Bjørn Lomborg’s comments to the 11-page critique in January 2002 *Scientific American (SA)*, (in black)

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[Background:
Recently I have received – through informal channels – the final proofs of an 11 page feature in Scientific American, all of it devoted to a throughout trashing of my recent book *The Skeptical Environmentalist*, Cambridge University Press 2001 (referred as SE in references).

As I write these words Scientific American has as yet not given me a chance to put my side of the argument before their readers. The four critiques and accompanying editorial will be the only statement that readers of SA will receive as the basis on which to judge the cogency of my arguments.

I have had some half-promises that I might get to state my side of the argument in Scientific American, but there is no firm promises with respect to either format or date.

I shall now present a preliminary critique of the feature, article for article, point for point. References to various works are, unless otherwise noted, to the same sources as used in SE. The full bibliography can be downloaded at www.lomborg.org.]

Scientific American, p61-71, January 2002, (in red).
The text comes from the final draft and has been transferred from pdf into Word, meaning that occasionally italics or words may have been dropped. Most of the layout has been retained in headings, subheadings and usage of capital lettering. The first page (p61) is an editorial by editor-in-chief, John Rennie, the other ten pages flow in three columns into each other, with a sentence on each page in very large font for interest. These sentences will be pointed out below, but may come from an editorial decision. On the web, Scientific American describes the collection of essays thus:

Misleading Math about the Earth
ESSAYS BY STEPHEN SCHNEIDER, JOHN P. HOLDREN, JOHN BONGAARTS AND THOMAS LOVEJOY

The book The Skeptical Environmentalist uses statistics to dismiss warnings about peril for the planet. But the science suggests that it's the author who is out of touch with the facts.
(http://www.sciam.com/page.cfm?section=currentissue).

Science defends itself against The Skeptical Environmentalist

This statement is potentially the most surprising of all – that the following critique should be *science defending itself against my book*. In a sense this encapsulates the bias of the following critiques. My book clearly makes a claim to science and to be factually based. I openly state the facts and my sources, and thus anybody is free to point out where these are faulty or incorrect and of course, such errors will then be posted on my web site.

Thus, there is no need to defend science from my book – any possible defeat of science was never the issue. The discussion is whether the statements in my book are correct or not. The need to make it sound like a battle of science *against* my book seems entirely to misplace and bias the focus. Rather, the standpoint that might need to defend itself from my book would be the alarmist environmentalism, and that is perhaps the headline that would make more sense: Alarmist environmentalism defends itself against the Skeptical Environmentalist.

MISLEADING MATH about the EARTH

CRITICAL thinking and hard data are the cornerstones of all good science. Because environmental sciences are so keenly important to both our biological and economic survival—causes that are often seen to be in conflict—they deserve full scrutiny. With that in mind, the book The Skeptical Environmentalist (Cambridge University Press), by Bjørn Lomborg, a statistician and political scientist at the University of Aarhus in Denmark, should be a welcome audit. And yet it isn't.

As its subtitle—Measuring the Real State of the World— indicates, Lomborg's intention was to reanalyze environmental data so that the public might make policy decisions based on the truest understanding of what science has determined. His conclusion, which he writes surprised even him, was that contrary to the gloomy predictions of degradation he calls "the litany," everything is getting better. Not that all is rosy, but the future for the environment is less dire than is supposed. Instead Lomborg accuses a pessimistic and dishonest cabal of environmental groups, institutions and the media of distorting scientists' actual findings. (A copy of the book's first chapter can be found at www.lomborg.org)

The problem with Lomborg's conclusion is that the scientists themselves disavow it. Many spoke to us at SCIENTIFIC AMERICAN about their frustration at what they described as Lomborg's misrepresentation of their fields. His seemingly dispassionate outsider's view, they told us, is often marred by an incomplete use of the data or a misunderstanding of the underlying science. Even where his statistical analyses are valid, his interpretations are frequently off the mark—literally not seeing the state of the forests for the number of the trees, for example. And it is hard not to be struck by Lomborg's presumption that he has seen into the heart of the science more faithfully than have investigators who have devoted their lives to it; it is equally curious that he finds the same contrarian good news lurking in every diverse area of environmental science.

Making it sound like *all* scientists disavow it is simply untrue. Many scientists, both in private and publicly (e.g. statements on the book) have praised the book. Below, you will see that none of the claims of "misrepresentation", "incomplete use of data" and "misunderstanding of the underlying science" are substantiated.

The only specific claim presented here by the editor is that I am "literally not seeing the state of the forests for the number of the trees." This can only refer to the one paragraph on forests by Lovejoy (the only treatment of the matter in the following text) – and here the analysis is quite clear. I try to show that environmental movements will tell us we are at risk of loosing "the last remaining forests on earth" and that our time is "the eleventh hour for the world's forests" (WWF, quoted in SE:110). Yet, the longest data series actually tells us of very little change in the world forested area in the post-war period (SE:111). Moreover, the longest future scenarios from the UN climate panel (IPCC) show that in all likelihood the Earth will have even greater forest cover in 2100 than it has had since 1950 (IPCC 2000b, SE:283). Here, exactly looking coolly at the longest data series gives us much better information than just going with the environmental myths and hype. Thus, in the editor's only concrete claim, he seems to be wide off the mark.

Pointing out that it seems questionable that I should know better than the people who've devoted their lives to particular areas, though clearly circumstantial, nevertheless looks like a powerful point. Yet, any person who has devoted his or her life to a single issue will naturally come to consider this area one of the most crucial issues, and any problem inside the area will likely be seen as necessary to solve.

And this is exactly my point – we should take the *science* of these people seriously, but we should not uncritically adopt their *evaluation* of the problems. There are a multitude of problems in any area of society – there are always things we would like to improve – but we only have a limited amount of resources. Thus, as a society we need to ask, whether the problems are getting bigger or smaller (are we going in the right direction), what can we do (much or marginal) and would this be the best use of our resources (other areas where we could do even more good). Such an appraisal does not come automatically from any single issue area. *This* is why we need to look, not only at the science of each area, but also to ask: 'so, all in all, how important a problem is your issue in the big scheme of things.' This is what I have attempted to do with *The Skeptical Environmentalist*.

We asked four leading experts to critique Lomborg's treatments of their areas—global warming, energy, population and biodiversity—so readers could understand why the book provokes so much disagreement. Lomborg's assessment that conditions on earth are generally improving for human welfare may hold some truth. The errors described here, however, show that in its purpose of describing the real state of the world, the book is a failure.

John Rennie, EDITOR IN CHIEF

Notice that these four experts have certainly not been chosen randomly – two of the four reviewers are actually directly criticized in my book. Lovejoy predicted back in 1980, that 15-20 percent of all species on earth would have died by the year 2000 (1980:331, SE:252), a prediction which clearly did not hold true and this is pointed out in the book. Holdren back in 1980 also clearly thought that many resources were running out. Along with Ehrlich and Holdren, he bet on this belief with Julian Simon:

> "Frustrated with the incessant claims that the Earth would run out of oil, food and raw materials, the economist Julian Simon in 1980 challenged the established beliefs with a bet. He offered to bet $10,000 that any given raw material – to be picked by his opponents – would have dropped in price at least one year later. The environmentalists Ehrlich, Harte and Holdren, all of Stanford University, accepted the challenge, stating that "the lure of easy money can be irresistible." The environmentalists staked their bets on chromium, copper, nickel, tin and tungsten, and they picked a time frame of ten years. The bet was to be determined ten years later, assessing whether the real (inflation-adjusted) prices had gone up or down. In September 1990 not only had the total basket of raw materials but also each individual raw material dropped in price. Chromium had dropped 5 percent, tin a whopping 74 percent. The doomsayers had lost.
>
> Truth is they could not have won. Ehrlich and Co. would have lost no matter whether they had staked their money on petroleum, foodstuffs, sugar, coffee, cotton, wool, minerals or phosphates. They had all become cheaper." (SE:137).

Since 1990 the price of raw materials has declined another third (*Economist* industrial price index, SE:138).

The editor claims that the experts are chosen to show why the book is causing so much "disagreement," but given the choice of four experts who clearly feel the book is fundamentally wrong, it is unclear how the reader should be able to understand that there might be any value to my argument, and thus to the disagreement. The obvious lack of any concern for presenting a balanced review of my work calls into question the real purpose of this Scientific American feature. However, one of its contributors, Stephen Schneider, has on a former occasion made a suggestion that might throw some light on the curious imbalance of the Feature under consideration.

Schneider considers the "ethical double bind" that might occur to the scientist who is also concerned to contribute to a better world. As a scientist he focuses on truth. As a concerned citizen he must take an interest in political efficiency. Quite obviously, Schneider finds that this presents a delicate dilemma and he expresses the hope that one might be both honest and effective. However, as Schneider agonizes over this dilemma he does offer the following bit of unambiguous advice "So we have to offer up scary scenarios, make simplified, dramatic statements, and make little mention of any doubts we might have."[1] Could John Rennie have taken this as editorial advice? I don't know, but I feel that it would account for the tone and the lack of balance of the Feature considered as a whole. Unfortunately, this tone and lack of balance also seem to represent a disappointing and painful abandonment of the long proud tradition of enlightenment and rationality for which Scientific American has been respected in the past.

Finally, John Rennie tells us that, yes – Lomborg's fundamental assertion may hold "some truth," and yet, in the very next statement that the book is "a failure." This could seem like somewhat of a glaring contradiction and at least it relies heavily on the ability of the ensuing reviews to establish fundamental and serious errors in the argument – something they never manage to do.

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[1] "On the one hand, as scientists we are ethically bound to the scientific method, in effect promising to tell the truth, the whole truth, and nothing but - which means that we must include all the doubts, the caveats, the ifs, ands, and buts. On the other hand, we are not just scientists but human beings as well. And like most people we'd like to see the world a better place, which in this context translates into our working to reduce the risk of potentially disastrous climatic change. To do that we need to get some broadbased support, to capture the public's imagination. That, of course, entails getting loads of media coverage. So we have to offer up scary scenarios, make simplified, dramatic statements, and make little mention of any doubts we might have. This 'double ethical bind' we frequently find ourselves in cannot be solved by any formula. Each of us has to decide what the right balance is between being effective and being honest. I hope that means being both." (Quoted in Discover, pp. 45-48, Oct. 1989, see also American Physical Society, APS News August/September 1996, http://cyclotron.aps.org/apsnews/0896/11592.html).

Stephen Schneider

GLOBAL WARMING: NEGLECTING THE COMPLEXITIES

For three decades, I have been debating alternative solutions for sustainable development with thousands of fellow scientists and policy analysts—exchanges carried out in myriad articles and formal meetings. Despite all that, I readily confess a lingering frustration: uncertainties so infuse the issue of climate change that it is still impossible to rule out either mild or catastrophic outcomes, let alone provide confident probabilities for all the claims and counterclaims made about environmental problems.

Even the most credible international assessment body, the Intergovernmental Panel on Climate Change (IPCC), has refused to attempt subjective probabilistic estimates of future temperatures. This has forced politicians to make their own guesses about the likelihood of various degrees of global warming. Will temperatures in 2100 increase by 1.4 degrees Celsius or by 5.8? The difference means relatively adaptable changes or very damaging ones.

Against this background of frustration, I began increasingly to hear that a young Danish statistician in a political science department, Bjørn Lomborg, had applied his skills in statistics to better determine how serious environmental problems are. Of course, I was anxious to see this highly publicized contribution— The Skeptical Environmentalist: Measuring the Real State of the World. A "skeptical environmentalist" is certainly the best kind, I mused, because uncertainties are so endemic in these complex problems that suffer from missing data, incomplete theory and nonlinear interactions. But the "real state of the world"—that is a high bar to set, given the large range of plausible outcomes.

And who is Lomborg, I wondered, and why haven't I come across him at any of the meetings where the usual suspects debate costs, benefits, extinction rates, carrying capacity or cloud feedback? I couldn't recall reading any scientific or policy contributions from him either. But there was this massive 515 page tome with a whopping 2,930 endnotes to wade through. On page xx of his preface, Lomborg admits, "I am not myself an expert as regards environmental problems"—truer words are not found in the rest of the book, as I'll soon illustrate. I will report primarily on the thick global warming chapter and its 600plus endnotes. That kind of deadweight of detail alone conjures at least the trappings of comprehensive and careful scholarship. So how does the reality of the text hold up to the pretense? I'm sure you can already guess, but let me give some examples to make clear what I learned by reading.

These paragraphs do not really discuss my book but establish several important rhetorical points that need to be mentioned. First is the John Rennie's incantation of "investigators who have devoted their lives" to the science: Schneider is the venerable scientist whereas I'm a nobody.

Second, he quotes my introduction where I state I'm not an expert as regards environmental problems. True, but the quote in full actually places this point in context:

> "I have let experts review the chapters of this book, but I am not myself an expert as regards environmental problems. My aim has rather been to give a description of the approaches to the problems, as the experts themselves have presented them in relevant books and journals, and to examine the different subject-areas from such a perspective as allows us to evaluate their importance in the overall social prioritization.
>
> The key idea is that we ought not to let the environmental organizations, business lobbyists or the media be alone in presenting truths and priorities. Rather, we should strive for a careful democratic check on the environmental debate, by knowing the real state of the world – having knowledge of the most important facts and connections in the essential areas of our world. It is my hope that this book will contribute to such an understanding." (pxx)

Of course, saying that truer words are not found in the rest of the book is clearly a rhetoric point, as much of what I say is simply quotes of the best available statistics from the official entities like the UN, OECD, World Bank, EU, US etc.

Third, Schneider lets us consider the argument that my many endnotes conjure at least trappings of academic argument. This seems an unreasonable critique, since it really makes it a lot easier for my critics to attempt to show exactly where I might be wrong. The argument also easily backfires, since Schneider does not supply any endnotes or other trappings of academic argument himself. Of course, Scientific American has limited space, but one could easily have imagined that SA would have put out an annotated version of the papers on their website. (That Schneider considers his SA article his best argument is evident from his other, shorter and fiercer article from http://www.gristmagazine.com/grist/books/schneider121201.asp, where he specifically refers to his SA piece and the Pimm & Harvey Nature article for documentation. Incidentally, the Nature article is also almost devoid of documentation, see download on my web-site, www.lomborg.org.)

