

Dr. Hanne Koktvedgaard
The Danish Committees on Scientific Dishonesty

20th May 2002

Your reference 612 – 02 – 0004

Dear Dr. Koktvedgaard and colleagues:

Thank you for your letter of the 1st May offering us the chance to discuss Lomborg's replies to our concerns. The package that accompanies your letter contains four documents:

1. A list of documents dated 1st May 2002.
2. A letter from Lomborg dated April 25th 2002 that appears is a partial response to the covering letter that we sent you.
3. A unnumbered 6 page response to the review of *The Skeptical Environmentalist* that we published in *Nature*.
4. A numbered 32 page document that consists of four replies to the four articles that appeared in *Scientific American* critical of Lomborg's book.

In the interests of your time, we will address only our major concerns. We will certainly solicit comments from our colleagues in other fields on Lomborg's rebuttal in *Scientific American* (and the associated comments on the web). However, it is unlikely we will have these to your committee in time. Please remember that the four critical papers in that journal tackled only four of the chapters in Lomborg's book.

This document contains three responses. The first is Stuart Pimm's response to these documents, the second is Jeff Harvey's, and the third is John Rennie's comments in *Scientific American* to Lomborg's rebuttal there. While Harvey and Pimm have reviewed each other's comments and have minimized some overlap, time prevents them from combining their responses into one seamless document.

1. Stuart Pimm's response to the documents

1. There is no response to many of the most serious charges.

In the list of documents that you provide, item 1 indicates that you could not originally open the attachments we sent on March 15th. We apologize for the vagaries of the internet. However, we understand from your records that you did receive them eventually for item 2 is a list of the 7 documents.

We are quite surprised that Lomborg makes no attempt to rebut the detailed and highly critical comments of 6 of these 7 documents. (One might argue that Dr. Rennie's comments are part of Lomborg's response to the *Scientific American* package. If so, then merely 5 of the 7 documents are unanswered.)

There is no point in repeating what you can already read, but we would like to make two key points in noticing Lomborg's failure to respond. The first is that those who raise these issues are senior, well-respected scientists: their comments have to carry considerable weight. The second is that the charges they raise are extremely serious.

We will present just two examples of unanswered charges. The first is from document 3, by Professor Jerry Mahlmann, who wrote:

"In effect, he gives some scientifically dubious skeptics arguments an unearned degree of physical and statistical acceptance. It is almost as if he truly believes that the top climate scientists in the world are somehow, mindfully or mindlessly, stacking the deck so as to exaggerate estimates of just how much the climate will warm up for a given amount of greenhouse gases added to the atmosphere. ... In my own rethinking and re-evaluating of what we do and do not know about this critical question, I see little basis for his allegations of systematic bias. I have frequently, however, seen both climate-science skeptics and policy advocates commit logical errors in preferred directions. In real science, there is no place for manipulative biases. In real science, uncertainty "just is." When better information becomes available, the best estimates of uncertain quantities are adjusted upward or downward after careful evaluation of the new evidence. To my considerable disappointment, the author appears to have spent little time evaluating intellectually flawed and/or inconsistent arguments before reaching conclusions that are unlikely to be correct. He appears to have abandoned his admitted uncritical "Greenpeace bias" for a similarly unscientific "skeptics bias." He and his book are thus likely to be dismissed as being fatally contaminated by the same kinds of bias he claims to be correcting."

Professor Mahlmann then continues in the review "the scientific credibility of Lomborg's analyses and assertions" in the sections that follow (to which we refer you for details).

The second example comes from Professor Peter Gleick, who writes:

There is nothing original or unique in Lomborg's book. ... What is new, perhaps, is the scope and variety of errors he makes. I ... ended up reading almost the entire thing—more than 350 pages of small print and many of the 2900 footnotes. It became a game similar to “Where's Waldo?” ... Waldo is a popular cartoon character... Tiny images of Waldo are carefully hidden in large pieces of colorful artwork with hundreds or thousands of small cartoon figures in complex cartoon landscapes. The goal is to find Waldo.

