontinuity that does occur at or above temperature increases of 6ºC. In recognition that other regions of the world would suffer greater or lesser losses (or even make gains) from higher-than-expected temperature changes, expected

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[5] Weitzman (forthcoming 2007, p. 3) appears to agree with this assessment, when he notes that "the *Stern Review* consistently leans towards (and consistently phrases issues in terms of) assumptions and formulations that emphasize ... pessimistically-high expected damages from greenhouse warming – relative to most other studies of the economics of climate change." Whether or not the Stern Review's "worse-case" approach comports with (presumed) "best practices" in regulatory BCA, is a question addressed later in this paper.

[6] In a perfect world, the Stern Review might have been held back until the publication, in 2007, of the IPCC's Fourth Assessment Report (FAR), which significant altered some of the projections made in the 2001 TAR. To his credit, Stern incorporated into his model a good deal of the science on which the FAR is based. However, he was operating according to an Autumn 2006 deadline set by the Chancellor of the Exchequer.

income affects are weighted to allow for comparison and aggregation across regions (Hope 2006, pp. 23-4).

Because of the great uncertainties in the occurrence and effects of any large-scale discontinuity, PAGE20002 provides wide parameter ranges. But as Hope (2006, p. 25) and Stern (2006, p. 151) both note, it is important not to ignore large-scale discontinuities in estimating the economic effects of climate change just because they are subject to great uncertainty. Their intuition seems right: it is better to incorporate wide parameter ranges with high margins of error than to virtually ensure large errors by summarily excluding large-scale discontinuities from the damage function.

Applying the Model

The Stern group ran the PAGE2002 model under two different assumed levels of climatic response presuming BAU (that is, no controls on present or future emissions rates) (1) the "baseline" scenario designed to give outputs consistent with the IPCC's TAR (as in Hope 2006), and (2) a "high climate" scenario which adds the risk of nonlinear damages, resulting for example from natural feedbacks in the climate system. Obviously, the "high climate" scenario results in a higher probability of larger temperature changes and, consequently, higher expected social costs. According to Stern, "[i]n the high scenario, global mean temperature rises to an average of nearly 4.3ºC above pre-industrial levels by 2100, compared with an average of 3.9ºC above pre-industrial levels in the baseline scenario" (Stern 2006, p. 158). And the temperature difference between the two scenarios increases after 2100. By 2200, the average increase in mean global temperatures increases by 7.4ºC above pre-industrial levels on the baseline scenario, and by 8.6ºC on the "high climate" scenario (Stern 2006, p. 158). Stern warns that temperature increases, especially beyond 2100, should be treated as "indicative" only because uncertainty about effects increases with time (Stern 2006, p. 158). But he ignores his own words of caution in deriving preliminary estimates of average losses in global per capita GDP in 2200 ranging from 5.3% to 13.8%, "depending on the size of climate-system feedbacks and what estimates of 'non-market impacts' are included" (Stern 2006, p. 155). He does, however, add the important caveat that "growth in GDP will have made the world considerably richer than it is now" (Stern

2006, p. 158).

The size of climate change costs is not the only important consideration. The distribution of those costs also matters. Stern notes (in accordance with virtually every other analyst) that the costs of climate change will not be distributed evenly or randomly throughout the world. Rather, the costs are expected to fall most heavily in less developed regions of the world that (a) have contributed least (so far) to GHG emissions and (b) can least afford to bear the costs of climate change (accord Schelling 2006a, pp. 34-5). "[I]n the baseline-climate scenario with all three categories of economic impact [market, risk of catastrophe, and non-market], the mean cost to India and South-East Asia is around 6% of regional GDP by 2100, compared with a global average of 2.6%" (Stern 2006, p. 158). The inequitable distribution of climate change costs introduces an important ethical component into climate change policy, which the Stern Review does not neglect.

In deriving its damage figures, the Stern Review employs standard economic assumptions from welfare economics theory, including the assumption of diminishing marginal utility of income in evaluating risks and future welfare. But then Stern deliberately courts controversy by choosing a very low pure rate of time preference (utility discount rate) of 0.1% per year at which to convert future streams of costs and benefits into present day dollars to facilitate expected value calculations. I will postpone discussion of the controversy engendered by this choice until the next section. For present purposes, we require only a proper understanding of the role the discount rate plays in the Stern Review's calculations. In particular, we need to distinguish carefully between pure rate of time preference and the interest rate used to discount consumption (see Weitzman forthcoming 2007, p. 5). In Frank Ramsey's modern formulation of Irving Fisher's dynamic general equilibrium model of the interaction of preferences and technological change

$$r = \rho + \eta g,$$

where r is the interest rate that discounts consumption, ρ is the pure rate of time preference, η is

the elasticity of marginal utility or the base-case coefficient of relative risk aversion,[7] and g is the per-capita growth rate of consumption (Weitzman forthcoming 2007, p. 5). In the Stern Review, $\rho = 0.1$, $\eta = 1$, and $g = 1.3$. Plugging these figures into the Ramsey formula, gives us a value of r (discount rate of consumption) = 1.4%. As Weitzman (forthcoming 2007, p. 7) notes, Stern's choice of a very low pure rate of time preference (ρ) and a relatively low coefficient of relative risk aversion (η) results in an unusually low consumption discount rate (r). Compared to other, more conventional discount rates, the Stern Review's $r = 1.4\%$ leads to estimated damage costs 100 years from now that exceed by two orders of magnitude estimates using a more conventional $r = 6\%$ (where ρ, η, and g each = 2) (Weitzman forthcoming 2007, p. 7).[8]

Based on its low parameter values, the Stern Review's analysis shows that under a BAU scenario (that is, uncontrolled GHG emissions indefinitely into the future), the risks of climate change between 2100 and 2200 will be equivalent to about 5% of global GDP each year, and possibly as high as 20%, forever (Stern 2006, p. 163). Again, with different parameter values yielding $r = 6\%$, those damage estimates would be only 1/100th as large.