> The climate chapter makes four basic arguments:
>
> **Climate science** is very uncertain, but nonetheless the real state of the science is that the sensitivity of the climate to carbon dioxide will turn out to be at the low end of the IPCC uncertainty range—which is for a warming of 1.5 to 4.5 degrees C if carbon dioxide were to double and be held fixed over time.
>
> **Emissions scenarios**, according to the IPCC, fall into six "equally sound" alternative paths. These paths span a doubling in carbon dioxide concentrations in 2100 up to more than tripling and well beyond tripling in the 22nd century. Lomborg, however, dismisses all but the lowest of the scenarios: "Temperatures will increase much less than the maximum estimates from IPCC—it is likely that the temperature will be at or below the B1 estimate [the lowest emissions scenario] (less than 2° C in 2100) and the temperature will certainly not increase even further into the twenty-second century."
>
> **Cost-benefit calculations** show that although the benefits of avoiding climate change could be substantial ($5 trillion is the single figure Lomborg cites), this is not worth the cost to the economy of trying to constrain fossil fuel emissions (a $3trillion to $33trillion range he pulls from the economics literature). Asymmetrically, no range is given for the climate damages.
>
> **The Kyoto Protocol**, which caps industrialized countries' output of greenhouse gases, is too expensive. It would reduce warming in 2100 by only a few tenths of a degree—"putting off the temperature increase just six years." This number, though, is based on a straw-man policy that nobody has seriously proposed: Lomborg extrapolates the Kyoto Protocol, which is applicable only up to 2012, as the world's sole climate policy for another nine decades.

Schneider deserves credit for making clear the main thrust of his criticism in these four points, though he clearly cannot bear just to state them without pejorative statements like "asymmetrically, no range…" and "straw-man policy", both of which are incorrect and will be dealt with below.

> Before providing specifics of why I believe each of assertions is fatally flawed, I should say something about Lomborg's methods. First, most of his nearly 3,000 citations are to secondary literature and media articles. Moreover, even when cited, the peer-reviewed articles come elliptically from those studies that support his rosy view that only the low end of the uncertainty ranges will be plausible. IPCC authors, in contrast, were subjected to three rounds of review by hundreds of outside experts. They didn't have the luxury of reporting primarily from the part of the community that agrees with their individual views.

There is an important distinction between secondary sources and media articles. When discussing the entire state of the world, it would be incredibly inefficient not to use the vast collection of data and theory offered by secondary sources – this is exactly the reason for secondary literature and in general why it is possible to have specialization in science. However, almost all of these secondary sources are exactly the ones used by almost all discussants of the state of the world – the reports of the UN, (FAO, UNDP, UNEP, WHO etc.), IMF, the World Bank, OECD, WRI, Worldwatch Institute, EU, US government agencies etc. In the climate chapter, which Schneider discusses, references to the IPCC reports constitute about one-third of all 646 endnotes. Yet, the IPCC reports are clearly secondary sources. Surely, most people – including myself – would consider these reports the best available summary of our understanding of the climate science, which exactly was my argument for primarily using them as references:

> "In the following I shall – unless otherwise stated – use the figures and computer models from the official reports of the UN climate panel, the IPCC. The IPCC's reports are the

foundation for most public policy on climate change and the basis for most of the arguments put forth by the environmental organizations." (SE:259).

When I use media articles this is almost always when analyzing media discussion and illustrating what I believe to be a bias towards bad news or even incorrect information that permeates environmental news reporting. When discussing the IPCC temperature interval for 2100 of 1.4-5.8°C, I point out that:

> "In the reporting from the major media, such as CNN, CBS, The Times, and Time, it was found that all used the high estimate of 5.8°C warming, and yet none mentioned the low estimate of 1.4°C."

Naturally, this statement uses media articles as reference but is the use problematic? Should such a bias not be pointed out?

Likewise, I debunk *U.S. News & World Report* for telling its readers in February 2001 of how global warming would have lots of serious consequences. One of the most outrageous would be the US prediction: "By midcentury, the chic Art Deco hotels that now line Miami's South Beach could stand waterlogged and abandoned," despite IPCC estimates of a water rise of just 16cm (6in) by 2050 (SE:289-91). Is this use of media sources unreasonable?

Then, the critique of my use of sources continues with the charge that when I use peer-reviewed articles I do so primarily to support my rosy view of a low range but no further evidence of this is offered.

> Second, it is ironic that in a popular book by a statistician one can't find a clear discussion of the distinction among different types of probabilities, such as frequentist and Bayesian (that is, "objective" and "subjective"). He uses the word "plausible" often, but, curiously for a statistician, he never attaches any probability to what is "plausible." The Third Assessment Report of the IPCC, on the other hand, explicitly confronted the need to quantify all confidence terms. Working Group I, for example, gave the term "likely" a 66 to 90 percent chance of occurring. Although the IPCC gives a wide range for most of its projections, Lomborg generally dismisses these ranges, focusing on the least serious outcomes. Not so much as one probability is offered for the chance of a dangerous outcome, yet he makes a firm assertion that climate "will certainly" not go beyond 2 degrees C warming in the 22nd century—a conclusion at variance with the IPCC, other national climate assessments and most recent studies in the field of climate science.

It is correct that IPCC has quantified its 'plausible', but IPCC themselves quite rightly made it clear what the limits were on the accuracy of their different types of probability: "the following words have been used where appropriate to indicate *judgmental estimates of confidence*: virtually certain (greater than 99% chance that a result is true); very likely (90-99% chance); likely (66-90% chance); medium likelihood (33-66% chance); unlikely (10-33% chance); very unlikely (1-10% chance); exceptionally unlikely (less than 1% chance)," (IPCC 2001d:2, italics added). Unless we are talking about events with very well-established probability distributions (which is the case for almost none of the important global warming issues) it really is just a *judgement* call whether something has a 89% or 91% chance of occurring – thus, had I made a similar endnote, defining the words of confidence, it might have *appeared* slightly more objective but not really made any addition to the facts at hand.

The second claim is much more serious: that I *generally* dismiss the IPCC ranges and focus on the least serious outcomes. Neglecting such ranges generally or without reason would, of course, be seriously misleading, which is why I don't do it in the book and which may explain why my critic offers no examples. Take two of the most important characteristics of global warming, sea level increases and temperature impacts on agriculture. For sea level increase I clearly write out the ranges from the main scenarios (SE:264) and for agriculture impact I clearly state the IPCC ranges (SE:288).

Next, it is claimed that I do not offer any probability of a dangerous outcome. This is plainly incorrect. In a whole section entitled "Fear of catastrophe" (SE: 315-7) I discuss the two major worries of dangerous outcomes, the sliding of the West Antarctic Ice Sheet (WAIS) and the shut-down of the thermohaline circulation (THC) that drives the Gulf Stream. Here I quote the 2001 IPCC report that a WAIS breakup is considered "very unlikely during the 21st century" (SE:315). Likewise, with respect to the THC, I write out that the 2001 "IPCC conclude that 'the current projections using climate models do not exhibit a complete shut down of the thermohaline circulation by 2100' but point out that it could completely, and possibly irreversibly, shut down 'if the change in radiative forcing is large enough and applied long enough'" (SE:316). In the endnote it is discussed how likely it is that the radiative forcing will be large enough and applied long enough for this shut-down to happen. Thus, for both the major dangerous outcomes I discuss the probability in detail, contrary to Schneider's claim.

The final quote of "will certainly" only works because it has been taken out of context:

> "This more realistic model holds several key points. First, it shows that global warming is not an ever worsening problem. In fact, under any reasonable scenario of technological change and without policy intervention, carbon emissions will not reach the levels of A1FI and they will decline towards the end of the century, as we move towards ever cheaper renewable energy sources. Second, temperatures will increase much less than the maximum estimates from IPCC – it is likely that the temperature will be at or below the B1 estimate (less than 2°C in 2100) and the temperature *will certainly* not increase even further into the twenty-second century. Third, …" (SE:286, italics added).

The quote "will certainly" comes from a model which is deemed "more realistic," but it is naturally only within this model that I can say that the temperature will be below 2°C and not keep increasing into the 22nd century. To make me say otherwise (that I should make "a firm assertion") is simply called misquoting.

> Now let us look in more detail at the four major arguments he makes in this chapter.
>
> **Climate science**. A typical example of Lomborg's method is his paraphrase of a secondary source in reporting a 1989 Hadley Center paper in the journal Nature in which the researchers make modifications to their climate model: "The programmers then improved the cloud parameterizations in two places, and the model reacted by reducing its temperature estimate from 5.2° C to 1.9° C." Had this been first-rate scholarship, Lomborg would have consulted the original article, in which the concluding sentence of the first paragraph presents the authors' caveat: "Note that although the revised cloud scheme is more detailed it is not necessarily more accurate than the less sophisticated scheme." In a similar vein, he cites Richard S. Lindzen's controversial stabilizing feedback, or "iris effect," as evidence that the IPCC climate sensitivity range should be reduced by a factor of almost three. He fails either to understand this mechanism or to tell us that it is based on only a few years of data in a small part of one ocean. Extrapolating this small sample of data to the entire globe is like extrapolating the strong destabilizing feedback over midcontinental landmasses as snow melts during the spring—such an inappropriate projection would likely increase estimates of climate sensitivity by a factor of several.

I am glad to have pointed out the typical way I refer to secondary sources – namely quote them accurately. The quote comes from *Science* magazine in 1997:

> "A few years ago, a leading climate model – developed at the British Meteorological Office's Hadley Center for Climate Prediction and Research, in Bracknell – predicted that an Earth with twice the preindustrial level of carbon dioxide would warm by a devastating 5.2 Degrees Celsius. Then Hadley Center modelers, led by John Mitchell, made two improvements to the model's clouds--how fast precipitation fell out of different cloud types and how sunlight and radiant heat interacted with clouds. The model's response to a carbon dioxide doubling dropped from 5.2 Celsius to a more modest 1.9 Celsius." (Kerr 1997a:1040).

However, the claim that I should have gone back to the original article seems suspect on several grounds. First, why would a major *Science* overview article not be a trustworthy source in general (and why not mention that the source is *Science*, rather than merely "a secondary source")? Second, it is of course possible that there are errors in secondary sources, though the risk is probably very small when using reputable sources like *Science*. The necessary question, though, is whether this is an important error? And if so, why has nobody (including my critic) corrected the article when it appeared in *Science*? Finally, is it really correct that the only relevant article to go back to is an article from 1989 (eight years earlier), where they point out the more detailed cloud scheme is "not necessarily more accurate"? Naturally, much research has been done since 1989 to establish which cloud scheme is the more accurate; in a 1993 article Michell points out (together with C. A. Senior):

> "The importance of the representation of cloud in a general circulation model is investigated by utilizing four different parameterization schemes for layer cloud in a low-resolution version of the general circulation model at the Hadley Centre for Climate Prediction and Research at the United Kingdom Meteorological Office. The performance of each version of the model in terms of cloud and radiation is assessed in relation to satellite data from the Earth Radiation Budget Experiment (ERBE). *Schemes that include a prognostic cloud water variable show some improvement on those with relative humidity-dependent cloud*, but all still

show marked differences from the ERBE data. The sensitivity of each of the versions of the model to a doubling of atmospheric $C0_2$ is investigated. Midlevel and lower-level clouds decrease when cloud is dependent on relative humidity, and this constitutes a strong positive feedback. When interactive cloud water is included, however, this effect is almost entirely compensated for by a negative feedback from the change of phase of cloud water from ice to water. Additional negative feedbacks are found when interactive radiative properties of cloud are included and these lead to an overall negative cloud feedback. *The global warming produced with the four models then ranges from 5.4° with a relative humidity scheme to 1.9°C with interactive cloud water and radiative properties.* Improving the treatment of ice cloud based on observations increases the model's sensitivity slightly to 2.1°C. Using an energy balance model, it is estimated that the climate sensitivity using the relative humidity scheme along with the negative feedback from cloud radiative properties would be 2.8°C. Thus, 2.8°–2.1°C appears to be a better estimate of the range of equilibrium response to a doubling of $C0_2$." (Senior & Mitchell 1993, http://ams.allenpress.com/amsonline/?request=get-abstract&issn=1520-0442&volume=006&issue=03&page=0393, italics added).

Here, they basically tell us that the model which produced the 1.9°C *is* better though not necessarily by a lot ("some improvement") and that the low-end estimates are "better estimates."

Thus, the example of secondary source quotation seems curious at best or deliberately misleading at worst.

The claim against Lindzen seems unreasonable as pointed out in Lindzen's own letter to *Scientific American*, available at my web-site. Here Lindzen writes:

"One small point of personal interest to me illustrates the rather bizarre nature of these attacks. Schneider claims that Lomborg cites a paper by me and colleagues (Lindzen, Chou and Hou, Does the Earth Have an Adaptive Infrared Iris?, *Bulletin of the American Meteorological Society*, 82, 2001) on what we refer to as the 'iris effect' in order to reduce the IPCC claimed sensitivities by a factor of 3. What Lomborg does, is devote a quarter of a page to our paper in order to point out that it 'might pose a challenge' to the IPCC range. Schneider goes on to chide Lomborg for failing to present an allegedly fatal flaw in our argument: that it is simply the extrapolation of data from "a few years of data from a small part of one ocean." He also presents an absurdly incomprehensible 'analogy' to positive feedbacks from midcontinental ice melts in spring. What Schneider really illustrates is that he completely misunderstands what we have done, which is to assess the effect of temperature on the behavior of cumulonimbus convection and its impact on large scale upper level cirrus clouds in the tropics. The primary requirement of such a study is that it deal with a period and a region which contain a large enough number of cumulonimbus towers; the results (which are normalized by a measure of cumulus activity) are then scalable to the entire tropics – a far cry from naive extrapolation . The period we dealt with (20 months in the paper, but now extended to 4 years) and the area looked at (30 o S-30 o N, and 130 o E-170 o W) amply satisfied this criterion. As a logical test of this, we showed that the dependence of the ratio of cirrus area to convective activity remained robust even when we restricted ourselves to arbitrary small subsets in time and space of our full data set. We have also ascertained that existing climate GCMs fail to replicate the observations. As our paper amply stresses (and as Lomborg acknowledges), there remain uncertainties in our work, but Schneider's concern over 'extrapolation' is not one of them.