In *The Skeptical Environmentalist*, “Waldo” became a series of conceptual errors, misunderstandings, and data problems. As I turned each page, the surprise was which Waldo (or Waldos) I would find next. There was no shortage. Some were trivial; others were dramatic in their scope and implication.

As with Professor Mahlmann, Professor Gleick then continues to describe these flaws in considerable detail. Much the same pattern follows in the other 4 papers. The range of accusations is astonishing. At the most general level, Lomborg does not answer Dr. Burke's stinging charge that no environmental organization believes what Lomborg asserts they do in his “litany.” (Lomborg's book is based on the premise that this litany is wrong.) At an entirely different scale are the detailed refutations of Lomborg's statements about the loss of biological diversity.

Every one of the 6 unanswered papers has numerous serious charges, yet Lomborg is silent on what he thinks of them. At least under British law, we understand that silence is taken to mean assent. That is, in not answering these charges, we assume he agrees with them. Whatever the law says, Lomborg's silence in rebutting these internationally known critics will be taken by the scientific community as evidence of his inability to do so.

We now turn to the few responses that Lomborg does provide.

2. General patterns in the response.

(a) Extensive and unrepentant bias in respect for sources.

A pervasive feature of all Lomborg's critics is his selective use of references. But there is also a pervasive bias in how he treats even those highly selective sources he does quote. Consider first the contrast between how he treats those whose views he wants to support and those he wishes to denigrate.

In his response to our *Nature* review, Lomborg writes (page 1) of one of his key sources, Professor Julian Simon, a man whose references appear far more times in the book than do all the papers in the internationally prestigious journal *Nature*.

“This is a criticism of Simon (which to a certain extent is correct — he did make some outrageous claims.”

Indeed, he did! But one would never know this from reading *The Skeptical Environmentalist*. Indeed, in the paragraph Lomborg then quotes from his book, one gets the impression that Professor Simon is a paragon of virtue

“a surprisingly large amount of his points stood up to scrutiny...”

There is no mention of the outrageous claims. (These include his ludicrous views on the expansion of the universe, his estimates of extinction rates that are four orders of magnitude smaller than all others, and so on.)

Perhaps Lomborg is generally kind to his sources? Far from it. He goes on to intimate that Professors Wilson and Ehrlich support a project that most readers would find offensive and ridiculous. Yes, Wilson and Ehrlich (and Pimm for that matter) support the Wildlands Project; but no the Wildlands Project does not do the ridiculous things that Lomborg asserts it does (or, indeed, anything like it).

We have raised this issue before (including in the Danish press) and are frankly offended by Lomborg’s refusal to acknowledge his mistakes here. *His response is typical of his response to so much of the criticisms levelled at his work. He simply states that someone else said so.* (In this case, a news article). Our accusation stands: what Lomborg says about Ehrlich and Wilson is a falsification.

Simply uncritically stating what a selected source says is undeniable bias when there is abundant documentation that others say something different. In this case, there is a larger bias —Lomborg impugns Ehrlich and Wilson while honouring Simon.

Lomborg’s treatment of Professor Colinvaux is equally egregious: quotes him out of context, with the implication that we do not know how many species are going extinct.

“Colinvaux admits in *Scientific American* that the rate is incalculable” (Lomborg’s reply to *Scientific American* page 31, first paragraph)

Dr. Lovejoy’s article in *Scientific American* gives the full context of Colinvaux’s text.

Lomborg continues in the second paragraph:

“...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.”

Well, that’s exactly what Professor Colinvaux *does* think: Here’s what he actually wrote:

“As human beings lay waste to massive tracts of vegetation, an incalculable and

unprecedented number of species are rapidly becoming extinct.”

Colinvaux does not “admit” anything; indeed, his actual text is about as strong a statement of belief that the “number is large” as one could imagine!

That bias continues with Professor Norman Myers whose estimates of extinction rates Lomborg derides. He tars Lovejoy with the same brush, writing at the top of page 28 of the reply to *Scientific American*

“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.”

This is an outrageous slur.