The Costs and Benefits of Carbon Mitigation/Climate Stabilization

Next, Stern assesses the economics of mitigation/stabilization. In accordance with IPCC data, he finds that stabilization of GHG concentrations necessary to avoid high costs of climate change will require "deep emissions cuts of at least 25% by 2050" (Stern 2006, p. 168). Nominally, that would be a decline of 30-35 $GtCO_2$ (Stern 2006, p. 201). To achieve that goal,

[7] John Quiggin (2006), explains the effect of the choice of η ("eta"): "Using eta = 1, a sacrifice of $70 per person (1 per cent of income) today would be justified if (and only if) it increased the income of our great-grandchildren in 2100 by at least $1,000. If this trade-off appears reasonable, then a value of eta = 1 is appropriate. If the future payoff required is higher (or lower) then so is the preferred value of eta." Put differently, η is the measure of how much someone would be willing to pay today to insure against some expected future loss.

[8] As Weitzman (forthcoming 2007, p. 8) notes, this "no frills stripped-down variant of the Ramsey model ... is liable to a thousand and one legitimate questions and criticisms about its oversimplifications, but at the end of the day ... this exercise is highlighting fairly what really counts in the economics of climate change."

emissions would peak during the next 10 to 20 years, and then decline by between 1% and 3% per year (Stern 2006, p. 201). The Stern Review estimates that cutting emissions 25% by 2050 cuts would cost approximately 1% of annual global GDP (about $1 trillion in 2050), plus or minus 3%, and would stabilize concentration levels at between 500-550 ppm of carbon dioxide equivalent (CO_2eq) (Stern 2006, pp. 168, 240). At such concentration levels, the most harmful effects/costs of climate change would be averted. Thus, by investing 1% of annual global GDP starting now (and continuing potentially forever) we could forestall costs to annual global GDP of 10% forever (Stern 2006, p. 285). In the "worst case" scenario, climate change mitigation would yield net global costs of 3.4% of annual global GDP. In the "best case scenario," climate change mitigation would yield net global benefits of 3.9% of annual global GDP (Stern 2006, p. 242).

Finally, the Stern Review considers policy options for mitigating GHG emissions, and uncontroversially recommends a Pigouvian carbon tax or tradeable permitting system, which would establish a price of carbon at or near the social cost level (Stern 2006, p. xviii). While the Stern Review presents a fairly comprehensive and detailed introduction to various policy options, it does not actually engage in a comparative BCA (or comparative cost-effectiveness analysis) of alternative policies for achieving its goal of climate stabilization at 500-550 ppm of CO_2eq by 2050.

CRITIQUES OF THE STERN REVIEW (RELATING TO THE THEORY AND PRACTICE OF REGULATORY BCA)

We now turn our attention to the critics of the Stern Review. Our focus will be on those criticisms that have the most salience for the general theory and practice of regulatory BCA. We will not, for example, attempt to assess or resolve complaints that Stern misinterpreted or misapplied scientific data (see, e.g., Carter et al. 2006). We take it as given any set of "best practice" standards for BCA would require use of the best available information inputs with due care to avoid errors.[9] We further assume that the Stern group endeavored to comply with this

[9] It is, perhaps, the first "best practice" standard of regulatory BCA to exercise due care in avoiding the problem of "garbage in, garbage out." Some reviewers have argued that Stern (2006) did not

requirement in good faith.[10] If good-faith errors of scientific fact have occurred, then that authors certainly should correct them in a supplemental BCA. We also we take for granted that any discrepancies between the science-basis of the Stern Review and IPCC's FAR (2007) should be dealt with in a supplemental BCA, prepared under auspices of HM Treasury either applying the same model used by the Stern group or explaining why the new data require changes in the model. Having cabined simple factual errors and subsequent improvements to the knowledge base, we can proceed to address what is truly significant in the Stern Review for the theory and practice of regulatory BCA.

Relevant critiques of the Stern Review focus on its assumptions about parameter values, including the discount rate, the choice of the η, and the rate of growth in per capita consumption. Regardless of alleged deficiencies in the Stern Review's damage estimates (see Mendelsohn 2006-7, p. 44; Yohe and Tol, 2007, p. 40 Table 1), nearly all of the differences in outcomes between the Stern Review and earlier studies can be accounted for by discrepancies in these parameter values.

Is ρ = 0.1% too low?

The single most controversial aspect of the Stern Review undoubtedly has been the choice of a 0.1% pure rate of time preference (ρ). The reason, of course, is that this choice

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avoid this problem. Mendelsohn (2006-7, p. 44), for example, argues that the Stern Review failed to present evidence supporting its quantification of nonmarket climate change effects, and failed to factor adaptation projects, such as dams and seawalls, into its climate change damage calculations. Yohe and Tol (2007, p. 40 Table 1) also provide "reasons for concern" about the Stern Review's damage estimates.

[10] Yohe and Tol (2007, p. 39) allege that the Stern Review "subjected academic standards to political goals." Similarly, Nordhaus (2006, p. 5) suggests that "the *Review* should be viewed as a political document"rather than "an academic study." Such statements fall short, however, of alleging deliberate manipulation of inputs or procedures. In any case, neither Nordhaus nor Yohe and Tol provide any evidence of deliberate manipulation. Since everyone knows that the selection of a very low discount rate raises the present value of future costs of climate change and the future benefits of actions to avoid those costs, it is doubtful that Stern would use such an obvious ploy simply to massage the numbers. Moreover, as we shall see, Stern does not attempt to apply his 0.1% discount rate surreptitiously, but provides substantial and facially valid justifications for it. We might agree or disagree with his choice, but he had legitimate reasons – aside from instrumental political values – for it.

dramatically effects the overall outcome of the BCA. As noted earlier, the Stern Review's low ρ, when combined with a low coefficient of risk relative aversion ($\eta = 1$) yields a low interest rate ($r = 1.4\%$) for discounting future consumption, which magnifies future damages by two orders of magnitude over a more conventional 6% interest rate for discounting future consumption. As Yohe and Tol (2007, p. 39) observe, "[m]oving from a discount rate of 0.1 percent to 1 percent would lower damage estimates by nearly 60 percent; moving to 2 percent by roughly another 20 percent, and moving to 3 percent by another 15 percent. As a result, damages calculated from the same underlying data with a 3 percent discount rate would produce damage estimates between 10 and 20 percent of the estimates reported in the *Review*."