Thus, at one fell swoop, Schneider misrepresents both the book he is attacking and the science that he is allegedly representing."

As a final example, he quotes a controversial hypothesis from Danish cloud physicists that solar magnetic events modulate cosmic rays and produce "a clear connection between global low-level cloud cover and incoming cosmic radiation." The Danish researchers use this hypothesis to support an alternative to carbon dioxide for explaining recent climate change. Lomborg fails to discuss— and I haven't seen it treated by the authors of that speculative theory either— what such purported changes to this cloud cover have done to the radiative balance of the earth. Increasing clouds, it has been well known since papers by Syukuro Manabe and Richard T. Wetherald in 1967 and myself in 1972, can warm or cool the atmosphere depending on the height of the cloud tops, the reflectivity of the underlying surface, the season and the latitude. The reason the IPCC discounts this theory is that its advocates have not demonstrated any radiative forcing sufficient to match that of much more parsimonious theories, such as anthropogenic forcing.

Schneider calls this theory "an alternative to carbon dioxide for explaining recent climate change." However, neither the Danish cloud physicists nor I say that they are an alternative, but a supplementary explanation: "the sun as *another* important factor in the explication of increasing global temperatures" (SE:276, italics added). Moreover, I do point out both its still unsolved scientific problems but also its force and an attempt to show the relative importance of the two:

> "A number of unanswered questions and unsolved scientific problems still remain in these theoretical relationships. But the point is that the sunspot theory has created a possible correlation in that a shorter sunspot cycle duration, such as the one we are experiencing now, means more intense solar activity, less cosmic radiation, fewer low-level clouds, and therefore higher temperatures. This theory also has the tremendous advantage, compared to the greenhouse theory, that it can explain the temperature changes from 1860 to 1950, which the rest of the climate scientists with a shrug of the shoulders have accredited to "natural variation."
>
> Notice that the connection between temperature and the sunspot cycle seems to have deteriorated during the last 10-30 years, with temperatures outpacing sunspot activity in Figure 165. Most likely we are instead seeing an increasing signal, probably from greenhouse gases like CO2. *Such a find exactly underscores that neither solar variation nor greenhouse gases can alone explain the entire temperature record*. Rather, the fact that the emerging greenhouse gas signal only appears now seems to indicate once again that the estimated CO2 warming effect needs to be lowered. One such critical study finds that the solar hypothesis explains about 57 percent of the temperature deviations and that the data suggest a climate sensitivity of 1.7°C, a 33 percent reduction of the IPPC best estimate" (SE:277-8, italics added).

In conclusion, I do not accept the charge of having misconstrued climate science. If I am so wrong, one would expect that my critic should have had an easy time showing it, not having to rely on nitpicking, quoting out of context, and misrepresenting.

> **Emissions scenarios**. Lomborg asserts that over the next several decades new, improved solar machines and other renewable technologies will crowd fossil fuels off the market. This will be done so efficiently that the IPCC scenarios vastly overestimate the chance for major increases in carbon dioxide. How I wish this would turn out to be true! But wishes aren't analysis. One study is cited; ignored is the huge body of economics work he later accepts to estimate a range of costs if we were to implement emissions controls. In fact, most of these economists strongly believe high emissions are quite likely: they usually project carbon dioxide doubling to tripling (or more) as "optimal" economic policy. I have attacked this literature for failing to point out that climate policies that raise the price of conventional fuels spur investments in alternative energy systems. But such incentives need policies first—and Lomborg opposes those very policies. No credible analyst can just assert that a fossil fuel intensive scenario is not plausible—and, typically, he gives no probability that it might occur.

This is perhaps the most curious and weak argument of Schneider. He does nothing to confront my critique of the new IPCC scenarios, which in the words of one of the modeling teams are "an attempt at 'computer-aided storytelling.'" Here, IPCC has abandoned any attempt to predict the future and instead only talk about different possible futures. However, if the stories generating the worst outcomes are consistently unlikely, then clearly there is a great risk that we might end up spending vast amount of resources to combat threats that only occur in very unlikely storytelling.

I point out how the price of renewables such as solar power have been dropping by more than 50% per decade over the past 30 years. Then I present a peer-reviewed model (Chakravorty et al. 1997), which shows that if this trend continues it will mean the beginning of renewables as a substantial source of energy by 2025 and the end of fossil fuels by 2065. Even if a much lower price decrease of 30% per decade is assumed for the future, it means phasing in renewables by 2035 and the end of fossil fuels by 2105 (SE:284ff). Schneider merely counters this by pointing to the many models of the cost and benefits of global warming (the integrated models), which do not show this decline in carbon emissions. This is correct but entirely irrelevant – these models deal with different issues of costs and benefits (primarily with the timing of costs from early phasing out of carbon emissions), and the Chakravorty paper was exactly written to counter this problem.

Thus, my critic does not really have a counter-argument – he only seems to dismiss the analysis as wishful thinking and stating that "no credible analyst" can say this. These are arguments of authority, not science. If Schneider is aware of any other study that has looked at the relative costs of renewables and fossil fuels over time, taking into account the remarkable increase in efficiency of the renewables over the past decades, but which shows that renewables will not take over – we should be given these references and more details.

> **Cost-benefit calculations**. Lomborg's most egregious distortions and poorest analyses are his citations of cost-benefit calculations. First, he chides the governments that modified the penultimate draft of the report from IPCC's Working Group II. These modifications downgraded the significance of economic studies that aggregate climate change damages. Lomborg says: "A political decision stopped IPCC from looking at the total cost-benefit of global warming." (As an aside, I should mention that it is strange he chose to cite the penultimate and pre-approval draft report in this case but didn't mention the very first item in the approved summary—that recent temperature trends have caused a discernible effect on plants and animals. Even more puzzling is his failure to discuss ecological impacts in general, focusing instead on health and agriculture, sectors he thinks won't be much harmed by climate change of the minuscule amount he predicts.)

Here, two different arguments are being confused. Yes, I chide governments for editing the WGII technical summary, which stated the controversial but fairly well established point: Moderate global warming will have grave, negative net impact on the developing world but zero or maybe even a positive net impact for the developed world. In the more technical language of the WGII summary, this was:

> "Published estimates indicate that increases in global mean temperature would produce net economic losses in many developing countries for all magnitudes of warming studied, and that the losses would be greater in magnitude the higher the level of warming. In many developed countries, net economic gains are projected for global mean temperature increases up to roughly 2°C. Mixed or neutral net effects are projected in developed countries for temperature increases in the approximate range of 2 to 3°C, and net losses for larger temperature increases. The projected distribution of economic impacts is such that it would increase the disparity in well being between developed countries and developing countries, with the disparity growing with higher temperatures. The more damaging impacts estimated for developing countries reflects, in part, their lesser adaptive capacity." (IPCC 2001b:Summary for Policymakers, original government draft, 2.6., SE:301)

In the final version, this clear message disappeared without any additional scientific information being supplied. It certainly seems reasonable to criticize such a move. Schneider suggests that this should be due to a downgrading of economic studies, and he then mentions my following quote "a political decision…". However, this is about WGIII – a totally different issue. (That this is not just an accidental typo is seen in Schneider's Grist article, where he also talks about WG2).

Thus, Schneider makes no argument against the fact that governments changed the unpleasant conclusion.

Instead he makes the almost incomprehensible aside that it is strange that I mention one thing from the penultimate report but not a completely different thing from the finished report. Such argument just does not make sense – I mention the one thing because it is important in the context of the issues discussed; Schneider may find that I should have talked about others, and in a book dealing with so many and such varied issues, such a critique if of course always possible. However, dealing with the impact of global warming on agriculture and health rather than ecology seems entirely justified given the much greater attention, both scientifically and media-wise, to the first two issues, and also the human-centered evaluation presented openly in the beginning of my book (SE:11-2).

> The government representatives downgraded aggregate cost-benefit studies for a reason: these studies fail to consider so many categories of damages held to be important by political leaders as to render them just a guideline on market sector transactions, not the "total cost-benefit" analysis Lomborg wants. A total analysis would have to include the value of species lost, crucial ecosystem services degraded, inequity created by the poor being hurt more than the rich (which Lomborg does acknowledge), quality of life reduced (for example, a rise in sea level driving small-island inhabitants from traditional homelands), and likely changes to climatic extremes and variability. Then again, Lomborg cites only one value for climate damages—$ 5 trillion—even though the same economics papers he refers to for costs of climate policy generally acknowledge that climate damages can vary from benefits up to catastrophic losses.

Here it is claimed without any reference that the government representatives should have stopped cost-benefit analysis because it did not include all categories, but honestly, it seems highly unlikely that it would have been stopped for such a reason – it would have been a much more obvious choice to make the cost-benefit analysis more inclusive. Moreover, it is scientifically unreasonable to argue that since we don't have all the data, we shouldn't even try to get them but just – stop! Then what do we do?

Finally, it is claimed that the $5 trillion (total, discounted cost) is unreasonable, since one economist (Nordhaus) has shown that the cost could be much less and with a catastrophe it could be more. I merely write out the mean estimated cost of Nordhaus' latest model. The cost is also comparable to the one estimated by IPCC in its 1996 report (which due to the end of cost-benefit analysis is not replicated in 2001), finding an annual cost of about 1.5-2% of global GDP. This is not unreasonable, when the global warming costs are compared to the same mean estimates of mitigation (see discussion below).

> It is precisely because the responsible scientific community cannot rule out such catastrophic outcomes at a high level of confidence that climate mitigation polices are seriously proposed. And to give one number—rather than a broad range—for avoided climate damages defies explanation, especially when he does give a range for climate policy costs. This range, however, is based on the economics literature but ignores the findings of engineers. Engineers dispute the economists' typical estimates because the economists fail to take into account preexisting market imperfections such as energy-inefficient machines, houses and processes. These engineering studies, including a famous one by five U.S. Department of Energy laboratories— hardly environmental radicals—suggest that climate policies that provide incentives to replace inefficient equipment with more efficient state-of-the-art products could actually reduce some emissions at *below-zero costs*.

Of course the higher the probability of catastrophic outcomes the more urgent climate mitigation policies appear. However, it is surprising that my critic can now tell us unequivocally that the reason the responsible scientific community cannot rule out catastrophic outcomes that we should cut back carbon emissions. On his interpretation this means that almost the entire three IPCC 2001 reports are worthless in the discussion on what to do about global warming, *because almost all of the models and results discussed are based on non-catastrophic outcomes*. Indeed, the only two major catastrophes discussed (WAIS slipping and THC shutting down), the current models are exactly showing very small risks of these happening (as discussed above). Moreover, almost all of the public discussion has focused on what will happen with rising sea levels, higher mean temperatures, possibly more malaria etc. – all of which are dependent on the non-catastrophic models of the IPCC. (Notice also, that these are the issues brought forth by Schneider himself above: "value of species lost, crucial ecosystem services degraded, inequity created by the poor being hurt more than the rich (which Lomborg does acknowledge), quality of life reduced (for example, a rise in sea level driving small-island inhabitants from traditional homelands), and likely changes to climatic extremes and variability.")

I do agree with Schneider that we need to focus more of our attention to possible catastrophic outcomes ("we ought to spend more effort looking into the likelihood of such [catastrophic] occurrences than on improving our mean prognosis, since it is the extreme occurrences that are truly costly" SE:316). However, this has clearly not been the mainstay of the global warming argument and to suddenly claim so seems both incorrect and disingenuous.

Then Schneider goes on to say, as he did in the summary above, that I give one figure for the cost of global warming ($5T) but a range for "climate policy costs" ($3-33T), and that this "asymmetry" is unreasonable. (In Grist, Schneider puts it less diplomatically: "this putative statistician quotes a range of costs when convenient but not a range of benefits when inconvenient")

The problem is that Schneider is clearly comparing two different kinds of numbers and two different kinds of ranges. The $5T is indeed the central cost estimate of global warming from Nordhaus and Boyer (and it is broadly consistent with the IPCC estimate as discussed above). This is simply the cost of global warming when comparing a Business-as-usual (BAU) world with the hypothetical BAU world, where there was no man-made global warming. In essence, this is the price we'll have to pay if we don't do anything.

The other figures ($3-33T) come from the *extra* costs of choosing different emission cut policies. For instance, the Nordhaus/Boyer estimate for a global stabilization policy (a kind of global Kyoto) would be about $8.5T (SE:310). However, had we done nothing the cost would still have been $5T, so the *extra* cost of choosing global stabilization is about $3.5T. If we instead choose a policy to limit the temperature increase to 1.5°C, the Nordhaus/Boyer estimate is a cost of $37.5T or an *extra* cost of $32.5T. These strategies are some of the limit points in the range of $3-33T, which I mention and which Schneider quotes (SE:318).

Now, clearly you cannot compare the $5T with the range $3-33T, because the $5T *includes* the cost of global warming, the range denotes the *extra* cost. Thus, Schneider is plainly wrong in comparing the two (or claiming that the one should be the cost, and the other the benefits, as he does in Grist).

This also shows why the complaint of range vs. single number is entirely misplaced. The $5T is the cost of global warming under BAU. You can only 'do nothing' in exactly one way. Therefore there is just one number. The $3-33T is a range of net costs under a wide variety of policy choices. Now, we can make lots of different policies, ranging from very light to very invasive – the light policies incurring only a smaller net cost ($3T) and the invasive ones incurring very high net costs (e.g. $33T). So there is no sinister, 'putative statistician' presenting ranges only when it fits him – the single number is a single cost – the range is net costs of a range of policy choices. Both comparing these, and complaining about their asymmetry is simply incorrect.