Lovejoy certainly did make that estimate in 1980; the error is in Lomborg’s assertion that Lovejoy has not confronted it. Twenty-two years ago, no one had much of an idea about how long critically endangered species lasted in the fragments of their habitats that remained after (for example) deforestation. The most famous study of all to resolve this issue, the subject of many papers in international journals, TV documentaries, magazine articles, and so on was established at that time north of Manaus in Brazil. It was for establishing that project, developing it, leading it, that Lovejoy — yes Lovejoy — shared the internationally prestigious Tyler Prize last year.

Lomborg repeatedly delves into old literature to find outdated statements that serve to impugn those highly respected scholars he wishes his readers to ignore. We would imagine that Nils Bohr in his early days probably wrote some things about the structure of the atom about which he changed his mind in later years.

There is a larger sense in which Lomborg besmirches scientists. Much as he might protest to the contrary, he starts with The Litany — at best a gross caricature of what environmental organizations believe (and Tom Burke is probably right in denying even that) and makes fun of it. Having done so, he makes no effort to distinguish how serious scientists differ from that Litany.

Nobel Laureates and those winning comparably prestigious prizes (including Ehrlich, Wilson, Myers, and Lovejoy) are not perfect and most certainly not to be believed uncritically. Nonetheless, they have attained their prestige by the most careful scrutiny of their work. Lomborg shows a repeated tendency to associate these scientists with views he wants to ridicule, even to the extent of grossly misrepresenting what they have written. At times, he simply dismisses everyone with whom he disagrees arguing that they chose their opinions to ensure funding “There are many grants at stake,” he writes.

(b) Having one's cake and eating it too

We are not sure how well this idiom translates in to Danish, but Lomborg repeatedly protests his innocence by pointing to his agreeing with his critics.

Thus as he points out at the top of page 4 of his 25th April letter, he does have a footnote (48) that says:

“The rate of extinction today is hundreds, if not thousands, of times higher than the natural background rate.”

Exactly so. Yet, the chapter on biodiversity starts by excoriating Myers and his estimate of 40,000 extinctions per year. This is a number he so often claims cannot be calculated. At the time when Myers made his estimate, a total of 40 million species was not thought as unlikely (and many still do favour such high estimates). So what does footnote 48 say? Lomborg stretches the estimate of species lifetimes in May et al. to its very limit, but does concede they chose a “lower lifespan.” Taking that estimate, and combining it with the well-accepted estimates of present extinction rates of about 1,000 extinctions per million species per year, one obtains 40,000 extinctions per year from 40 million species.

However we may quibble with these numbers, the point is that Lomborg in footnote 48 concedes that extinction rates are extraordinarily high. He might have even said that “as human beings lay waste to massive tracts of vegetation, an incalculable and unprecedented number of species are rapidly becoming extinct.” (You will recall that this is actually what Colinvaux said.)

This is a repeated pattern of writing extremely critical material in the main body of the text — and especially early in the book and each chapter — and then pretending that he agrees with others in the footnotes. Claims of high extinction rates are a major part of his often derided Litany. Having deride high estimates and those who make them, he then proceeds to insert a footnote that broadly agrees with high estimates and claims to show no bias.

Another example involves deforestation, which appears as the next example.

(c) The selective reporting remains.

“I have tried to present all the facts” Lomborg assures us. He doesn't even come close.

The most blatant example of this appears on page 5 on his April 25th letter, where we accuse him of “deceptive use of statistical methods.” The paragraph leads off:

“Globally, forest cover has remained remarkably stable over the second half of the twentieth century. ... global forest cover increased from 30.04% of the global land area in 1960 to 30.89% in 1994. In the newest forest study, ... a new estimate of forested area from 1990 to 2000, showed a small decline from 29.5 to 28.8 (% of global land

cover)”

This is a striking, emphatic, and prominent statement, one that can leave the reader with no doubt as Lomborg’s insistence that “things are getting better.” It’s also completely false, as Lomborg himself admits!

Elsewhere — yet another footnote 801 — he accepts the UN estimates of 0.5% loss of original area per year. Over a 34 year span (1960 to 1994) that would be a forest lost of 17% of original area! There is no discussion of the spectacular discrepancy between that figure and the increase in forest cover quoted above.”