In fact, the choice of a very low pure rate of time preference in the Stern Review results in damage estimates under BAU far higher than those derived in any other economic study of climate change ever conducted. Tol (2005) provides a meta-analysis of 28 published studies providing a total of 103 estimates of marginal damage costs of carbon dioxide emissions. The utility discount rates in those 28 studies ranged between 1 and 3 percent (with some using hyperbolic discounting). Among those studies, Tol (2005, p. 2072) found that the utility discount rate/pure rate of time preference has a

> stark[] influence on the central estimate but particularly on the uncertainty. If we use a pure rate of time preference of 3% – corresponding to a social rate of discount of 4-5%, close to what most western governments use for most long-term investments – the combined mean estimate is $16/tC (ton of carbon), not exceeding $62/tC with a probability of 95%. Lower social rates of discount lead to higher estimates but particularly to greater uncertainty, but even for a 1% pure rate of time preference the combined mean is $51/tC. Even lower discount rates may be morally preferable, but are clearly out of line with common practice.[11]

[11] The last sentence of the quote from Tol (2005) is intriguing. If a lower discount rate is "morally preferable" but "out of line with common practice," which should be adjusted, the "morally preferable" discount rate or the common practice? As we shall see later, Weitzman (forthcoming 2007) makes a strong ethical argument that the discount rate should be made to comport to convention.

Compare Tol's (2005) combined mean estimates with the Stern Review's (2006, p. xvi) estimate of \$85/tCO_2eq under BAU. No wonder the Stern Review calls for quicker and steeper reductions in GHG emissions than any previous BCA. This is not to say, however, that the Stern Review's cost-estimate is necessarily wrong. As Tol (2005, p. 2067) himself notes earlier in his meta-analysis:

> The current generation of aggregate estimates may understate the true cost of climate change because they tend to ignore extreme weather events; exclude low probability/high consequence scenarios, such as a shut-down of the thermohaline circulation ... or a collapse of the West-Antarctic ice sheet...; underestimate the compounding effect of multiple stresses; and ignore the costs of transition and learning.

Each of the factors missing from the earlier studies is present and accounted for in the Stern Review. That must justify a higher cost-estimate. But how much higher? Tol (2006, p. 979) concludes that the Stern Review "overestimates the impacts of climate change, and therefore the benefits of emission reduction."

William Nordhaus (2006) agrees with Tol's assessment, and concludes that it the Stern Review's choice of a very low ρ, more than any other factor, that explains why the Stern Report's results differ so dramatically from those of other climate change BCAs, including his own:

> The *Review* proposes using a social discount rate that is essentially zero. Combined with other assumptions, this magnifies enormously impacts in the distant future and rationalizes deep cuts in emissions, and indeed in all consumption, today. If we were to substitute more conventional discount rates used in other global-warming analyses by governments, by consumers, or by

businesses, the *Review*'s dramatic results would disappear...."[12]

Of course, Nordhaus has a dog in this fight. He has been conducting economic analyses of climate change for nearly 20 years, and his more recent studies have utilized a 3% discount rate, declining to 1% per year over 300 years. The Stern Review expressly argues that Nordhaus's studies using the DICE IAM (among other IAMs) have systematically underestimated the costs of climate change under a BAU scenario (Stern 2006, pp. 149-52).[13] By attacking the Stern Review's relatively low parameter values, Nordhaus (2006) is defending his own choices, which are in fact more consistent with the rest of the literature (if only because Nordhaus has so greatly influenced the literature).[14]

It is not just the choice of a low pure rate of time preference that bothers reviewers. Mendelsohn (2006-7, p. 43) rightly complains that the Stern Review fails to apply its discount rate consistently throughout the BCA. It discounts the costs and benefits of climate change under BAU, but fails to discount the costs of mitigation. "To be consistent," Mendelsohn notes, "the opportunity cost of investing in mitigation must also be valued using the same discount rate as was used to determine the cost of climate change. Because investing in mitigation substitutes for investing in other activities that can earn the market rate of interest, society loses the income that it could have gained from other valuable projects."

But why did Stern (2006) choose such a low discount rate in the first place? Was it a politically motivated choice to inflate cost estimates so as to justify more rapid and extreme measures to control GHG emissions? While the Stern Review is replete with expressions of concern that earlier studies had under-estimated the costs of climate change, Stern certainly does

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[12] More technically, Nordhaus (2006, p. 8 n. 11) notes that "[z]ero discounting leads to deep mathetmatical problems such as non-convergence of the objective function and incompleteness of the functional.

[13] Other critics have made similar arguments about Nordhaus's model. See, e.g., Cline (2004); Ackerman and Finlayson (2005); Quiggin (2006).

[14] In an e-mail message he sent to Sir Nicholas Stern, Nordhaus is reported to have to written the following: "'I think it's a great study, but it's 50 years ahead of its time.... Since everybody else is 50 years behind the times, if you average the two, you might come out just right" (Leonhardt 2007).

not attempt to justify the choice of a low discount rate on this basis alone. Rather, he follows the lead of several other prominent economists such as Frank Ramsey, Amartya Sen, and Robert Solow, and presents affirmative and legitimate *ethical reasons* against anything higher than a minimal discount rate for estimating the costs of climate change (Stern 2006, p. 45). Ramsey (1928, p. 261) famously argued that discounting is "ethically indefensible and arises merely from the weakness of the imagination." The late David Pearce (2003, p. 122) pointed out that discounting is a "brute fact" because people "do discount for time and for space." However, that "brute fact" does not contradict Ramsey's point. The decision to discount and the choice of a social discount rate is not just a matter of positive economics but normative ethics. Sir Nicholas Stern, for one, thinks that people should not discount for time and space. In fact, the only reason the Stern Review uses a 0.1% pure rate of time preference rather than zero rate is the slim risk of human extinction during the course of the current century.