Then it is claimed that I base the cost estimates on the economics literature, "but ignores the findings of engineers." This is incomprehensible and again incorrect. I spend almost two pages discussing these alternative engineer (or bottom-up) cost estimates (see SE:312-3). The problem with most of these engineer estimates is that they only count direct costs and benefits but neglect the (typically much bigger) indirect costs and benefits on economic production. A clear example of this is given in the book, where UNEP (the UN Environmental Programme) evaluates the CO_2 reduction potential in Denmark: "The main question is: How much of this [CO_2 reduction] potential can be realised without substantial increases in costs associated with finding and implementing these options, and without serious welfare losses? None of these costs are included in the following calculations, which are based on the concept of direct costs" (UNEP 1994:II, 21, SE:426). Thus, I conclude – along with most economists – that these engineer estimates are likely to be huge overestimates of the actual opportunities, because they systematically neglect the costs down the line. Schneider may disagree and have new data to convince us, but he should present such data instead of incorrectly claiming that I have not dealt with the issue.

Finally, Schneider writes that researchers have found that it is possible to provide incentives, which in some cases will cut carbon emissions with net benefits. He also writes this as if it was astounding and somehow in opposition to my arguments. However, the statement that *some emissions* may actually be cut at below-zero costs is entirely standard, and also replicated in my book. The argument is not *whether* there are below-zero cost ways to cut emissions (the so-called 'no regret' options) but *how much*:

> "Most economists are therefore extremely skeptical towards assertions of such improvements in efficiency which can be implemented at no cost or even produce a profit, among other things because these calculations, as we have seen above, often omit important items of expenditure. For this reason, economists also argue that if it really is possible to implement profitable restructuring then it would be reasonable to expect that the possibility would already have been exploited.
>
> One typical economist's expression is that "there is no such thing as a free lunch" – that costs are bound to occur somewhere along the line. Nordhaus expresses the problem of the possible, profitable carbon dioxide emissions reductions thus: "In the colloquialism of economics, this analysis suggests not only that there are free lunches, but that in some restaurants you can get paid to eat!"
>
> A new study also seems to suggest that these "no regret" possibilities are much more limited than normally assumed; it turns out that they can probably only reduce consumption by a couple of percent and could possibly be pushed to providing 5 percent. Equally, a study of monthly electricity bills showed that the engineers' estimates of huge savings from attic insulation fell far short of real payoffs, which were closer to what economists would have expected." (SE:313).

Again, Schneider does not add to our information but he manages to make it seem as if he has countered an argument of mine, though I clearly write that there could 2-5% cuts that could be made at below-zero cost.

> **The Kyoto Protocol**. Lomborg's creation of a 100-year regime for a decade-long protocol is a distortion of the climate policy process. Every IPCC report has noted that carbon dioxide emissions need to be cut by more than 50 percent below most baseline projections to avoid large increases in concentration in the late 21st and 22nd centuries. Most analysts know "Kyoto extended" can't make such large cuts and that both developed and developing nations will have to fashion cooperative and cost-effective solutions over time. This will take a great deal of learning-by-doing: international cooperation is not a common experience. Kyoto is a starting point. And yet Lomborg, with his creation of a straw-man 100-year projection, would squash even this first step.

The book clearly shows that Kyoto-in-itself will have very little effect on global warming, and it is good to see that Schneider concurs. However, he then claims that Kyoto-in-itself is a straw-man, because we should be doing much more. Now, this entails both an analytic and a democratic problem. To take the democratic first: almost all democratic discussion is about choosing or not choosing Kyoto. Since this is the deal offered, would it not be reasonable to discuss *what is the actual outcome of the deal that we are talking about*? Likewise, if Schneider contends that the real issue is not Kyoto but something much more restrictive, *would it not be democratically more honest to say that the decision is not Kyoto but something much more stringent*? Moreover, it is somewhat of a rhetorical misnomer to talk about "Lomborg's *creation* of a 100-year regime for a decade-long protocol is a distortion of the climate policy process." This extension I refer to actually comes from an article written by one of the lead-authors of the 1996 IPCC report, Wigley. He does in fact extend the Kyoto protocol to ascertain what its effect might be. In doing this, is professor Wigley really creating a distortion of the climate policy process?

The other, analytic problem is that Schneider only talks about my analysis of Kyoto and actually neglects that I deal with a range of much more stringent policies (which was why we got the $3-33T range, mentioned above). This seems odd, to say the least. Of course, if Schneider wants to advocate a policy of much-more-than-Kyoto, that is fine, but the cost-benefit analyses are *very clear* on the issue. Kyoto is almost irrespective of how it is implemented a bad deal, and going even further is *a much worse deal*.

This is the absolutely central issue of the book, which Schneider ignores: That *all* cost-benefit analyses show that high carbon reductions are not justified (SE:318): "A central conclusion from a meeting of all economic modelers was: 'Current assessments determine that the 'optimal' policy calls for a relatively modest level of control of CO_2.'"

The last sentence, which claims that I want to "squash" the Kyoto protocol, is language from policy, not science. I try to point out the costs and benefits of our different policy choices, and yes, I point out that for the benefit of Kyoto will be to postpone global warming in 2100 by six years, whereas the cost of Kyoto each year will be as great as the one-off cost of giving clean drinking water and sanitation to every single human being, forever.

> So what then is "the real state of the world"? Clearly, it isn't knowable in traditional statistical terms, even though subjective estimates can be responsibly offered. The ranges presented by the IPCC in its peer-reviewed reports give the best snapshot of the real state of climate change: we could be lucky and see a mild effect or unlucky and get the catastrophic outcomes. The IPCC frames the issue as a risk-management decision about hedging. It is not the everything-will-turn-out-fine affair that Lomborg would have us believe.

It ought not to be necessary to point out, but IPCC offers us insight into the science of global warming, but they (exactly because of the political decision to stop pursuing cost-benefit analysis) do not give us the answer to whether our limited resources are better spent on averting more global warming or on e.g. supplying clean drinking water and sanitation to the world.

Saying that my book is an everything-will-turn-out-fine statement is a rhetorical and entirely misleading treatment of my book. I point out that we should deal with environmental problems, work to decrease air pollution even further, invest in renewable energy research and development etc., as well as tackle the many other, important global problems such as poverty and starvation. The point I make, however, is that we should continuously be aware of the necessary prioritization – that we should strive to make the decisions, which actually do good and not just the ones that sound good. This requires straight and honest analysis that is willing to challenge any however well established myth.

> For such an interdisciplinary topic, the publisher would have been wise to ask natural scientists as well as social scientists to review the manuscript, which was published by the social science side of the house. It's not surprising that the reviewers failed to spot Lomborg's unbalanced presentation of the natural science, given the complexity of the many intertwining fields. But that the natural scientists weren't asked is a serious omission for a respectable publisher such as Cambridge University Press.

The claim of "unbalanced presentation of the natural science" clearly cannot be upheld, given my critic's lack of ability to provide such examples. This also suggests that his stated regret that Cambridge University Press has chosen to publish my book really amounts to a desire to see critical arguments suppressed.

> Unfortunately, angry reviews such as this one will be the result. Worse still, many laypeople and policymakers won't see the reviews and could well be tricked into thinking thousands of citations and hundreds of pages constitute balanced scholarship. A better rule of thumb is to see who talks in ranges and subjective probabilities and to beware of the myth busters and "truth tellers."

This, of course, is a surprising ending. The entire Scientific American piece is sold as a truth-telling story. My own understanding of science is that we should exactly try to bust myths and be truth tellers. Questioning truth saying and myth busting seems to undercut the entire endeavor that Scientific American is trying to achieve, but perhaps and unfortunately it is a very accurate description of the state of the critique.

Stephen Schneider, professor in the department of biological sciences and senior fellow at the Institute for International Studies at Stanford University, is editor of Climatic Change and the Encyclopedia of Climate and Weather and lead author of several IPCC chapters and the IPCC guidance paper on uncertainties.

John P. Holdren

ENERGY: ASKING THE WRONG QUESTION

> Lomborg's chapter on energy covers a scant 19 pages. It is devoted almost entirely to attacking the belief that the world is running out of energy, a belief that Lomborg appears to regard as part of the "environmental litany" but that few if any environmentalists actually hold. What environmentalists mainly say on this topic is not that we are running out of energy but that we are running out of environment—that is, running out of the capacity of air, water, soil and biota to absorb, without intolerable consequences for human well-being, the effects of energy extraction, transport, transformation and use. They also argue that we are running out of the ability to manage other risks of energy supply, such as the political and economic dangers of overdependence on Middle East oil and the risk that nuclear energy systems will leak weapons materials and expertise into the hands of proliferation prone nations or terrorists.

It is good to see that Holdren is actually saying it is correct, we're not running out of energy, and that I am right. Somewhat contradictorily is the pejorative "scant" 19 pages – if I'm right and the issue is easily settled, presumably there is no need to use many more?

However, Holdren then goes on to say that environmentalists are worrying about running out of environment (the statement singled out in SA) and running out of the ability to manage political, economic and military dangers. This is exactly the kind of exposition which I try to counter in my book – without *any references* Holdren manages to describe everything as going *ever worse* and even include into the environmental agenda concepts that are far removed from its core, such as nuclear proliferation, terrorism and economic recession from oil price hikes. Let us just point out one issue-area, air pollution (estimated by the US EPA to be the by far most important area, SE:163). Here we are plainly *not* running out of environment or running out of the air's capacity to absorb without intolerable consequences for human well-being – *all* criteria pollutants in the US have diminished in concentration over the past few decades, as I demonstrate EPA references for in the book (SE:ch.15). Here, Holdren simply choose a sound-good quote (running out of environment), presumably in the quest to defend science, but without references and plainly incorrect, even as demonstrated in my book.

> That "the energy problem" is not primarily a matter of depletion of resources in any global sense but rather of environmental impacts and sociopolitical risks—and, potentially, of rising monetary costs for energy when its environmental and sociopolitical hazards are adequately internalized and insured against—has in fact been the mainstream environmentalist position for decades. It was, for example, the position I elucidated in the 1971 Sierra Club "Battlebook" *Energy* (coauthored with Philip Herrera, then the environment editor for *Time*). It was also the position elaborated on by the Energy Policy Project of the Ford Foundation in the pioneering 1974 report *A Time to Choose*; by Amory Lovins in his influential 1976 *Foreign Affairs* article "Energy Strategy: The Road Not Taken"; by Paul R. and Anne H. Ehrlich and me in our 1977 college textbook *Ecoscience*; and so on. So whom is Lomborg so resoundingly refuting with his treatise on the abundance of world energy resources? It would seem that his targets are pundits (such as the correspondents for *E* magazine and CNN cited at the opening of this chapter) and professional analysts (although only a few of these are cited, and those very selectively) who have argued not that the world is running out of energy altogether but only that it might be running out of cheap oil. Lomborg's dismissive rhetoric notwithstanding, this is not a silly question, nor one with an easy answer.

Holdren acknowledges that my targets are pundits and analysts, who have been arguing that we would be running out of cheap oil. Are these not reasonable people to challenge? He also tries to point out that many even in the 70s did not worry about running out of oil, but it is curious how he neglects the most important and influential environmental influence from the 70s, the *Limits to Growth* argument that clearly predicted oil to run out before 1992 (Meadows et al. 1972:58). Likewise, Ehrlich worried in 1987 that the oil crisis would return in the 1990s (Ehrlich and Ehrlich 1987:222). Finally, he tries to say that the pundits and analysts say something else than do I, because they just worry about running out of cheap oil. But of course, this is the same thing, as is also pointed out in the book: "Even if we were to run out of oil, this would not mean that oil was unavailable, only that it would be very,

very expensive. If we want to examine whether oil is getting more and more scarce we have to look at whether oil is getting more and more expensive" (SE:122).

> Oil is the most versatile and currently the most valuable of the conventional fossil fuels that have long provided the bulk of civilization's energy, and it remains today the largest contributor to world energy supply (accounting for nearly the whole of energy used for transport, besides other roles). But the recoverable conventional resources of oil are believed (on substantial evidence) to be far smaller than those of coal and probably also smaller than those of natural gas; the bulk of these resources appears to lie in the politically volatile Middle East; much of the rest lies offshore and in other difficult or environmentally fragile locations; and it is likely that the most abundant potential replacements for conventional oil will be more expensive than oil has been. For all these reasons, concerns about declining availability and rising prices have long been more salient for oil than for the other fossil fuels. There is, accordingly, a serious technical literature (produced mainly by geologists and economists) exploring the questions of when world oil production will peak of oil might be in 2010, 2030 or 2050, with considerable disagreement among informed professionals on the answers.

This paragraph does not really criticize, but contains the statement "it is likely that the most abundant potential replacements for conventional oil will be more expensive than oil has been." This statement is supplied without references and on faith, but I actually give reference to the US Energy Information Agency (EIA 1997c:37) that today it is "possible to produce about 550 billion barrels of oil from tar sands and shale oil at a price below $30, i.e. that it is possible to increase the present global oil reserves by 50 percent. And it is estimated that within 25 years we can commercially exploit twice as much in oil reserves as the world's present oil reserves" (SE:128). Thus, Holdren's statement seems wrong.

> Lomborg gets right the basic point that the dominance of oil in the world energy market will end not because no oil is left in the ground but because other energy sources have become more attractive relative to oil. But he seems not to recognize that the transition from oil to other sources will not necessarily be smooth or occur at prices as low as those enjoyed by oil consumers today. Indeed, while ridiculing the position that the world's heavy oil dependence may again prove problematic in our lifetimes, he shows no sign of understanding (or no interest in communicating) why there is real debate among serious people about this.

Holdren then agrees with me again, but accuses me of neglecting that the transition *may not necessarily* be smooth or cheap. It is of course true that this could happen (nobody can predict anything 100%) but the basic argument in the book is exactly that the crisis, Holdren sees *may* happen is indeed very unlikely – we have had this kind of fear of running out many times, and each time it has proven incorrect, and moreover, we have good reason to believe that the many different energy sources can give us sufficient energy also for future use at competitive prices.

> Lomborg does not so much as offer his readers a clear explanation of the distinction—crucial to understanding arguments about depletion—between "proved reserves" (referring to material that has already been found and is exploitable at a profit at today's prices, using today's technologies) and "remaining ultimately recoverable resources" (which incorporate estimates of additional material exploitable with today's technology at today's prices but still to be found, as well as material both al will be exploitable with future technologies at potentially higher future prices). And, while noting that most of the world's oil reserves lie in the Middle East (and failing to note, having not even introduced the concept, that a still larger share of remaining ultimately recoverable resources is thought to lie there), he placidly informs us that it is "imperative for our future energy supply that this region remains reasonably peaceful," as if that observation did not undermine any basis for complacency. (At this juncture, one of his 2,930 footnotes helpfully adds that this peace imperative for the Middle East was "one of the background reasons for the Gulf War"!)