Lomborg then attempts to justify why he ignores an incredible literature on deforestation. He asks (page 3, April 25th)

“but at least one could want somewhat better sources than just blanket references to *Science* and *Nature*. Certainly, it would also seem appropriate to indicate why citing the UN figures would constitute selective and hidden discarding of unwarranted results.”

There is nothing that so damns Lomborg that this extraordinary statement.

It is an explicit acknowledgement of his selectivity. No, there is nothing wrong in *including* UN figures. There is everything wrong with using *only* these figures. There is everything wrong selecting one set of figures when multiple estimates are available especially when those other estimates disagree. That is what is meant by “selective” — one selects what one wants to support an argument, and ignores what is inconvenient. Lomborg simply does not bother to check what others have to say about a topic. Julian Simon provides him with more citations than the journal *Nature*! Why are *Nature* and *Science* so important? They have the most brutal review process: we know, Pimm serves on the board of reviewing editors for *Science*, Harvey had a similar job at *Nature*. Their intensive review processes are precisely why scientists would check with them on a given topic, for no other two sources would guarantee such carefully scrutinized research. Lomborg cheerfully believes just his UN figures (well, at least one small subset of them) and chooses not to look anywhere else and carefully avoids the places where he might find — for example — the most detailed analysis of tropical deforestation.

How difficult is it to be non-selective? It took me just 5 minutes to find the following articles about tropical forest loss published within the last 5 years.

R. O. Lawton, et al. *Science* 2001 October 19; 294: 584-587.

Robert Bonnie, et al. *Science* 2000 June 9; 288: 1763-1764.

Mark A. Cochrane, et al. *Science* 1999 June 11; 284: 1832-1835

José Paulo Silveira, et al. *Science* 2001 June 1; 292: 1651-1654

Bernice Wuehrich *Science* 2000 July 7; 289: 35-37.

William F. Laurance, et al. *Nature* 404, 836 (20 Apr 2000)

F. Siegert, et al. *Nature* 414, 437 - 440 (22 Nov 2001)

Stuart L. Pimm *Nature* 393, 23 - 24 (07 May 1998)

Daniel Nepstad *Nature* 415, 476 (31 Jan 2002)

Daniel C. Nepstad, et al. *Nature* 398, 505 - 508 (08 Apr 1999)

These articles in themselves — and the references they contain — provide an abundant source of information to refute the notion that forest cover has remained “stable” over the last half century.

Many of Lomborg’s critics notice his almost total lack of scientific publications. This is relevant here. When one publishes in reputable journals, there is a requirement to amass the relevant evidence for and against one’s hypotheses. (The contrast to a lawyer presenting a case is obvious: science is not advocacy.) Lomborg shows throughout his book — and particularly in his responses to the case we have laid against him — that he simply does not understand this need for balance. He picks and chooses at will. He has no experience of what scientific publication requires.

Thus, the 32 page rebuttal is not a rebuttal at all.

Rather it is a tedious list of Lomborg answering well-established, well-published critics who have reviewed and assessed the full range of opinions, with statements of the form: “I wrote A, because reference B said it was that way.” What I — and all his other critics — find so completely outside of acceptable practice is this blind faith that this is academically acceptable. It isn’t. Unbiased, unselective science requires a good faith effort to present all the results. Choosing to write A, because B says so is selective when one can readily find that C, D, E, F, etc said something else, is exactly why so many scientists have so strenuously objected to Lomborg’s work.

One might overlook selecting one reference (D, for instance) if all the other references said much the same thing. Yet Lomborg ignores these other references, making no look for them in obvious places, and impugning the reputations of well-respected scientists on the few occasions that he does quote opposing views. In short, selectivity is bad; Lomborg is wilfully selective.

The examples of this include his curious philosophical justification that things are getting better and that percentages (not absolute numbers) tell us so. (Millions more children are starving, but it’s OK, he tells us, and provides pages of justification. It seems a damnable callous view in our opinion, but we are not philosophers.)