The bottom-line question is whether the Stern Review's pure rate of time preference is so low as to violate some presumed "best practice standard." As a matter of theory and practice, this is a difficult question to answer. The choice of social discount rate is inherently subjective, and as Portney and Weyant (1999, p. 4) have observed, "[t]hose looking for guidance on the choice of discount rate could find justification [in the literature] for a rate at or near zero, as high as 20 percent, and any and all values in between." Thus, the Stern Review's choice of a discount rate close to zero does not seem invalid *per se*. That it deviates from the literature does not make it wrong, only deviant. We might conclude that deviance from an accepted norm would violate a duty of BCA producers not to impose their own values paternalistically on society.[15] But it is not at all clear that there is yet such an accepted norm for selecting social discount rates.[16]

Is $\eta = 1$ too low?

One prominent critic of the Stern Review, Partha Dasgupta (2006) has no complaint with Stern Review's selection of 01.% for the pure rate of time preference. This should not surprise

[15] Weitzman (2007, pp. 8-9) makes this point.

[16] We will return to this question in the following section.

us, given that Dasgupta himself has suggested that the discount rate for climate change might be zero or even negative if costs are severe enough to require reductions in consumption (Dasgupta, Mäler, and Barrett 1999). Moreover, Dasgupta notes that there is precedent for Stern's choice of a low ρ. William Cline's (1992) pioneering economic analysis of global warming, like the Stern Review, set the pure rate of time preference at zero (Dasgupta 2006, p. 5).[17]

However, Dasgupta is highly critical of the Review's choice of η (the elasticity of marginal utility or coefficient of relative risk aversion). "To assume that [η] equals 1 is to say that the distribution of well-being among people doesn't matter much, that we should spend huge amounts for later generations even if, adjusting for risk, they were expected to be much better off than us" (Dasgupta 2006, p. 7). A high η would "imply greater sensitivity to risk and inequality in consumption" (Dasgupta 2006, p. 8).

To give a feel for how changing the eta alters consequences, Dasgupta provides the following example. If the rate of return on investment is 4% per annum, under the Stern Review's assumptions of $\rho = 0.1$ and $\eta = 1$, the social saving rate ought to be 97.5%, which is so out of line with social practice as to be ridiculous. If, however, we altered the Stern Review's assumptions so that η equaled 3, the optimum saving rate would fall to a far more reasonable (but still pretty darn high) rate of 25% of net aggregate output. Dasgupta (2006, p. 8) notes that the Stern Review contains no sensitivity analysis for its set of assumptions about the values of ρ, η, and g.[18] From the perspective of "best practices" for BCA, this is probably his most significant

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[17] In fact, Cline (1992) adopted parameter values that were very close to those chosen by Stern (2006). In Cline (1992), $\rho = 0\%$, $\eta = 1.5$, and $g = 1\%$, which in the Ramsey model yields $r = 1.5\%$, a mere one-tenth of one percent above the Stern Review's value for r. And Weitzman (forthcoming 2007, p. 13) notes that, like Stern (2006), Cline (1992) reached "strong activist conclusions."

[18] In addition to his critique of the Stern Review's choice of η, Dasgupta (2006, p. 8) makes a further salient observation about the public choice implications of the Review's conclusion that the world should spend 1% of global GDP annually to combat climate change. He reasonably assumes that all of the spending would come from the world's developed countries, and that 1% of global GDP would equal 1.8% of developed countries GDP. But, he notes, that figure would be "some seven times the annual global aid budget." Implicit is the assumption that spending to mitigate global climate change constitutes foreign aid. This assumption receives strong support from Tom Schelling's assessment of the issue. According to Schelling (2006a and 2006b), spending to counteract climate change constitutes foreign aid because the primary (if not exclusive) "investors" would be developed countries, while the primary (though not exclusive) beneficiaries would be developing countries, which are expected to bear the brunt of climate change costs.

complaint.

John Quiggin (2006, p. 15) takes issue with Dasgupta's argument about the Stern Review's valuation of η based on a presumed 4% market rate of return on investment. According to Quiggin, "the fact that we see more rapid growth with lower rates of net saving seems to imply that there must exist many projects with rates of return greater than or equal to 4 percent." In addition, "[i]n an economy where most growth in consumption arises from technical progress, the optimal rate of saving is far lower than that derived by Dasgupta. Finally, "[a] more direct way of refuting Dasgupta's argument is to observe that the major premise must be false. If there existed an infinite supply of projects with riskless returns of 4 per cent, the rate or return on riskless bonds would have to equal 4 per cent, rather than the 1 to 2 per cent observed in practice."

It's the *r* stupid!

As a practical matter, neither the choice of ρ (pure rate of time preference) nor η (coefficient of the relative rate of risk aversion), alone, determines the consumption discount rate (r) under the Ramsey model. Rather, it is the combination of those factors along with g (the assumed growth rate of per capita consumption) which determine r – the interest rate at which future consumption is discounted. As Weitzman (forthcoming 2007) usefully reminds us, it is the r that ultimately matters most in BCA. As we have seen, the Stern Review selects the following values: $\rho = 0.1\%$, $\eta = 1$, and $g = 1.3\%$ to yield $r = 1.4\%$. Weitzman (forthcoming 2007, p. 2) agrees with Nordhaus (2006) and Tol (2006) that "[t]he discount rate we choose is all important

Assuming, with Dasgupta and Schelling, that spending on climate change mitigation would constitute foreign aid (entirely or predominantly), a key question becomes how to persuade voting publics in developed countries to instruct their governments, collectively, to increase foreign aid spending so dramatically. Dasgupta suggests that simply stating it as a moral imperative is unlikely to be effective. But, of course, the Stern Review does not simply make a moral argument about the need for more spending to minimize the social costs of climate change; its argument is first and foremost economic. If spending more now can reduce costs down the road (after those costs are discounted to present value), then the choice is a matter of both economic efficiency and ethics.

and *Stern's* results come from choosing a very low discount rate."[19] However, his own analysis suggests that the ρ cannot carry that much responsibility. Weitzman (forthcoming 2007, p. 6) notes that if we assume that $\rho = 2\%$, $\eta = 2$, and $g = 2\%$, then $r = 6\%$. If we assume instead that $\rho = 0\%$, $\eta = 3$, and $g = 2\%$, then r still = 6%. Thus, we can derive the same consumption discount rate whether ρ equals 2% or 0% by adjusting the η and/or g.