Holdren spends half this paragraph complaining that I do not explain all distinctions, while above arguing that I make an obvious point (so that I presumably should not spend vast amounts of space explaining everything). Even on a kind reading, this critique seems excessively compulsive.

Accepting that I do point out that most of the world's oil reserves lie in the Middle East, Holdren nevertheless criticizes me for not spending enough paper on digressing into other areas like International Relations (the relative peacefulness of the Middle East and its consequences for commodity trade). Again, it is unclear what standard this critique sets up, wanting the energy discussion to take much more space or much less?

The final parenthetical comment entirely leaves out that I actually refer to a congressional research paper for this statement.

> Lomborg's treatment of energy resources other than oil is not much better. He is correct in his basic proposition that resources of coal, oil shale, nuclear fuels and renewable energy are immense (which few environmentalists—and no well-informed ones—dispute). But his handling of the technical, economic and environmental factors that will govern the circumstances and quantities in which these resources might actually be used is superficial, muddled and often plain wrong. His mistakes include apparent misreadings or misunderstandings of statistical data—in other words, just the kinds of errors he claims are pervasive in the writings of environmentalists— as well as other elementary blunders of quantitative manipulation and presentation that no self-respecting statistician ought to commit.

This is the paragraph in which Holdren gets tough. Here he says that the rest is not much better than the treatment of oil (where Holdren agreed with much and found no concrete errors). Here he also says Lomborg "is correct in his basic proposition," but then that I make loads of misreadings or misunderstandings as well as elementary blunders. These are harsh words Holdren should be able to back up below.

> He tells us correctly, for example, that the world has huge resources of coal, but in observing that "it is presumed that there is sufficient coal for well beyond the next 1,500 years" he says nothing about the rate of coal use for which this conclusion might obtain. Concerning the environmental questions that increased reliance on coal would raise, he writes the following: "Typically, coal pollutes quite a lot, but in developed economies switches to low-sulfur coal, scrubbers and other air-pollution control devices have today removed the vast part of sulfur dioxide and nitrogen dioxide emissions." To the contrary, data readily available on the Web in the Environmental Protection Agency report National Air Pollutant Emission Trends 1900–1998 reveal that U.S. emissions of nitrogen oxides from coal-burning electric power plants were 6.1 million short tons in 1980 and 5.4 million short tons in 1998. Emissions of sulfur dioxide from U.S. coal-burning power plants were 16.1 million short tons in 1980 and 12.4 million short tons 1998. These are moderate reductions, welcome but hardly the "vast part" of the emissions.

The first main example of how I misread or misunderstand environmental data ("just the kinds of errors [Lomborg] claims are pervasive in the writings of environmentalists") clearly suggests a casual reading of what I have written. Holdren claims that I'm correct in saying that the world has huge coal resources, but when stating that the world has 1,500 years of coal, that I should say nothing about the rate of coal use for which this conclusion might obtain. This is curious, because I use the same metric throughout: that the years-of-consumption are measured from the year discussed (SE:127):

> "As with oil and gas, coal reserves have increased with time. Since 1975 the total coal reserves have grown by 38 percent. In 1975 we had sufficient coal to cover the next 218 years at 1975 levels, but despite a 31 percent increase in consumption since then, we had in 1999 coal reserves sufficient for the next 230 years. The main reason why years-of-consumption have not been increasing is due to reduced prices. The total coal resources are estimated to be much larger – it is presumed that there is sufficient coal for well beyond the next 1,500 years."

And if readers are curious about the 1,500 years, they (as almost everything else in the book) have a reference, which can be consulted. Why not get hold of this reference before attacking me for misreading or misunderstanding? And even *if* there was a problem, why would it be important, when Holdren accepts that the main point (huge coal resources) is correct?

Holdren's other claim is that my statement on diminished pollution from coal is incorrect. It is unclear whether he believes that I am misreading or misleading, since he does not seem to have checked my source, from which I take this statement. Anyway, Holdren claims that US emissions for SO_2 have only declined 23 percent since 1980 (0.23=1-12.4/16.1), rendering my statement incorrect. However, Holdren seems to neglect that the US use of coal for coal-burning power plants has increased dramatically over the past decades – since 1980 it has increased from 569.3 million short tons to 951.6 million short tons in 1999 (http://www.eia.doe.gov/emeu/aer/txt/tab0703.htm). Thus, the SO_2 pollution per quantity of coal burned has declined not just 23 percent but 56 percent. Moreover, why did Holdren pick 1980 as the starting point, when clearly environmental improvements have been taking place since at least 1970? And from 1970, the SO_2 pollution per quantity of coal has dropped by 75 percent, underscoring that the statement of vastly diminished pollution from coal burning is correct.

Moreover, I clearly state the very significant contribution to air pollution that is still being made by coal but this goes unrecognized in the SA critique. For the benefit of those who do not have access to my text I repeat below the unequivocal statement I make about the environmental hazard of coal (SE:127):

> "Typically, coal pollutes quite a lot, but in developed economies switches to low-sulfur coal, scrubbers and other air-pollution control devices have today removed the vast part of sulfur dioxide and nitrogen dioxide emissions. Coal, however, is still a cause of considerable pollution globally, and it is estimated that many more than 10,000 people die each year because of coal, partly from pollution and partly because coal extraction even today is quite dangerous."

> Concerning nuclear energy, Lomborg tells us that it "constitutes 6 percent of global energy production and 20 percent in the countries that have nuclear power." The first figure is right, the second seriously wrong. Nuclear energy provides a bit less than 10 percent of the primary energy supply in the countries that use this energy source. (It appears that Lomborg has confused contributions to the electricity sector with contributions to primary energy supply.) After a muddled discussion of the relation between uranium-resource estimates and breeding (which omits altogether the potentially decisive issue of the usability of uranium from seawater), he then barely notes in passing that breeder reactors "produce large amounts of plutonium that can be used for nuclear weapons production, thus adding to the security concerns." He should have added that this problem is so significant that it may preclude use of the breeding approach altogether, unless we develop technologies that make breeding much less susceptible to diversion of the plutonium while not making this approach even more uneconomic than it is today.

Holdren is correct here that the 20 percent is an error – I should have written 20 percent of the electricity generation from nuclear power (this will naturally be put up on the error page of my web site). Naturally, one would like such errors not to occur, but to claim that it is a "serious error," when the figure is given as general information and not used in any arguments seems out of proportion.

The other critique, that Lomborg "barely notes in passing" the added security concerns seems again out of proportion – the entire nuclear fission discussion takes up three paragraphs of 272 words, where the security concern is mentioned twice. This is hardly "barely notes in passing." For reference, here are the three paragraphs (SE:129):

> "Ordinary nuclear power exploits the energy of fission by cleaving the molecules of uranium-235 and reaping the heat energy. The energy of one gram of uranium-235 is equivalent to almost three tons of coal. Nuclear power is also a very clean energy source which, during normal operation, almost does not pollute. It produces no carbon dioxide and radioactive emissions are actually lower than the radioactivity caused by coal-fueled power plants.
>
> At the same time nuclear power also produces waste materials that remain radioactive for many years to come (some beyond 100,000 years). This has given rise to great political debates on waste deposit placement and the reasonableness of leaving future generations such an inheritance. Additionally, waste from civilian nuclear reactors can be used to produce plutonium for nuclear weapons. Consequently, the use of nuclear power in many countries also poses a potential security problem.
>
> For the moment there is enough uranium-235 for about 100 years. However, a special type of reactor – the so-called fast-breeder reactor – can use the much more common uranium-238 which constitutes over 99 percent of all uranium. The idea is that while uranium-238 cannot be used directly in energy production it can be placed in the same reactor core with uranium-235. The uranium-235 produces energy as in ordinary reactors, while the radiation transforms uranium-238 to plutonium-239 which can then be used as new fuel for the reactor. It sounds a bit like magic, but fast-breeder reactors can actually produce more fuel than they consume. Thus it is estimated that with these reactors there will be sufficient uranium for up to 14,000 years. Unfortunately these reactors are more technologically vulnerable and they produce large amounts of plutonium that can be used for nuclear weapons production, thus adding to the security concerns."

> Lomborg has some generally sensible things to say about the large contributions that are possible from increased energy end-use efficiency and from renewable energy—on these

> topics he seems, to his credit, to be more a contributor to the "environmental litany" than a critic of it. But on these subjects as on the others, his treatment is superficial, uneven and marred by numerous errors and infelicities. For example, he persistently presents numbers to two- and three-figure precision for quantities that cannot be known to such accuracy: "43 percent of American energy use is wasted"; "the costs of carbon dioxide" emissions are "0.64 cents per kWh"; plant photosynthesis is "1,260 EJ" annually. He makes claims, based on single citations and without elaboration, that are far from representative of the literature: "We know today that it is possible to produce safe cars getting more than 50-100 km per liter (120–240 mpg)." (How big would these cars be, and powered how?) He bungles terminology: "Energy can be stored in hydrogen by catalyzing water." (He must mean "by electrolyzing water" or "by catalytic thermochemical decomposition of water.") And he propagates a variety of conceptual confusions, such as the idea that grid-connected wind power requires "a sizeable excess capacity" in the windmills because these alone "need to be able to meet peak demand."

Again, Holdren says I am right about many things, but still the treatment is criticized thoroughly. Most incredibly, I am criticized for being *too* precise. Of course, there are a lot of numbers that we do not know well, but the general idea in statistics is that if these numbers have been generated by a process described by evenly distributed errors, the more precise number is still the best predictor of the real number – or to put it more clearly: If studies have shown that 43 percent of all American energy use is wasted, then the real number may very well be 38-48 percent, but had I rounded this figure down to 40 or up to 45, it would have been worth less – and Holdren could then have criticized me for conveying muddled results. Moreover, the 43 percent is actually described right off one of the best-selling college environment books by professor Miller – is Holdren also claiming that he is wrong?

Holdren claims that I make claims that are far from representative of the literature, gives us one example, *but does not give us other references that show this statement to be incorrect or even an indication of why this statement would be far from representing the literature.*

I am accused of bungling terminology – it is true that 'catalyzing' was translated from the Danish version, and should have been electrolyzing. But again, how important is this?

The conceptual confusion seems to stem from Holdren not reading the two paragraphs. If the wind-mills were connected to a coal-fired power grid, then clearly they would not need to be able to meet peak demand, but this clearly would not be a long-term renewable strategy. Rather, I discuss the interaction of dams and windmills (SE:134):

> "If the power grid is hooked to dams, these can be used for storage. Essentially, we use wind power when the wind blows, and store water power by letting water accumulate behind the dams. When there's no wind, water power can produce the necessary electricity.
>
> However, this implies that both wind power and water power require a sizeable excess capacity, since both need to be able to meet peak demand. The solution also depends on relatively easy access to large amounts of hydroelectric power."

> Of course, much of what is most problematic in the global energy picture is covered by Lomborg not in his energy chapter but in those that deal with air pollution, acid rain, water pollution and global warming. The last is devastatingly critiqued by Stephen Schneider on page 62. There is no space to deal with the other energy-related chapters; suffice it to say that I found their level of superficiality, selectivity and misunderstanding roughly consistent with that of the energy chapter reviewed here. This is a shame. Lomborg is giving skepticism— and statisticians—a bad name.

Given that Holdren could find little but a badly translated word and a necessary specification for nuclear energy production in this chapter, I find comfort that he finds the other chapters of equal value. However, I do find the tone of the entire critique surprisingly rough, indicating that Holdren found it necessary to substitute good analysis with plain negative words.

> *John P. Holdren is the Teresa and John Heinz Professor of Environmental Policy at the John F. Kennedy School of Government, as well as professor of environmental science and public policy in the department of earth and planetary sciences, at Harvard University. From 1973 to 1996 he coled the interdisciplinary graduate program in energy and resources at the University of California, Berkeley. He is a member of the National Academy of Sciences and the National Academy of Engineering.*

John Bongaarts

POPULATION: IGNORING ITS IMPACT

Around the world, countries are experiencing unprecedented demographic change. The best-known example is an enormous expansion in human numbers, but other important demographic trends also affect human welfare. People are living longer and healthier lives, women are bearing fewer children, increasing numbers of migrants are moving to cities and to other countries in search of a better life, and populations are aging. Lomborg's unbalanced presentation of some of these trends and their influences emphasizes the good news and neglects the bad. Environmentalists who predicted widespread famine and blamed rapid population growth for many of the world's environmental, economic and social problems overstated their cases. But Lomborg's view that "the number of people is not the problem" is simply wrong.

First, Bongaarts write that things are going better and that the environmentalists' predictions of widespread famines were wrong. Okay. So, not much of 'Science defending itself against Lomborg' here. But then he sets the high standard of saying that I am wrong in saying that the number of people is not the problem. We will see below that Bongaarts does not even try to lift the burden of proving this statement and rather abandon it at the end. But, of course, its inclusion here makes the piece appear stronger.

Curiously, Bongaarts also neglects to write *why* I say the number of people is not the problem and instead identify *poverty* (SE:48):

> "We often hear about overpopulation of the Earth. We most often see overpopulation illustrated by large glossy color pictures of tightly packed masses or overcrowded underground stations.
>
> The famous population biologist Paul Ehrlich in his best-seller on the population explosion wrote: "Psychologically, the population explosion first sunk in on a stinking hot night in Delhi. The streets were alive with people. People eating, people washing themselves, people sleeping, people working, arguing and screaming. People reaching their hands in through taxi windows to beg. People shitting, people pissing. People hanging off buses. People driving animals through the streets. People, people, people."
>
> The point is, however, that the number of people is not the problem. Many of the most densely populated countries are in Europe. The most densely populated region, Southeast Asia, has the same number of people per square km as the United Kingdom. The Netherlands, Belgium and Japan are far more densely populated than India, and Ohio and Denmark are more densely populated than Indonesia.
>
> Today, Ehrlich and others also agree on this. Instead, two other interpretations of overpopulation have come into the fore. One of them conjures up images of starving families; wretched, cramped conditions and premature death. Such images are real enough but are actually the result of poverty rather than population density. We shall discuss poverty below."