At times, the arguments drop into complex calculations out of which come entirely the wrong answers. On page 3 and 4 of the April 25th letter (and elsewhere) he tries to justify by tortuous calculation the estimate of losing only 0.7% of species in the next 50 years. We are not surprised that there is a source for that estimate. What is egregious is that there are dozens of books (Red Lists, prepared under the auspices of the International Union for the Conservation of Nature) that provide detailed lists of species. They suggest that >10% of species are likely to go extinct in the next half century. (And that is based on present, not future, human impacts.)

The selectivity mirrors Julian Simon, of course. He has merely taken Simon's selection of facts and largely made them his own. While he does indeed acknowledge this (in a footnote, of course) it is strikingly poor scholarship to add so little substance. (Below, Jeff Harvey has a more detailed discussion of the extent to which Lomborg follows Simon and others with little or no modification.)

There is an interesting example about presenting all views. It is a review of the estimates of the modern rates of species extinction. It is in *Science* and, naturally, Lomborg overlooks it as he does almost all of my work. (I was too young to be criticized by Simon until just before Simon's death, I publish many of my papers in *Science*, *Nature*, and *PNAS* and so miss out on Lomborg's criticism.) In the relevant figure, I do not ignore Simon's estimate, but place it there along with all the other estimates, showing it to be four orders of magnitude below those others.

2. Jeff Harvey's detailed response to comments of Bjorn Lomborg

Dr. Bjorn Lomborg has posted on his Web page a long response to the critiques that appeared in *Scientific American* of four of the chapters in his book, *The Skeptical Environmentalist*. This includes the responses in his defence (25 April) which I received yesterday. I would like the committee to note that many highly critical reviews of Lomborg's book have been published since November, some two months after the initial publication of TSE. In submitting our claim of scientific dishonesty, we included a sample of these, most of which (except for the reviews in *Nature* and *Scientific American*), Lomborg has ignored, in spite of the fact that each and every one contains substantial evidence that Lomborg is guilty of the aforementioned charges that we have levied against him. Moreover, please note that Lomborg felt obliged to use all of the submissions he received in response to the appeal for help he broadcast to a long e-mail list after the *Scientific American* critiques appeared.¹

A question of objectivity – or bias

I feel that it is important to point out that several of the individuals to which he personally wrote (e.g. Dr. David Wojick) are spokespersons for corporate-funded groups such as the *Greening Earth Society*, which is a coal industry lobby group. These groups continually disseminate non-peer reviewed material to the public and policymakers that downplay the seriousness of environmental problems. Furthermore, the industry-funded Competitive Enterprise Institute, one of the leading right-wing opponents of the Kyoto process (and, indeed of all efforts to reduce fossil fuel emissions to slow global warming) rolled out the red carpet for Lomborg when he visited Washington last autumn on his book tour. On October 4, 2001, a CEI/corporate-sponsored anti-Kyoto group, the Cooler Heads Coalition, hosted a congressional and media briefing for Lomborg at the U.S. capitol.

This brings into question his objectivity, since one would think that he would steer well clear of groups that clearly have a vested interest in maintaining a "business-as-usual" policy, and more so since most of the book is spent railing against the motives of environmental NGO's. In writing TSE, Lomborg has attempted to show that environmental scientists and NGO's have had an undue influence on public policy decisions. He excoriates the four critics in *Scientific American*, writing in his response there

¹ In a "Dear Sir or Madam" broadcast e-mail sent out by Lomborg on December 18, he wrote, *inter alia*, "Naturally, I plan to write a rebuttal to be put on my web-site. However, I would also love your input to the issues -- maybe you can contest some of the arguments in the SA pieces, alone or together with other academics. Perhaps you have good ideas to counter a specific argument. Perhaps you know of someone else that might be ideal to talk to or get to write a counter-piece."

“I believe many readers will have shared my surprise at the choice of four reviewers so closely identified with environmental advocacy.”

In contrast, he completely ignores the role that the much better funded corporate-funded lobby groups and think tanks have had on public policy. This is clear evidence that Lomborg is not impartial or may even have an agenda, and the fact that he has courted conservative interests for their support backs this up.