The Stern Review's $r = 1.4\%$ is comparatively low because the selected rate of time preference is very low *and* the value chosen for the coefficient of the relative rate of risk aversion, $\eta = 1$, is "the lower bound of just about any economist's best-guess range" (Weitzman forthcoming 2007, pp. 6-7). Weitzman (forthcoming 2007, p. 7) points out that under an alternative $r = 6$, the Stern Review's presented discounted value of damages from climate change over the next 100 years would be lower by two orders of magnitude. And so, "what really counts in the economics of climate change [is] the hidden discounting assumptions whose role tends to be more obscured than informed by the big IAMs" (Weitzman forthcoming 2007, p. 8).[20] Of course, this is not just a criticism of the Stern Review and its choice of parameter values. Rather, it is a challenge to the entire practice of economics analysis of climate change. But it seems particularly relevant to the Stern Review simply because its authors assumed parameter values that ignored how people are, in fact, observed to act. As Weitzman (forthcoming 2007, pp. 8-9) puts it:

> *Stern*'s worldview tends to blow off market-based observations and behavioral influences as being (for a variety of reasons including market incompleteness) largely irrelevant to long-run discounting, which should instead be based primarily on the 'ethical' value $\rho \approx 0$ that *Stern* imposes on *a priori* grounds.... While there may be *something* to *Stern*'s position about the limited relevance of

[19] He does not argue, however, that Stern was wrong to adopt such a low pure rate of time preference, but only notes that "*Stern* follows a decidedly-minority paternalistic view (which, however, includes a handful of distinguished economists" (Weitzman forthcoming 2007, p. 6).

[20] It is not clear to the present why Weitzman believes that the discounting assumptions in Stern (2006) or other models have been "hidden." They are debatable to be sure, and they certainly seem to drive the outcomes not only of Stern (2006) but Nordhaus and Boyer (2000) as well, among other economic analyses of climate change.

> market-based inferences for putting welfare weights on the utilities of one's great-grandchildren, and there might be *some* sporadic support for *Stern*'s preferred taste parameters scattered throughout the literature, I ultimately find such an extreme stance on the primacy of $\rho \approx 0$, $\eta \approx 0$ unconvincing when super-strong policy advice is so dependent upon nonconventional assumptions that go so strongly against mainstream economics.[21]

In essence, Weitzman's argument is about the value of convention and prudence in BCA: analysts should not rest strong and socially costly policy recommendations on conclusions from analyses that depend so heavily on unconventional assumptions, especially when those assumptions run counter to observed economic behavior. In addition, Weitzman seems to be making an important point about the role of economic analysts: it is not their job to impose their own values and preferences on society. Their job, rather, is to input the best data available in a responsible fashion, and analyze that data using models based on prudent assumptions that are broadly consistent with the way people are actually observed to behave. Then, they can present the results to policy-makers, whose job it is to make decisions that either comport with or "go ... strongly against mainstream economics." If I am interpreting him correctly, Weitzman is making a strong ethical argument about how BCAs should and should not be done.

Or is it the *g*?

Although Weitzman strongly criticizes the Stern Review for its assumptions and analytical weaknesses, he is not so ready as other reviewers to denounce its conclusions or its policy recommendations. Indeed, he suggests that the Stern Review's value of *r* may end up "being more right than wrong when full accounting is made for the uncertainty of the discount rate itself, which is arguably the most important uncertainty of all in the economics of climate change" (Weitzman forthcoming 2007, p. 9). Why? Because "[t]he very same force of

[21] Later in his review, however, Weitzman (forthcoming 2007, pp. 15-6) shows that observed economic behavior creates problems not just for Stern's (2006) choice of parameter values but "threatens all such formulations."

compound interest that makes costs and benefits a century from now seem relatively insignificant, and that additionally creates the 'majority tilt' of a pain-postponing climate policy ramp of emissions reductions starting from a low gradual base [as in Nordhaus's model], also forces us to recognize the logic that over such long periods we should be using interest rates at the lower end of the spectrum of possible values" (Weitzman forthcoming 2007, pp. 9-10). Specifically, Weitzman (forthcoming 2007, p. 27) suggests that uncertainty over which discount rates to use for the costs and benefits of climate change a century from now might reduce the value of r from 6% to as low as 2%, which not far above the Stern Review's value of r.

Weitzman plugs uncertainty into the Ramsey model. I will not reproduce his formal modeling here, but his argument boils down to this: the kinds of parameter values Stern (2006) uses (and Cline 1992 used) may be justified because the damages of climate change over the next 100 years may not coordinate well with aggregate economic activity. The economic sectors most likely to be harmed by climate change, such as agricultural, outdoor recreation, and natural landscapes (including nonmarket ecosystem values) are not "highly correlated with technological progress in computer power, furniture making, or better pharmaceuticals a century from now" This creates a problem for those who favor basing the discount rate on the economy-wide return on capital. The only other option is to base the discount rate on the risk-free rate of return, which "is close to the *Stern* interest rate." The "moral" Weitzman draws "is that the nature of the impacts of climate change determine whether we should end up closer to using the risk-free rate or the economy-wide return on capital." But "trying to forecast costs and benefits of climate change scenarios a hundred years or so from now is more the art of inspired guesstimating by analogy than a science (imagine forecasting today's world a century ago) (Weitzman forthcoming 2007, pp. 14-16).[22] Is Weitzman suggesting that the state of the art of economic analysis is not yet up to the task of dealing with problem as potentially large and long-term as climate change? The last three sections of Weitzman's review suggest that the answer to this question is a qualified "yes."

The problem, in a nutshell, is the wide range of possible temperature increases under the IPCC's (2007) most current climate change models, including a 3% possibility that temperature

[22] Shelling (2006a, pp. 33-4) makes a similar point.

increases will exceed 6ºC by 2150. Weitzman (forthcoming 2007, p. 18) notes that "any honest economic modeler would have to admit"complete uncertainty about the social, economic, and environmental effects of such a temperature increase because "such high temperatures have not existed for some tens of millions of years." Even if their probability is low, high temperature increases – the kind of "worse-case" and worst-case scenarios with which the Stern Review seemed obsessed – could result in what Weitzman (forthcoming 2007, p. 19) calls "low-g disasters" (especially if g is defined to include existence values of ecosystems). The possibility of such disasters make prediction uncertain in the Knightian sense. As Weitzman (forthcoming 2007, p. 19) puts it, "[w]ith an extraordinary sochastic process like global climate change, the world is not standing still long enough for us to accumulate the relevant information to accurately assess tail probabilities." We don't even know how much we don't know about the probabilities. The structural uncertainties, Weitzman (forthcoming 2007, p. 22) notes, are extremely likely to matter more than the risk for "whomever wants to model optimal-expected-utility growth under endogenous greenhouse warming."