His selective use of statistics gives the reader the impression that the population problem is largely behind us. The global population growth rate has indeed declined slowly, but absolute growth remains close to the very high levels observed in recent decades, because the population base keeps expanding. World population today stands at six billion, three billion more than in 1960. According to U.N. projections, another three billion will likely be added by 2050, and population size will eventually reach about 10 billion.

Here, Bongaarts accuses me of fudging the statistics. However, the ensuing documentation seems to point its accusing finger the other way. Bongaarts says that the global population growth rate has indeed declined slowly, but the absolute growth remains close to the top. First, he makes it sound like I don't say that, but I do, as can be seen here in the relevant paragraph from the book (SE:47):

> "As demonstrated in Figure 13 [graph of the rate and absolute number of growth], the growth of the global population peaked in the early 1960s at just over 2 percent a year. It has since fallen to 1.26 percent and is expected to fall further, to 0.46 percent, by 2050. Even so, the absolute growth of the population did not peak until 1990, when almost 87 million people were added to world population. Today growth is around 76 million per year and will have fallen to approximately 43 million by 2050."

Second, when Bongaarts say that the rate has only declined “slowly”, though it has actually declined from 2.17% in 1964 down to 1.26% today (more than a 40% decline). Bongaards claim that the absolute growth remains close to the very high levels of the recent decades – yet today’s 76 million is the lowest number in the last two decades.

I also show the UN graph of population development from 1750-2200 (SE:46), with the latest 2000 UN estimates, where I point out that the expectation is actually 9.3 billion in 2050, and the stable population estimated at almost 11 billion (not 10, as Bongaarts claim):

> “The UN continuously calculates how many of us live on Earth now and will in the future. These figures have been adjusted downward by 1.5 billion for 1994, 1996 and 1998 and upwards again by half a billion for 2000, because of changes in the speed with which the fertility falls in different countries. The latest long-term forecast from 2000 can be seen in Figure 11. It shows that there will be almost 8 billion people on Earth by 2025 and about 9.3 billion by 2050. It is estimated that the world’s population will stabilize just short of 11 billion in the year 2200.” (SE:47).

Apparently, Bongaarts’ paragraph was supposed to show that I fudged the statistics, but not only did Bongaarts not show this, indeed his own argument seemed curiously fudged.

> Any discussion of global trends is misleading without taking account of the enormous contrasts among world regions. Today’s poorest nations in Africa, Asia and Latin America have rapidly growing and young populations, whereas in the technologically advanced and richer nations in Europe, North America and Japan, growth is near zero (or, in some cases, even negative), and populations are aging quickly. As a consequence, nearly all future global growth will be concentrated in the developing countries, where four fifths of the world’s population lives. The projected rise in population in the developing world between 2000 and 2025 (from 4.87 to 6.72 billion) is actually just as large as the recordbreaking increase in the past quarter of a century. The historically unprecedented population expansion in the poorest parts of the world continues largely unabated.

Again, Bongaarts seem to accuse me of not taking into account the enormous differences among world regions, though one of the most consistent factors in the book is the presentation of data for both developed and developing countries. Yet, I did not present a chart for population growth in the developing world, so I will bring it here (Figure 1). Here, again we see almost the same pattern as in Figure 13 from the book (SE:47), and indeed we also see how questionable Bongaarts analysis is: He claims that the historically unprecedented population expansion continues largely unabated, yet, the growth of 74 million in 2001 is the lowest since 1984, and the rate has dropped from the maximal 2.6% to 1.5% today. The claim that the growth from 1975-2000 is almost the same as the growth 2000-2025 is technically true, but very misleading – the growth in the early quarter century period came from ever *increasing* numbers of people being added, whereas every year from 2000 onwards will see ever *fewer* numbers being added.

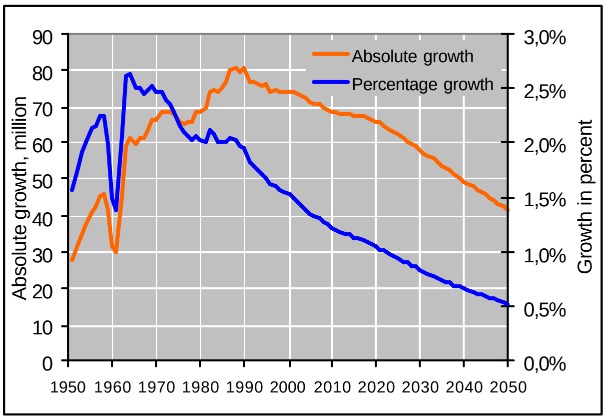

Figure 1 Absolute and relative population growth for the developing world, 1950-2050, prediction from 2001. Source: USBC 2001a.

> Past population growth has led to high population densities in many countries. Lomborg dismisses concerns about this issue based on a simplistic and misleading calculation of density as the ratio of people to all land. Clearly, a more useful and accurate indicator of density would be based on the land that remains after excluding areas unsuited for human habitation or agriculture, such as deserts and inaccessible mountains. For example, according to his simple calculation, the population density of Egypt equals a manageable 68 persons per square kilometer, but if the unirrigated Egyptian deserts are excluded, density is an extraordinary 2,000 per square kilometer. It is therefore not surprising that Egypt needs to import a large proportion of its food supply. Measured properly, population densities

> have reached extremely high levels, particularly in large countries in Asia and the Middle East.

It is curious that Bongaarts, trying to show how wrong I am, is forced to use a hypothetical argument (of Egypt) and does not even find it necessary to point out that I never make this argument. The real challenge is in the text shown above, where I point out some of the most densely populated areas of the world are in Europe. Is Bongaarts obvious point at all relevant to my examples of e.g. the Netherlands being far more densely populated than India? We are never told.

> Why does this matter? The effect of population trends on human welfare has been debated for centuries. When the modern expansion of human numbers began in the late 18th century, Thomas Robert Malthus argued that population growth would be limited by food shortages. Lomborg and other technological optimists correctly note that world population has expanded much more rapidly than Malthus envisioned, growing from one billion to six billion over the past two centuries. And diets have improved. Moreover, the technological optimists are probably correct in claiming that overall world food production can be increased substantially over the next few decades. Average current crop yields are still below the levels achieved in the most productive countries, and some countries still have unused potential arable land (although much of this is forested).

Again, Bongaarts almost tell us that I am correct, though he does underplay the argument in two rather conspicuous ways. He says diets have improved. That is true, though an understatement. Actually, the global availability of calories has increased from 2257 calories in 1961 to 2792 in 1998, an increase of 24%, and for the developing world an even greater increase of 38% from 1932 to 2663 calories (FAO 2001a).

Moreover, he says that food production is probably capable of being increased substantially because of yield increases and "some countries still have unused potential arable land (although much of this is forested)." This, again, is a serious understatement for arable land and an overstatement for forests. In the latest FAO report on Agriculture towards 2030 (FAO 2000d), FAO explicitly discusses the use of land in agricultural production and the extra availability of agricultural land.

FAO estimates that at present about 1.5Gha or 11% of the globe's land surface is used for agriculture, and an additional 2.9Gha has crop production potential (FAO 2000d:98). Of this area about 45% is forested (FAO 2000d:103). So there is ample room for bringing in new agricultural land and none of it needs be forested. Actually, the FAO estimate for the developing countries (developed will probably not increase their area at all) a much lower increase in land use till 2030 of about 0.12Gha, an increase of 12%. This means that the agricultural land usage will go from 32% of the potential land use to 36% in 2030. Globally, this probably means an increase in agricultural land use from 11% to 12% of the land surface.

> Agricultural expansion, however, will be costly, especially if global food production has to rise twofold or even threefold to accommodate the demand for better diets from several billion more people. The land now used for agriculture is generally of better quality than unused, potentially cultivable land. Similarly, existing irrigation systems have been built on the most favorable sites. And water is increasingly in short supply in many countries as the competition for that resource among households, industry and agriculture intensifies. Consequently, each new increase in food production is becoming more expensive to obtain. This is especially true if one considers environmental costs not reflected in the price of agricultural products.

This is one of the stunningly simplistic analyses from the environmental Litany: Since we have already used the best land sites with the easiest irrigation etc., an expansion of the agricultural production will lead to higher prices. However, this clearly neglects the historical trend towards ever more efficient production and better crops which has given us steadily declining prices. However, Bongaarts simply does not supply any evidence that his scenario of increasing prices should become true – yet, both IFPRI, USDA and the World Bank predicts ever lower prices (IFPRI 1997, 1999; ERS 1997:4; USDA 2000b; Mitchell et al. 1997), which is a continuation of the almost constant decrease in food prices since 1800 (data on wheat prices from 1316-2000 in SE:62).

> Lomborg's view that the production of more food is a non-issue rests heavily on the fact that world food prices are low and have declined over time. But this evidence is flawed. Massive governmental subsidies to farmers, particularly in the developed countries, keep food prices artificially low. Although technological developments have reduced prices, without these massive subsidies, world food prices would certainly be higher.

The last hypothetical sentence is true – without subsidies, prices would be higher, but the argument lies with the *trend* of the price, which is downwards, and has been so since early 1800s. It is still curious that Scientific American lets their critic 'defend science' by referring to such fickle speculation instead of giving real references. Again, this could possibly be due to the fact that all major food analysis institutions still predict decreasing food prices (as above, IFPRI 1997, 1999; ERS 1997:4; USDA 2000b; Mitchell et al. 1997).

> The environmental cost of what Paul R. and Anne H. Ehrlich describe as "turning the earth into a giant human feedlot" could be severe. A large expansion of agriculture to provide growing populations with improved diets is likely to lead to further deforestation, loss of species, soil erosion, and pollution from pesticides and fertilizer runoff as farming intensifies and new land is brought into production. Reducing this environmental impact is possible but costly and would obviously be easier if population growth were slower. Lomborg does not deny this environmental impact but asks unhelpfully, "What alternative do we have, with more than 6 billion people on Earth?"

Surprisingly and without any statistical backing, Bongaarts invoke the doomsday metaphor of "turning the earth into a giant human feedlot." However, as we saw above, we are currently using about 11% of the global land surface area for agriculture, and in 2030, where we will be feeding more than 8 billion *much* better (3100 calories per person) we will be using 12% – hardly "turning the earth into a giant human feedlot." Moreover, had Bongaarts accessed the available statistics, he could have seen that the increase in agricultural land use was actually *bigger* over the last 25 years than the coming 30 years (increasing land use by 0.173Gha, compared to the expected increase of 0.12Gha, FAO 2000d:105).

Again, Bongaarts actually acknowledges that I do discuss the environmental impact, and yet only says I ask the unhelpful question of what are the alternatives? Yet, this is misleading on at least three counts. First, I do give an insight as to how to control population in the long run – this is a question of poverty reduction and development (SE:46). This is why it was also problematic that Bongaarts cut off the quote at the top of his article where I also point out poverty (SE:48).

Second, I do actually show some of the bad consequences of listening to parts of the environmental movement in their advice as to abating some of the problems with food production. An often heard call is to move to organic farming, because it would mean less fertilizer runoff. Yet, this would have other, much more drastic consequences (SE:197):

> "Today, it is estimated that 40 percent of all crop nitrogen comes from synthetic fertilizer, and about one-third of human protein consumption depends on synthetic fertilizer. Moreover, fertilizer allows us to produce more food on less farmland. This is one of the reasons why the global population could double from 1960 to 2000 and get better fed, although farmland area only increased 12 percent.
>
> This should be compared with the quadrupling of farmland from 1700 to 1960 which of course came from the conversion of large tracts of forests and grasslands. Essentially, the extraordinary increase in fertilizer availability from 1960 onwards has made it possible to avoid a dramatic increase in human pressure on other natural habitats. Had fertilizer use remained at the 1960 level, we would need at least 50 percent more farmland than the present-day use – the equivalent of converting almost a quarter of the global forests. Over the coming decades to 2070, were we to forsake fertilizer, the need for farmland to feed 10 billion people better would place ever higher demands on the globe – one study puts the farmland requirement at an impossible 210 percent of the land surface area. Thus, synthetic fertilizer has been and especially will be crucially important in feeding the world while leaving sufficient space for other species. However, the doubling of globally available nitrogen has also caused problems." [The text goes on to talk about the problems of fertilizer runoff problems.]

Third, it is amazing that Bongaarts criticizes me for not answering the question (which I do) and yet does not himself come up with any answer. This becomes evident in the following paragraph.

> Lomborg correctly notes that poverty is the main cause of hunger and malnutrition, but he neglects the contribution of population growth to poverty. This effect operates through two distinct mechanisms. First, rapid population growth leads to a young population, one in which as much as half is below the age of entry into the labor force. These young people have to be fed, housed, clothed and educated, but they are not productive, thus constraining the economy. Second, rapid population growth creates a huge demand for new jobs. A large number of applicants for a limited number of jobs exerts downward pressure on wages,

> contributing to poverty and inequality. Unemployment is widespread, and often workers in poor countries earn wages near the subsistence level. Both of these adverse economic effects are reversible by reducing birth rates. With lower birth rates, schools become less crowded, the ratio of dependents to workers declines as does the growth in the number of job seekers. These beneficial demographic effects contributed to the economic "miracles" of several East Asian countries. Of course, such dramatic results are by no means assured and can be realized only in countries with otherwise sound economic policies.

Here, Bongaarts makes what seems like a reasonable argument – though unfortunately without any references. But notice his argument is entirely conditional: "Both of these adverse economic effects are reversible by reducing birth rates." Yes, and that was the main problem, right? Yet, his only indication of how these birth rates could be reduced is that this can happen "in countries with otherwise sound economic policies." Thus, Bongaarts solution is a subset of my point that in order to combat population growth, we need to address and reduce poverty. (It is a subset, because sound economic policies that only benefit the rich will probably still leave the mass of poor people with a high reproduction.)