In responding to the critical essays published in *Scientific American*, it is also instructive that Lomborg apparently did not feel he could manage an adequate response by himself. (In this, at least, he was correct. But he clearly could not manage it with help, either.) Just as the book itself betrays a seeming inability of its author to discriminate sensible arguments from nonsensical ones, so also does his response to the various critiques suggest that Lomborg just tossed in, uncritically, whatever replies popped into his head or into his e-mail “in” box. Here, I directly respond to Lomborg’s comments of our complaint. I will then add additional evidence of scientific dishonesty.

Failure to acknowledge bias in interpretation

Lomborg has in my view unsuccessfully attempted to counter the points we made in March. His response is short, lacking in details and digresses time and time again from the thrust of our arguments, as Stuart Pimm has already shown. This is something that I have noticed repeatedly characterizes Lomborg. His typical defense is to deny the questioner and then to take the argument off in a new direction, ultimately avoiding having to admit his error. This was most recently evident when he was interviewed on the BBC programme, “HARDTALK”. At one point the interviewer, Tim Sebastian, asked Lomborg why he asserts, with no analysis whatsoever, that only the mildest [climate change] scenarios will happen and that the dangerous ones won’t happen, a point raised earlier by climate scientists Stephen Schneider and Jerry Mahlmann.

Lomborg evaded the issue by making an emphatic “no, no no”, thus denying what he clearly states in his book, and then switched the subject to a discussion of cost projections of adaptation versus prevention. Even here, Lomborg throws figures around randomly, stating that the costs of prevention would be 5-8 trillion dollars, then 8 trillion dollars (in his book, he says 5 trillion dollars, so Lomborg’s guesstimate includes an inflation rate of >50% in less than a half hour). These estimates, based on a single model by conservative business economist William Nordhaus, have been repeatedly criticized by other economists including John Quiggen and Clive Hamilton, but Lomborg ignores these economists and many others who just so happen to disagree with his own clear bias. Amongst other factors, the models of Nordhaus are flawed because they ignore the social (= external) costs of climate change e.g those borne on natural (‘unmanaged’ in Nordhaus’s jargon) ecosystems. But how can we ignore the costs on systems that

sustain us? Lomborg, in his response to Schneider's critique, claims that he ignores ecological consequences because as he states in his response to Schneider, he "wrote the book from a human perspective". But this is a complete smokescreen. Lomborg thus believes that the damage inflicted upon ecological systems by climate change amongst other anthropogenic factors must be inconsequential, because, he makes the following statements:

On page 251, he downplays the importance of ecosystems as providers of services because "they have no market". Furthermore, on page 115, he states that "one cannot argue that species [at risk from extinction] constitute an actual economic resource".

But this does not mean that humanity is somehow exempt from natural laws, and in the past 15 years or so we have begun to understand that ecosystems and the services they generate sustain us (Daily et al., 1997). Any evaluation of the "state of the planet", however elusive that may be, must acknowledge this fact. But Lomborg not only downplays the importance of biodiversity, *he actually claims that it is not important*. As John Rennie writes in his lengthy rebuttal to Lomborg in *Scientific American*:

But as a reading of Lomborg's chapter on biodiversity reveals, that passage does not merely question whether biodiversity is important—it argues that biodiversity is not. In less than a page, Lomborg trivializes the value of a biologically diverse environment, brushing past the most serious criticism that economic analyses of biodiversity generally underestimate its value because the majority of services that the environment provides are not represented in economic markets. It sets the context for minimizing the significance of species loss that persists throughout the rest of the chapter.

Many economists are also well aware that prices do not reflect ecologically damaging subsidies e.g. the price of food fails to reflect the environmental and ultimately social costs of pollution, soil degradation, and overuse of groundwater that are externalized from the costs of food production. Similarly, the price of oil fails to capture the costs of damage to ecosystems because of environmentally damaging extraction practices and the production of greenhouse gases that are affecting global climate. These points have been raised to him in debates and critiques but he brushes them aside.