Weitzman (forthcoming 2007, pp. 22-4) lauds the Stern Review for attempting to treat seriously the possibility of rare, high-temperature, "low-g" catastrophes, but thinks that Stern (2006) should have dealt with them more forthrightly, rather that "through the back door with unreasonably low values of ρ and η." Presumably, this means that damage estimates should be increased or the value of g should be lowered based on some, admittedly error-prone, calculation of the anticipated effects of high-temperature changes on consumption levels. But Weitzman also doubts the ability of Stern, or any other economic analyst, to perform such calculations given the current state of scientific and economic knowledge. And he faults Stern "for giving readers an authoritative-looking impression that seemingly-objective best available-practice professional economic analysis robustly supports it conclusions, instead of more openly disclosing the full extent to which the *Review*'s radical policy recommendations depend on controversial extreme assumptions and unconventional discount rates that most economists would consider much too low" (Weitzman forthcoming 2007, p. 28).

Finally, Weitzman (forthcoming 2007, p. 25) urges caution in approaching these problems in economic analyses. "A responsible policy approach neither dismisses the horror stories just because they are two standard deviations away from what is likely nor gets

stampeded into overemphasizing false dichotomies as if we must make costly all-or-nothing investment decisions right now to avoid theoretically-possible horrible outcomes in the distant future." Thus, he recommends a "middle course" that rejects the Stern Review's call for an "all-out war" on GHG emissions, but combines Nordhaus's gradual implementation of increasingly stringent GHG emission reductions with serious research into low-probability, high-magnitude ("low-*g*") events "and what might be done realistically about them should they start to materialize" (Weitzman forthcoming 2007, p. 26). For now, we simply "lack a commonly-accepted usable economic framework for dealing with these kinds of thick-tailed extreme disasters, whose probability distributions are inherently difficult to estimate (which is why the tails must be thick in the first place)" (Weitzman forthcoming 2007, p. 26).

Implications for the Theory and Practice of BCA

What are the implications of the Stern Review and its critics for the theory and practice of BCA? I think there are several potential lessons, which are listed below and only briefly discussed in no particular order of importance (although I will have something to say about the relative importance of various lessons).

On Confidence and Caveats

BCAs about problems and policies at the frontiers of scientific knowledge are bound to be controversial and error prone, especially where time horizons are long and uncertainty looms large. In some cases, they are likely to be little more than shots in the dark. In such circumstances, authors should be especially parsimonious about the conclusions they draw and the recommendations they make. Nearly all reviewers agree that the Stern group pretended to greater confidence in their comparatively radical conclusions and recommendations than the facts and analysis warranted. While the Stern Review contained several warnings about the problems associated with forecasting the costs and benefits of climate change and mitigation, it did not heed its own warnings when presenting its conclusions and recommendations.

On the Inherently Political Nature of BCAs and the Importance of Sensitivity Analyses

The Stern Review was a political and ethical document, as much as it was an economic study. Several reviewers consider this a significant failing. But while the Stern Review may have been more politicized than other BCAs, it would be wrong to suppose that ethically-, politically-, or ideologically-based choices can ever be avoided completely in preparing BCAs. Given all of the subjective elements of BCA, from the valuations of nonmarket goods (including human lives) to the choice of value parameters (including discount rates), and given the influence those subjective elements invariably assert over outcomes, each and every BCA will obviously be informed by the ethical, political and/or ideological predilections of its author(s). A chief virtue of BCAs as a decision tool is that they make those predilections transparent (at least as compared to other decision tools) because, according to "best practices," authors must make their assumptions, including choices of parameter values, explicit.

The authors of the Stern Review generally complied with this "best practice"standard. They may have ignored observed economic behavior in specifying discount rates based on paternalistic ethical values, but at least they were explicit about both the discount rates they utilized and the ethical values that informed their choices. Having said that, the Stern Review authors certainly should have included a sensitivity analysis in their BCA. Sensitivity analyses should be standard practice because the choice of parameter values *always* has some effect on outcomes; and sensitivity analysis is helpful in demonstrating the relative robustness of outcomes across alternative parameter values. In the case of the Stern Review, a sensitivity analysis is especially important because of the extraordinary significance of their parameter-value choices in the context of long-run climate change. In response to their critics, they eventually did prepare what Martin Weitzman (forthcoming 2007, p. 7) has called "a halfhearted sensitivity-analysis postscript." That sensitivity analysis purported to demonstrate the robustness of the Review's conclusions but really only succeeded in demonstrating the sensitivity of those conclusions to the choice of parameter values (something that had already been demonstrated in critiques of the Stern Review). That postcript did, at least, explicitly assert that "ethical positions cannot be dictated by policy analysts."

On Discounting

Perhaps the most obvious lesson from the Stern Review and its critics (at least for those who had not previously learned it) is that choices of parameter values (discount rates, coefficients of relative risk aversion, and per capita consumption growth rates) can greatly influence the outcome of BCAs. Unfortunately, the Stern Review and its critics also remind us of just how far away we remain from being able to specify a consensus "best practice" for selecting those parameter values. Many (but not all) reviewers complain that the Stern Review's choice of a 0.1% pure rate of time preference is too low. This assessment is supported by two reasons: (a) such a low discount rate ignores how people actually behave in markets; and (b) it deviates significantly from some "conventional range" of discount rates in the BCA literature. Both of these assertions are true, and yet they cannot justify, alone or combined, a conclusion that Stern's (2006) choice of discount rate was "wrong" or violated some "best practice" of BCA.