> Lomborg approvingly notes the huge ongoing migration from villages to cities in the developing world. This has been considered a welcome development, because urban dwellers generally have higher standards of living than villagers. Because the flow of migrants is now so large, however, it tends to overwhelm the absorptive capacity of cities, and many migrants end up living in appalling conditions in slums. The traditional urban advantage is eroding in the poorest countries, and the health conditions in slums are often as adverse as in rural areas. This points to another burden of rapid population growth: the inability of governments to cope with large additions of new people. In many developing countries, investments in education, health services and infrastructure are not keeping up with population growth.

Here, Bongaarts again makes an unsubstantiated claim that "the traditional urban advantage is eroding in the poorest countries." Where does this come from? However, the following sentence is logically flawed because it compares health conditions among the *worst* areas of the city (slums) with the *average* rural areas, a typical and incorrect comparison as I have already pointed out in my book (SE:362):

> "Of course, shanty-town newcomers may not compare favorably with the *average* rural population, but presumably many left the countryside because they were worse off than the average, see e.g. Siwar and Kasim 1997:1,532 for a partly unsuccessful test."

> It is true that life has improved for many people in recent decades, but Lomborg does not acknowledge that this favorable trend has been brought about in part by intensive efforts by governments and the international community. Investments in developing and distributing "green revolution" technology have reduced hunger, public health campaigns have cut death rates, and family-planning programs have lowered birth rates. Despite this progress, some 800 million people are still malnourished, and 1.2 billion live in abject poverty. This very serious situation calls for more effective remedial action. Lomborg asks the developed nations to fulfill their U.N. pledge to donate 0.7 percent of their GNPs to assist the developing world, but few countries have met this goal, and the richest nation on earth, the U.S., is one of the stingiest, giving just 0.1 percent of its GNP. The trend in overseas development assistance from the developed to the developing world is down, not up. Unfortunately, the unrelenting we-are-doing-fine tone that pervades Lomborg's book encourages complacency rather than urgency.

This is another give-away like Schneider saying that one should be wary of truth-tellers and myth-busters. Basically, Bongaarts is telling us that I am correct in my description of life, but that I should also have said who exactly carried out the concrete improvements. This borders to silly – of course, the improvement has physically been carried out "in part by intensive efforts by governments and the international community," and presumably in part by intensive efforts by individuals and communities. Who else could have done so? This is seemingly a request to make an almost tautological addition to the text: "These documented examples of progress were in part due to actions from government and the international community" and that this statement is missing constitute a failure of the analysis.

Yet, the issue, as Scientific American puts it up, is whether my data is correct – whether the state of the world has indeed improved or whether the doomsayers have been right in claiming declines everywhere. And their third critic simply comes out and says: 'Yes, Lomborg is right, but he fails to add a tautological statement.'

> Population is not the main cause of the world's social, economic and environmental problems, but it contributes substantially to many of them. If population had grown less rapidly in the past, we would be better off now. And if future growth can be slowed, future generations will be better off.

This last statement takes out much of the strength of the entire piece. It basically leaves the argument to a plain conditional: if we *could* have cut population growth things *would* be better, but of course, we could only have cut population growth if things would have been better – which is what is happening now, and which is why population growth is constantly decreasing.

> *John Bongaarts is vice president of the Policy Research Division of the Population Council in New York City. From 1998 to 2000 he chaired the Panel on Population Projections of the National Academy of Sciences, National Research Council. He is a member of the Royal Dutch Academy of Sciences.*

Thomas Lovejoy

BIODIVERSITY: DISMISSING SCIENTIFIC PROCESS

> Biologists are trained to have a healthy respect for statistics and statisticians. It was disconcerting, therefore, to find that before even examining the extinction problem— and the numbers invoked to demonstrate that it is or is not a problem— Lomborg begins the chapter on biodiversity with a section questioning whether biodiversity is important. In less than a page, he discounts its value both as the library for the life sciences and as provider of ecosystem services (in part because of a general absence of markets for these services).

Lovejoy opens his critique with saying that it was "disconcerting" that I begin the biodiversity chapter with questioning whether biodiversity is important before discussing its size. I understand why this would be disconcerting to an environmental advocate or policy participant, but why would it be disconcerting to a scientist that we question the basis for our concern?

Notice, how Lovejoy does not actually contend the finding of my discussion – only that simply by asking the question, it should be obvious to any good man or woman that I am wrong.

> When he finally gets to extinction, he totally confounds the process by which a species is judged to be extinct with the estimates and projections of extinction rates. Highly conservative rules hold that to be declared officially extinct, not only does a species have to be known to science, it has to be observed going to extinction (as in the case of the passenger pigeon, the last individual of which perished in the Cincinnati Zoo in 1914). Or, in the absence of direct observation, it must not have been seen in nature for 50 years.

The text claims that I totally confound the process of judging a species to be extinct and the projections of extinction rates. This seems like an odd statement, given that I make exactly the same point as Lovejoy in at least two places:

> "Note that because of the severe regulations for documenting extinctions these figures certainly underestimate their true number." (SE:250, note to table).

Likewise, it is pointed out during the discussion of extinction (SE:252):

> "… when we look at the last 400 years, there are other things to consider as far as extinction is concerned. For one thing, in order to document extinction, one must have looked for the species for several years wherever it may exist. A task of this magnitude requires a lot of resources, which reduces the number of documented cases of extinction to a minimum. For another thing, there is much greater focus on mammals and birds than on the other groups."

> Projections of extinction rates, on the other hand, are generally based on the long-established relation between species number and area (which dates to 1921, not to the 1960s, as Lomborg maintains, and which demonstrates the rate at which species number increases with increase in area). Researchers then project what the reduction in a natural habitat will mean in terms of species loss. The disappearance of a species is not necessarily instantaneous, and thus some species that survive the initial reduction of the habitat are essentially "living dead"—they are not able to survive over the long term. The loss of species from habitat remnants is a widely documented phenomenon—in contrast to Lomborg's inclusion of an out-of-date assertion that no credible attempt has been made to pin down the underlying scientific assumptions.
>
> As a consequence, a seemingly major contradiction that Lomborg then offers is no contradiction at all: the reduction of the Brazilian Atlantic Forest formation to something on the order of 10 percent of its original extent and the lack of large numbers of recorded extinctions. First, this is a region with very few field biologists to record either species or their extinction. Second, there is abundant evidence that if the Atlantic forest remains as reduced and fragmented as it is, it will lose a sizable fraction of the species that at the moment are able to hang on.

Yes, the Brazilian Atlantic rainforest has been cut down by about 90% – this would mean that the species-area formula would expect a loss of about half of all species. Yet, when the Brazilian Society of Zoology analyzed a group of almost 300 animals and could not find *a single species* which had died out. Likewise, when they examined their list of plants, they could find no extinctions either (see the quotes of their report in SE:255). Of course, as Lovejoy points out, there are fewer biologists than perhaps in Europe or the US to register these losses, but expecting 150 dead species and finding none?!

This is why the IUCN (World Conservation Union, the organization which manages all species threats and extinctions) found this evidence quite disturbing as to the species-area relationship.

The statement that the species are essentially 'living dead' – just barely hanging on, but will eventually die out, this seems almost farcial – what Lovejoy neglects to mention, is that the clearing of the Brazilian Atlantic rainforest happened in the 1800s, so we have had more than 100 years to see the 'barely hanging on species' die out, and they haven't. This is the second reason why the field experiment is so powerful. And this really deserves a better discussion than Scientific American gives it.

If the example is powerful and surprising for IUCN, it seems unreasonable that Lovejoy simply says that the "seemingly major contradiction that Lomborg then offers is no contradiction at all." It is a contradiction for IUCN:

> "The coastal forests of Brazil have been reduced in area as severely as any tropical forest type in the world. According to calculation, this should have led to considerable species loss. Yet no known species of its old, largely endemic, fauna can be regarded as extinct." (Holden 1992:xvii).

> In another supposed example of species surviving habitat loss, he notes that few species went extinct when the eastern forests of the U.S. were reduced to 1 to 2 percent of their original area. But only the old-growth forests shrank that much; total forest cover never fell below roughly 50 percent—allowing much biodiversity to survive as forest returned to an even greater area. Consequently, the small number of bird extinctions does not contradict what species-area considerations predict but instead confirms them.

Again, this is another example from the discussion of the IUCN (and the figures of forest reduction comes from the IUCN, too) – if IUCN consider it problematic, it seems slightly haughty to ignore it on such flimsy basis.

> In presenting an analysis for Puerto Rico, Lomborg again cites apparently contradictory evidence that although 99 percent of the primary forest was lost, the island ended up with more birds than it supported before deforestation. First of all, total forest cover was never so dramatically reduced. More significant, he ignores that seven of the 60 species unique to Puerto Rico were lost, and the additional species are not only invasives from other parts of the world but live in a wide variety of habitats. He completely misses the point that the world's bird fauna was reduced by seven species.

This is a very good example of the quality of the critique. Here Lovejoy claims that Lomborg "ignores that seven of the 60 species unique to Puerto Rico were lost." Well, here is the quote (SE:254, italics added):

> "The largest tropical study of the correlation between rainforest and the extinction of species was carried out in Puerto Rico by Ariel Lugo of the United States Department of Agriculture. He found that the primary forest had been reduced by 99 percent over a period of 400 years. 'Only' *seven out of 60 species of birds* had become extinct although the island today is home to 97 species of birds. This indicates a serious problem with Wilson's rule of thumb. And what is perhaps more astonishing is that even though the area of primary forest on Puerto Rico was reduced by 99 percent they ended up with more species of birds! [Endnote 2048: Obviously, if one alters the number of niches there will be room for more birds, so this need not come as such a surprise. The most significant finding is that *only seven species of birds became extinct*.]"

Lovejoy claims that I ignore and completely miss the fact that seven birds went extinct. Yet, I write it both in the text and in the accompanying endnote.

> Lomborg takes particular exception to projections of massive extinction that started with Norman Myers's 1979 estimate that 40,000 species are being lost from the globe every year. There is some justification for this objection: Myers did not specify the method of arriving at his estimate. Nevertheless, he deserves credit for being the first to say that the number was large and for doing so at a time when it was difficult to make more accurate calculations. Current estimates are usually given in terms of the increases over normal extinction rates, which is preferable in that it is not necessary to assume a figure for the total number of species on the earth. That science does not know the total number of species does not prevent an estimation of extinction rates. Lomborg cynically dismisses the use of multiples of normal rates as being done because it sounds more "ominous" rather than recognizing the altered approach as an improvement in the science.

Here Lovejoy essentially says that Myers did not have scientific basis on which to claim his 40,000 species loss per year guesstimate, but nevertheless it was good that he said it. This is not how I understand good science.

Moreover, it is astounding that Lovejoy does not feel any need to confront the fact that he himself in the *Global 2000* report from 1980 estimated about 15-20% of all species would have died in 2000 (Lovejoy 1980:331).

I do agree with Lovejoy that since we don't know the absolute number of species, we have to talk in size-independent terms. But for public discussion I disagree with the use of multiples of natural extinctions and suggest instead using percentage of number of species, mainly because most non-biologists have no sense of the 'natural extinction rate' and therefore not able to assess the overall damage of a rate 1,500 times the natural extinction rate. Such understanding comes much more easily with such description as 'we are losing 0.7% of all species within the next 50 years.' Therefore, I do not "cynically" dismiss these rates but argue that the percent-terms are more easily comprehensible.

> Estimates of present extinction rates range from 100 to 1,000 times normal, with most estimates at 1,000. The percent of bird (12), mammal (18), fish (5) and flowering plant (8) species threatened with extinction is consistent with that estimate. And the rates are certain to rise—and to do so exponentially—as natural habitats continue to dwindle.

This is the end of Lovejoy's biodiversity discussion. Here, two things are important to note. First, Lovejoy does not actually challenge my central prediction of 0.7% species loss over the next 50 years, which is in line with the UN prediction of 0.1-1%/50 years. This being the central point of the biodiversity chapter, I find the absence of any challenge greatly encouraging, though one might argue it should have been mentioned in the Scientific American piece.

Second, Lovejoy ends by telling us that it will get much worse, as natural habitats continue to dwindle. This of course is a question of whether we believe that forests (especially tropical forests) will continue to be cut down, and the only long-run estimates that we have, come from the IPCC scenarios till 2100. Here, it is expected that inhabitants in the developing countries (where most of the tropical forests lie) will be *at least* as rich in 2100 as we are today (IPCC 2000b, SE:281). This of course also means that they are unlikely not to value nature much more (at least as much as we do today), that they will be less likely to go out to clear tropical forests for low-productivity agriculture (if you are rich you will have much better jobs) and they will be rich enough to keep or even regrow their forests. This is why *all but one* IPCC scenario expects there to be *more* forest in the world by 2100 (IPCC 2000b, SE:283). This then seems to indicate that Lovejoy's long-term it-will-get-much-worse expectation is unfounded.

> The consideration of acid rain in a separate chapter is equally poorly researched and presented. Indeed, the research is so shallow that almost no citation from the peer-reviewed literature appears. Lomborg asserts that big-city pollution has nothing to do with acid rain, when it is fact that nitrogen compounds (NOx) from traffic are a major source. His reference to a study showing that acid rain had no effect on the seedlings of three tree species neglects to mention that the study did not include conifer species such as red spruce, which are very sensitive. There is no acknowledgment of the delayed effects from acid rain leaching soil nutrients, particularly key cations. He confounds tree damage from air pollution 30 to 60 years ago with subsequent acid rain damage and makes an Alice-in-Wonderland statement that the only reason we worry about foliage loss is "because we have started monitoring this loss." It is simply untrue that "there is no case of forest decline in which acidic deposition is known to be a predominant cause." Two clear-cut examples are red spruce in the Adirondacks and sugar maple in Pennsylvania.

Lovejoy takes me to task for claiming that big-city pollution has nothing to do with acid rain. Yet his evidence is that the reverse is true (that acid rain has something to do with big-city pollution, since about 20% of the NO_x comes from big city traffic). Moreover, my statement was clearly directed against a shrill worry that acid rain would act as a pollutant and kill us: "In cities all over the Earth, people are being suffocated – or dying" because of acid rain, which is simply incorrect.