Misleading use of data

With regards to the global net loss of forests, Lomborg has completely failed to address our point, that *FAO statistics are unreliable and were never meant to evaluate forest cover in the first place*. These are the words of the head of the United Nations Forest Resources Assessment Programme (FRA), who was contacted by Emily Mathews of the World Resources Institute after she first learned of Lomborg's inaccurate claims that world forest cover is generally

increasing. The FAO used their data primarily to measure the extent of agricultural cover, and not forests. This is exactly why the survey was discontinued in 1994. Most importantly *we are not challenging the UN figures but instead are supporting them*, based on the latest and most up-to-date FRA data. It is Lomborg who is distorting the UN data. Considering the profound uncertainty over the extent of forest loss in the first place, he astonishingly comes up with the figure of 4.172401e9 or 31.15% for forest cover (footnote 767) (footnote). However, FRA data show that the world lost 4.2% of its natural forest in the 1990's, and that most of this loss is accounted for by tropical forests, of which about 8.7% were lost in this decade (compounding significant losses from the 1970's and 1980's). I would like to highlight the fact that Lomborg also incorporates tropical forest loss as a measure of world forest loss, a ploy cleverly designed to mask the serious extent to which tropical forests are being cleared. Furthermore, we must also be point out that estimates of forest loss are conservative, because they ignore the effects of selective logging practices, fire, and other processes which have simplified forests to such an extent that they are incapable of supporting birds and other wildlife (*Birdlife International*, 2000). The FAO data even included many clear cuts in its estimates of forest cover, if they were planned for reforestation. However, this measures land use, and not habitat. Similarly, Lomborg fails to address the important question of quality: much of the world's forest cover consists of second growth forests (since much of the primeval old-growth forests have been cleared) and plantations, which are clearly different ecologically. Yet Lomborg summarily fails to address this critical point, instead preferring to rely on fuzzy quantitative data to support his simple hypothesis that the planet has lost little of its forests. He even states that "the world has lost only 30% of its forests", a clear value judgement which cannot be effectively evaluated without some qualitative analysis. However, in TSE we get none. Perhaps the clearest example of Lomborg's bias with respect to forests comes from his complete disregard for the summary of the 2000 UN FRA, which states:

"[In the 1990's], The world's natural forests continued to be lost or converted to other land uses at a very high rate".

In my view, he has completely and unsatisfactorily failed to address our criticisms with regards to forest cover and loss (see also comments by Pimm).

Failure to cite relevant studies which would lead to different conclusions

Similarly, Lomborg's reply on extinction rates is incorrect. Here, I will prove that Lomborg's estimate is at least 10 times less than even his calculations suggest. In his reply to Tom Lovejoy in *Scientific American*, Lomborg states that:

"I accept that we are causing species extinction at probably about 1,500 times the natural rate".

This figure, as it turns out is in line with what most experts in the field are saying. But his rate of 0.7% over 50 years is strikingly discordant with a background rate of extinction 1,500 times higher than the natural rate. This is why: using birds as an example, there about 10,000 species worldwide (actually several hundred less, but this rounded-up figure gives Lomborg the benefit of the doubt). Most authorities believe that the “shelf-life” (e.g. longevity) of a species is between several hundred thousand and several million years (papers by Raup and Jablonski make this point) with a good baseline average being around a million. If birds became extinct at natural background rates, we would expect to see one bird become extinct about every 100 years ($100 \times 10,000 = 1$ million years). However, we are now seeing birds disappearing at between 1 and several species per year (1 species = 100 times the background rate and 10 would = 1,000 times the background rate). With the increased pressure on tropical forests, this level is set to sharply rise, a point that is indisputable amongst experts. If Lomborg is correct, and we are losing species at the rate of 1,500 times the natural rate, then this would indicate that we would be losing 15 bird species per year (a level that may be approached soon). Over 50 years, this would = 750 species lost, which means $10,000/750 = 7.5\%$. This figure is more than ten times higher than Lomborg’s 0.7% estimate, *even though Lomborg himself agrees that extinction rates are perhaps 1,500 times higher than natural rates*. What we see here is part of Lomborg’s pattern of random incompetence: throw figures around without properly doing the math.