It is obviously true that market participants almost always display implicit discount rates higher than that adopted by the Stern Review, but that is neither a necessary nor a sufficient reason for governments to adopt similarly high discount rates in framing policies to deal with long-run social-cost problems, particular where those problems are largely the result of the short time horizons and high discount rates of market participants. Moreover, as Stern (2006) rightly points out, the choice of discount rate in regulatory BCA is not just a question of mimicking the market; it is an ethical judgment.

But is it the job – or the right – of a BCA author to make ethical or political judgments that can determine the outcome of the BCA? Some critics of the Stern Review suggest not. They believe that the author of a BCA should avoid imposing his or her own ethical values on policymakers. Rather, the author should input the best available data, crunch the numbers in the most neutral way possible, and present the unbiased results to those whose responsibility it is to make policy. Otherwise, the author of the BCA inappropriately usurps the role of policymaker. This is a sensible argument. But, as I suggested above, I do not think it is entirely possible for any author of any BCA to avoid imposing their values on the BCA in ways the affect the outcome simply because of the subjective judgments that each BCA inevitably entails. In other words, every author of every BCA is, to a greater or lesser extent, a would-be policymaker. This

is as true of Nordhaus and his 3% discount rate as it is of Stern and his 0.1% discount rate.

As for the "conventional range" of utility discount rates, it is true that the Stern Review's choice of a 0.1% pure rate of time preference is lower than the rates chosen in most other BCAs. Does this deviance render it illegitimate? Well, standard practices can, and sometimes do, become norms that govern decisions. But Portney and Weyant (1999) found no such norm governing decisions about discount rates for BCAs. And even if there were a generally accepted range of discount rates for BCAs, prominent economists have argued that climate change represents a special case – a long-run phenomenon subject to relatively great uncertainties, including uncertainty about the discount rate itself, which might lead to reductions in the growth rate of per capita consumption – requiring lower-than-usual discount rates (see Weitzman forthcoming 2007 and Dasgupta, Mäler, and Barrett 1999).

Taking a different tack, Tom Schelling (2006b) argues that discounting is not appropriate for assessing the costs and benefits of climate change in the first place because those who bear the costs are not the same people, generally speaking, who will receive the benefits. The costs of climate change, now and in the future, will fall disproportionately on developing countries; but the costs of mitigating GHG emissions to stabilize the climate and reduce the costs of climate change, under the Kyoto Protocol or any other reasonably conceivable program, will be borne predominantly by wealthier, developed countries. Thus, efforts to mitigate climate change are in the nature of a foreign aid program, like the Marshall Plan, NATO, or disease eradication campaigns in Africa. As such, discounting is inappropriate. Schelling (2006b, p. 52) writes that "[t]he alleged inborn preference for ealier rather than later consumption is exclusively concerned with the consumer's impatience with respect to his or her *own* consumption."[23] And Schelling (2006b, p. 53) goes on to note that

> decisions to invest in greenhouse gas-emissions abatement for the benefit of future generations are not "saving decisions" – not decisions about postponing one's own consumption – but are instead decisions about redistributing income, one's own income. To invest resources now in reduced greenhouse gas emissions

[23] Italics in original.

> is to transfer consumption from present-day people – whoever those people are who are making these sacrifices – for the benefit of people in the distant future. It is very much like making sacrifices now for people who are distant geographically or distant culturally.

If Schelling is right about this, then Stern (2006) and Cline (1992) were right to adopt low values of ρ. However, this does not mean that Schelling would necessarily concur in their conclusions or policy prescriptions.

A final response to the argument from convention is that the Stern Review's 0.1% pure rate of time preference is not unprecedented among climate change BCAs. The pioneering economic analysis of Cline (1992) adopted a pure rate of time preference of 0.0%. Taken together, the ethical arguments of Ramsey (1928) (among others), the conceptual arguments of Schelling (2006b), Portney and Weyant's (1999) evaluations of practice, and Cline's (1992) application counsel against a conclusion that there is a convention or social norm that the Stern Review violated in adopting a 0.1% pure rate of time preference.

Even if there were such a convention or social norm, its violation would not, by itself, make much difference to the overall BCA. As Weitzman (forthcoming 2007) demonstrates, it is not the pure rate of time preference (ρ) alone that is significant, but the combination of parameter values for ρ, η, and g that together determine r, the consumption rate of discount. The pure rate of time preference is only one element of a larger equation. And it is the product of that equation, *r,* which seems ultimately significant for BCA.

Three concerns about the Stern Review's discount rate remain, however, which relate to other arguable conventions, social norms or "best practices" of BCAs.

First, in selecting a pure rate of time preference the Stern Review summarily ignored HM Treasury's (2003) own *Green Book* of discount rates. Interestingly, that schedule closely tracks Nordhaus and Boyer's (2000) model, with a range of discount rates that decline from 3.5% (for costs and benefits between year 0 and year 30) to 1% (for costs and benefits arising after 300 years). Stern (2006, p. 47) makes only a single, oblique reference to the HM Treasury's *Green Book*, and does not provide anything like an adequate explanation as to why it chose to ignore the discount rates *mandated* by its own authorities. This should be deemed to violate a "best

practice" standard according to which agencies must follow their own rules, unless they provide a complete and transparent explanation justifying deviation. Not doing so generates at least two problems: (a) it creates the appearance (at least) that something fishy is going on in the BCA; and (b) it unjustifiably erodes the authority of government policymakers.

Second, whatever discount rate is chosen, it must be applied consistently to the future costs and benefits of mitigation, as well as the future costs and benefits of harm. According to Mendelsohn (2006-7), the Stern Review failed to discount the costs of mitigation. This is an obvious flaw, which the Stern group should correct in a supplemental BCA.

Third, over the last decade or so it has become common for government agencies, at least in the United States, to prepare BCAs that include a variety of calculations using various discount rates (see, e.g., EPA 1997). This practice has the benefit of presenting policymakers with more information and a clearer understanding of how *their* choice of parameter values effects expected valuations of future costs and benefits. The authors of the Stern Review might at least have conducted alternative analyses under HM Treasury's official schedule of declining discount rates and its own preferred parameter values. Given the inherently subjective nature of those values, I would support the establishment of a "best standard" requiring not just sensitivity analyses, but complete alternative sets of calculations using various parameter values.