Lovejoy claims I confound tree damage from air pollution of old with acid rain, but this seems disingenuous because all I do is to point out that research shows that foliage loss was equally high 30-60 years ago as today. This is also why the alleged Alice-in-Wonderland statement might not be so outrageous, and moreover it is not my statement but the statement of very competent Norwegian acid rain researchers. Also, note that Lovejoy does not supply any alternative reference or data, only smears the argument by calling it Alice-in-Wonderland.

Finally, in saying that it is flatly untrue that "there is no case of forest decline in which acidic deposition is known to be a predominant cause," Lovejoy seems to neglect to mention that the quote is not mine but the conclusion of NAPAP, the official American acid rain project (National Acid Precipitation Assessment Program), which is the world's biggest, longest and most expensive study of acid rain; it spanned most of a decade, involved about 700 scientists, and cost half a billion dollars. It would seem that a little more than "simply untrue" would be needed for Lovejoy to counter this conclusion.

> The chapter on forests also suffers from superficial research and selective use of numbers. Lomborg starts by displaying Food and Agriculture Organization (FAO) data from 1948 to 2000. The FAO began by just reporting sums of "official data" furnished by governments (such data are notoriously uneven in quality and frequently overestimate forest stocks). Subsequently, the FAO adopted so many different definitions and methods that any statistician should know they could not be used for a valid time series.

Lovejoy write that I make selective use of the numbers, but neglects to say that this is the *only* long-run data series, and that I actually point out that these data series are uncertain (SE:111): "Data availability is poor but by far the best available." Moreover, this very data series is typically also used by the experts discussing the state of the world's forests, as in e.g. Michael Williams forest chapter in Cambridge University Press *Changes in Land Use and Land Cover*.

> Lomborg's discussion of the great fire in Indonesia in 1997 is still another instance of misleading readers with selective information. Yes, the WWF (World Wide Fund for Nature) first estimated the amount of forest burned at two million hectares, and Indonesia countered with official estimates of 165,000 to 219,000 hectares. But Lomborg fails to mention that the latter were not in the least credible and that in 1999 the Indonesian government and donor agencies, including the World Bank, signed off on a report that the real number was 4.6 million hectares.

Let me just recount the passage from the book (SE:116):

> "Finally, we heard a great deal about the forest fires in Indonesia in 1997, which for months laid a thick layer of smog over all of Southeast Asia from Thailand to the Philippines. The fires constituted a genuine health problem and with a total cost of almost 2 percent of GDP had appreciable economic impact. However, they were also exploited as a means to focus attention on deforestation. The WWF proclaimed 1997 as "the year the world caught fire" and their president, Claude Martin, stated unequivocally that "this is not just an emergency, it is a planetary disaster." Summing up, WWF maintained that, "in 1997, fire burned more forests than at any other time in history."
>
> This is not the case, however. In their report, the WWF estimated that the fires in Indonesia involved 2 million hectares, despite the fact that this is higher than any other estimate cited in the report. Although the 2 million hectares are mentioned constantly, it is only well into the text that it becomes apparent that the figure comprises both forest and "non-forest" areas. The official Indonesian estimate was about 165,000-219,000 hectares. Later, satellite-aided counting has indicated that upwards of 1.3 million hectares of forests and timber areas may have burnt. The independent fire expert Johann Goldammer said that "there is no indication at all that 1997 was an extraordinary fire year for Indonesia or the world at large."

Here, again, the issue is whether this was 'the year the world caught fire.' Moreover, the two million ha of forest burnt turned out in *their own* report to be both forest and 'non-forest' areas. I clearly do not accept the Indonesian estimate and do also say, that satellite pictures indicates upwards of 1.3 million ha, a number which Lovejoy fails to mention in the text. I am surprised that Lovejoy will use as evidence a non-peer-reviewed report (which he does not give any references to) which Indonesia apparently along with donor agencies signed on to after the Asian crisis of 1998 – it strains the imagination that a loan and aid package should have been opposed by an economically weak Indonesia, because of a number in a tie-in report.

> From the very outset—his introductory chapter—Lomborg confuses forests and tree plantations. In criticizing a WWF estimate of loss of "natural wealth," he implies that the only value of forests is harvestable trees. That is analogous to valuing computer chips only for their silicon content. In fact, the metric the WWF used includes natural forests (because of their biodiversity) and omits plantations (because of their general lack thereof).

It is curious how in a critique of my laxness with references that Lovejoy should claim I confuse forests and tree plantations. Actually, I do address that issue here (SE:115-6):

> "Similarly, many allege that although forest cover has remained constant, this is because we have less natural forest and more plantations. The old natural forest has a wealth of species, while plantations consist of genetically identical trees which support very few other plant and animal species. This, of course, is an offshoot from the general biodiversity argument. But for one thing it is not obvious that plantations reduce overall biodiversity. Certainly, they do have fewer species locally, but precisely because the purpose of plantations is to produce masses of wood, they reduce the economic pressure on other natural forests. As a result, these forests are better shielded, can support higher biodiversity or become better recreational areas for

> humans; 60 percent of Argentina's wood is produced in plantations which constitute just 2.2 percent of the total Argentinean forest area, thus relieving the other 97.8 percent of the forests. For another thing, plantations are typically claimed to be huge. WWF states that plantations "make up large tracts of current forest area." Of course, words such as "large tracts" are vague, but according to FAO, plantations make up just 3 percent of the world's forest area."

It is also interesting if Lovejoy really wants to commit to an idea, which his old organization (WWF) actually abandoned some two years because it became too obviously wrong, namely the decline of the worth of the biosystem. Let us just see the issue from the book (SE:17):

> "Finally, WWF uses among other measures these forest estimates to make a so-called Living Planet Index, supposedly showing a decline over the past 25 years of 30 percent – "implying that the world has lost 30 per cent of its natural wealth in the space of one generation." This index uses three measures: the extent of natural forests (without plantations), and two indices of changes in populations of selected marine and freshwater vertebrate species. The index is very problematic. First, excluding plantations of course ensures that the forest cover index will fall (since plantations are increasing), but it is unclear whether plantations are bad for nature overall. Plantations produce much of our forest goods, reducing pressure on other forests – in Argentina, 60 percent of all wood is produced in plantations which constitute just 2.2 percent of the total forest area, thus relieving the other 97.8 percent of the forests. While WWF states that plantations "make up large tracts of current forest area," they in fact constitute only 3 percent of the world's total forest area.
>
> Second, when using 102 selected marine and 70 selected freshwater species there is naturally no way of ensuring that these species are representative of the innumerable other species. Actually, since research is often conducted on species that are known to be in trouble (an issue we will return to in the next chapter, but basically because troubled species are the ones on which we need information in order to act), it is likely that such estimates will be grossly biased towards decline.
>
> Third, in order to assess the state of the world, we need to look at many more and better measures. This is most clear when WWF actually quotes a new study that shows the total worth of the ecosystem to be $33 trillion annually (this problematic study estimating the ecosystem to be worth more than the global production at $31 trillion we will discuss in Part V). According to WWF, it implies that when the Living Planet Index has dropped 30 percent, that means that we now get 30 percent less from the ecosystem each year – that we now lose some $11 trillion each year. Such a claim is almost nonsensical. Forest output has not decreased but actually increased some 40 percent since 1970. And the overwhelming value of the ocean and coastal areas are in nutrient recycling, which the Living Planet Index does not measure at all. Also, marine food production has increased more than 60 percent since 1970 (see Figure 58). Thus, by their own measures, we have not experienced a fall in ecosystem services but actually a small increase."

It is fine, if Lovejoy wants to accept the ill-documented and abandoned WWF idea that we are actually loosing some $11 trillion each year partly due to forest loss, but since this does manifestly not come from forest products, it would seem appropriate if he were to attempt to document how this could actually be the case with a loss of biodiversity at the level of 0.014% per year.

> The central question of the book— Are things getting better?—is an important one. The reality is that significant progress has been made in abating acid rain, although much still needs to be done. And major efforts are under way to stem deforestation and to address the tsunami of extinction. But it is crucial to remember that whereas deforestation and acid rain are theoretically reversible (although there may be a threshold past which remedy is impossible), extinction is not. A dispassionate analysis, which Lomborg pretends to offer, of how far we have come and how far we have yet to go would have been a great contribution. Instead we see a pattern of denial.

That we should see a pattern of denial is exactly what the rest of Lovejoy's text should have established. To that extent he does not seem particularly successful.

> The pattern is evident in the selective quoting. In trying to show that it is impossible to establish the extinction rate, he states: "Colinvaux admits in *Scientific American* that the rate is 'incalculable, ' " when Paul A. Colinvaux's text, published in May 1989, is: "As human beings lay waste to massive tracts of vegetation, an incalculable and unprecedented number of species are rapidly becoming extinct." Why not show that Colinvaux thought the number is large? Biased language, such as "admits" in this instance, permeates the book.

Let us just see the text (SE:254):

"The issue of biodiversity resembles the classic battle between model and reality. The biologists acknowledge that there is a problem when it comes to the figures. Myers says that "we have no way of knowing the actual extinction rate in the tropical forests, let alone an approximate guess." Colinvaux admits in Scientific American that the rate is "incalculable." Even so, E. O. Wilson attempts to put a lid on the problem with the weight of his authority: "Believe me, species become extinct. We're easily eliminating a hundred thousand a year." His figures are "absolutely undeniable" and based on "literally hundreds of anecdotal reports.""

Here it is evident that I am trying to establish the fact that the vast extinction numbers are unsupported by evidence or empirically validated theory. Of course, Lovejoy would like me to quote that Colinvaux really does believe that the number is large, but this is a personal and unsubstantiated point. If someone says "Nixon resigned in 1974 and that was a great loss to the nation," Lovejoy's requirement that I can only quote the scientific statement when also quoting the personal conviction is equivalent to requiring me not only to quote that Nixon resigned in 1974 but also that it was a great loss to the US.

In addition to errors of bias, the text is rife with careless mistakes. Time and again I sought to track references from the text to the footnotes to the bibliography to find but a mirage in the desert.

Without references, this is an impossible critique to deal with – of course Lovejoy could have attempted to contact me (Scientific American did so on Holdren's catalyzing/electrolyzing water).

Far worse, Lomborg seems quite ignorant of how environmental science proceeds: researchers identify a potential problem, scientific examination tests the various hypotheses, understanding of the problem often becomes more complex, researchers suggest remedial policies—and *then* the situation improves. By choosing to highlight the initial step and skip to the outcome, he implies incorrectly that all environmentalists do is exaggerate. The point is that things improve *because* of the efforts of environmentalists to flag a particular problem, investigate it and suggest policies to remedy it. Sadly, the author seems not to reciprocate the respect biologists have for statisticians.

Of course, I do tackle this process, and moreover, Lovejoy's simple process is only sometimes and somewhat correct. Take for instance the issue of air pollution, where the air has gotten ever cleaner in London since the late 1800s. Here is the issue for the most important pollutant, particulate pollution (SE:169-70):

"The reason for the dramatic fall in particle levels is partly that the emission of SO2 causing much of the particle pollution has fallen dramatically – in the EU by about 50 percent since 1980 and in the US by about 37 percent since 1970. This has been achieved by reducing consumption of fossil fuels, especially high-sulfur coal, by using smoke scrubbing equipment on power plant smokestacks and by increasing energy efficiency.

The political decision to limit sulfur emissions is closely linked to the question of acid rain, which was very much on people's minds in the 1980s. The fear of acid rain, which we will look into later, proved to be grossly exaggerated, although the SO2 reduction efforts did turn out to be reasonable because they helped reduce the particle pollution.

However, reductions in urban areas have several other causes. Historically, a move away from siting power plants in urban areas and the use of taller smokestacks were two of the primary causes of pollution reduction. At the same time we no longer use coke ovens and we have reduced our dependence on oil central heating, having instead changed to natural gas and district heating. Finally, cars pollute much less than they used to, partly because of catalytic converters but also because diesel vehicles now use low-sulfur diesel oil. However, compared to gasoline cars, diesel cars pollute much more in terms of particulate matter – in the UK, although diesel cars make up only 6 percent of the total car park, they contribute 92 percent of all vehicle emissions. Thus, a marked increase in the use of diesel cars could slow the decline in particulate emissions

Specialist literature has contained a lot of discussion about the degree to which legislation has been crucial, or at least important, to the reduction of air pollution. Many studies have – perhaps surprisingly – not been able to document any noteworthy effect. Analysis of the British Clean Air Act of 1956 shows that while pollution has, of course, fallen, the difference between the rate of fall before and after 1956, or the difference between cities that did or did not have pollution plans, is not discernible. "It seems likely that in the absence of the Clean Air Act of 1956 substantial improvements in air quality would have occurred anyway." The explanation is to a high degree to be found in improved products and technology for industry and the home.

In a study of three US cities, it was found that the mandated pollution control had an effect, but that the effects of regulatory control "generally have been overshadowed by the effects of economic changes, weather, and other factors." Generally it is probably true to say that regulation is one of the reasons for the reduction of pollution but that other, technological factors also play a major role.

In conclusion, it is worth emphasizing that particle pollution in terms of cost to humanity is by far the most important air pollutant and consequently (since air pollution makes up about 96 percent of all social benefits stemming from EPA regulation) by far the most important pollutant of all. And here the conclusion is unambiguously clear. Our most substantial pollution problem has been drastically reduced."

Thus, it is only partially correct that air quality improved because of regulation, and this was exactly the point in my text. Yet, even within regulation, it is incorrect, as Lovejoy seems to do, to accord all responsibility of improvement to environmentalists, since this entirely ignores all the scientists and politicians working hard on practical solutions to the very real pollution problems.

Finally, and ultimately much more important, is the fact that things are improving and this runs counter to the general doom-and-gloom message which runs through public discourse. I'm glad that Lovejoy in general agrees to this, but of course it also further undermines the basic thrust of the Scientific American critique.

Thomas Lovejoy is chief biodiversity adviser to the president of the World Bank and senior adviser to the president of the United Nations Foundation. From 1973 to 1987 he directed the World Wildlife Fund–U.S., and from 1987 to 1998 he served as assistant secretary for environmental and external affairs for the Smithsonian Institution in Washington, D.C.

MORE TO EXPLORE

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