The same effect can be obtained – and arguably is obtained in the case of climate change assessments – by the independent generation of multiple BCAs assessing the same social problems and policies using different parameter values. The Stern Review is not, after all, the only economic analysis we have of climate change. The fact that other BCAs of climate change exist – from Cline (1992) to Mendelsohn *et al.* (1998) and Nordhaus and Boyer (2000) – reduces the significance of the omissions, miscalculations, and idiosyncracies, of any one set of analyses, conclusions and recommendations. The various economic analyses, and critiques of them, are all out there for policymakers to learn from and use in what US Supreme Court has called "the marketplace of ideas."[24]

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[24] *Keyishian v. Board of Regents*, 385 U.S. 589 (1967). The phrase is often mistakenly attributed to Justice Oliver Wendell Holmes's dissent in the earlier case of *Abrams v. US*, 250 U.S. 616 (1919). While that dissent clearly articulated the same notion, using phrases like "free trade in ideas," he did not write the phrase "marketplace of ideas."

The Stern Review, as an official government document produced with the imprimatur of HM Treasury, arguably carries special weight. That can be the only reason for the extraordinary public and media attention its publication received, in stark contrast to earlier economic analyses of climate change produced privately by individual academics or small groups of scholars. A real risk exists that the Stern Review could influence public policy more than other economic analyses not because of its relative merits but because of its pedigree. This certainly presents a potential problem for producers and consumers of BCAs, but the implications for the theory and practice of BCA are unclear. Perhaps we can take solace in the fact that after the initial surge of public and media enthusiasm over the Stern Review, it has not inordinately influenced policy in Britain or elsewhere, but has taken its place alongside other economic analyses in the relative obscurity of academic discussions such as this.

What's a Human Life or a Functional Ecosystem Worth?

At the beginning of the last section, I purported to cabin concerns about the Stern Review's damage estimates, so as to focus on the choice of parameter values, which after all explain virtually the entire gap between the Stern Review's large damage estimate and the smaller damage estimates of earlier studies. However, there is one glaring omission from the Stern Review's damage assessment that seems a clear violation of the "best practice" standard that all assumptions in BCAs should be transparent.

The Review is much less clear than it could be about the anticipated mortality and morbidity effects of climate change under different scenarios. I cannot recall reading another BCA that was so opaque about its valuation assumptions for human lives and nonmarket goods. The overall damage estimate presumably includes human mortality and morbidity effects, as well as effects on other nonmarket goods. But nowhere in the Review could I find any explicit value placed on a human life lost as a result of climate change or saved as a result of mitigation or adaptation efforts.[25] Nor are dollar values specified for other nonmarket effects. Given the

[25] I looked hard more than once; perhaps I missed them. If so, they were not presented explicitly enough. The same is true for Hope (2006), which sets out the PAGE2002 model.

importance of those values for the damage estimate, their absence is perplexing. A reader of the Stern Review has no way of determining whether (a) the authors' valuations deviate significantly from the range of human life and other nonmarket valuations supported in the literature, and (b) the total damage estimates are calculated properly. Lacking valuations of nonmarket damages, no subsequent analyst could even attempt to replicate the Stern Review's damage assessment. It is a given that any good BCA should provide enough information for other analysts to replicate its results. In this respect, at least, the Stern Report is not a good BCA.

Finally, A Couple of Positive Implications of the Stern Review

The implications of the Stern Review for the theory and practice of BCA are not entirely negative. Arguably, it has made one or two highly valuable positive contributions to the field. For one, it has demonstrated the potential importance of paying attention not only to the mean of expected values of present and future costs and benefits but to low-probability, high-magnitude (potential "low-*g*") events at the tails of damage distribution curves. As Weitzman (forthcoming 2007) notes, events that are highly unlikely may nevertheless be so significant for future consumption growth rates that they simply must be accounted for in economic analyses. Nordhuas and Boyer (2002) paid some attention to potentially catastrophic climate-change events, but the Stern Review is the first to focus its entire BCA beyond the mean estimate of damages. This lesson may not have great importance for economic analyses of standard, run-of-the-mill government policies and projects, but for arguably special cases like climate change, which combine unusually long time-horizons with very high levels of Knightian uncertainty, worse-case (as well as worst-case) scenarios should be treated seriously.

A second positive lesson from the Stern Review for the practice of BCA is not to be found in the Review itself, but in Sir Nicholas Stern's willingness to engage his critics. As noted earlier, one of the major failings of the Stern Review was the absence of a sensitivity analysis to demonstrate the robustness of its conclusions across alternative parameter values. Stern responded by adding a sensitivity analysis to the Review as a postscript. Weitzman (forthcoming 2007, p. 7) may be right that the postscript was a "half-hearted" effort to placate the critics, but at least Stern did it (and, of course, it was the right thing to do). Stern has also made himself

available for conferences and meetings that have been more or less devoted to tearing apart his Review. For example, on February 15, 2007, Sir Nicholas appeared at a conference at Yale, where he confronted some of his staunchest critics, including William Nordhaus and Robert Mendelsohn. Not every author of every BCA would have been so brave or open to disputation. Stern's willingness to engage his critics remind us that economic analysis is an dynamic, iterative process, where the cycle of analysis and criticism leads to improved analyses and further criticisms. In other words, no BCA is an island. And all *ex ante* economic analyses are provisional. Stern (2006, p. 144) explicitly recognizes this fact in acknowledging that his economic model of climate change is merely "one contribution to [a] discussion."

Conclusion

The Stern Review and its serious academic critics should all be read by anyone interested in benefit-cost analysis as a discipline. They raise serious issues about the practice and practicability of BCA for long-run policies (including BAU) with potentially severe consequences under high levels of Knightian uncertainty. Stern (2006) takes a strong position that such uncertainty is not an excuse for inaction, especially where the stakes are enormous, as is arguably the case with climate change. Despite numerous flaws in his analysis, I tend to agree with him about that most important point. Stern's (2006) postscript provides something of a concise summation of this entire meta-review of the Review and its critics: "We must be transparent and clear. If you take little account of the interests of future generations you will care little about climate change. But ethical positions cannot be dictated by policy analysts...."

If only Stern himself had taken these words more to heart, reviewers like me would be spending more time celebrating the Stern Review instead of criticizing it.

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