As is the case in many areas of his book (outlined by Stuart Pimm) Lomborg has selectively ignored or excluded many peer-reviewed studies that provide different conclusions from his own, and has intentionally failed to acknowledge this fact time and again when it is presented to him. Perhaps the most telling statements made by Lomborg occur at opposite ends of the book. Lomborg states on page xx (preface) that:

“I am not an expert as regards environmental problems”, and on page 327 that “I have tried to present all the facts”.

Whereas the first statement is an admission of honesty, the second is clearly not true. If it were, Lomborg would publicly admit that literally hundreds of peer-reviewed studies – published in significant journals such as *Nature*, *Science*, and *Proceedings of the National Academy of Sciences* - are indeed important and would certainly lead to different conclusions from the ones he presents in his book based on a narrow range of studies he has included on different areas of research. But Lomborg resolutely refuses to acknowledge the importance of these studies. This clearly shows that he is either an environmental expert after all, (although I find this almost impossible to believe considering his complete lack of contribution to the relevant fields of research), or else that he is not telling the truth because to do so would undermine his credibility and those presented in his book as “facts”.

In his reply to us, Lomborg attempts to defend the fact that he bases the biodiversity chapter on a chapter in the book, "The State of Humanity", written by the late Julian Simon and Aaron Wildavsky. He states that this fact is clearly stated in the footnotes, but this is just another smokescreen, since most readers are not going to painstakingly go through every minute detail that is buried there. That both Simon and Wildavsky are business economists and not biologists did not apparently enter into Lomborg's decision to use their chapter as a template for his chapter on biodiversity. But did not Lomborg state in his reply to our *Nature* review (page 1) that the whole project was initiated to check the validity of Julian Simon's arguments? If this indeed was the case, it is logical that he would distance himself from Simon's writings and focus on evaluating the empirical evidence from peer-reviewed literature. However, he repeatedly cites material from books edited by Simon to support his views, a practice that borders on plagiarism, and he makes the same claims that they do, although most of their arguments have already been publicly disproved in subsequent papers that Lomborg ignores.

For example, Lomborg states (pg. 254) that:

"In the US, the eastern forests were reduced over two centuries to fragments totaling just 1-2 percent of their original area, but nonetheless this resulted in the extinction of only one forest bird".

Lomborg's aim was to discredit the area-extinction models devised by Professor Edward O. Wilson that have been used for many years to estimate extinction rates. Lomborg's chapter and this example are more-or-less lifted straight and uncritically out of Simon/Wildavsky chapter in "The State of Humanity" (1994). A year after the Simon/Wildavsky piece was published, Stuart Pimm and Robert Askins published an article in the *Proceedings of the National Academy of Sciences* (*PNAS* 92, 9343-9347, 1995) which criticized the erroneous Simon/Wildavsky piece (the authors, both eminent conservation biologists, wrote their piece explicitly as a corrective). *PNAS* is hardly an obscure source. In examining the data, Pimm and Askins found that at any one time, a maximum of 52% of forest was lost and 4 species of endemic birds became extinct as a consequence, not 1 (with two more seriously wounded and barely hanging on at present). These results support, rather than refute, the area-extinction models Lomborg disparages.

Lomborg's only defense thus far has been to claim that he based his argument on an article by Daniel Simberloff (1990), and that his critics should be attacking Simberloff, and not him. But this is clearly not how science works. The very publication of the Pimm and Askins paper in such a prestigious source as *PNAS* itself is a corrective of Simberloff's paper.

Please bear in mind what Lomborg had said in his book: to reiterate, *that he has no expertise whatsoever in environmental science, and that he is trying to present all the facts*. Consequently he should have included the corrective paper as an

important reference. But it is excluded from TSE, along with many other papers that empirically support the area-extinction models and disagree with the non-peer reviewed Simon/Wildavsky piece that Lomborg bases his chapter on. (I would like the committee to also note that Lomborg first published the incorrect story of avian extinctions in North American birds in the 1998 Danish edition of TSE, and that he was subsequently corrected by Professor Jon Fjeldsa, Zoological Museum, University of Copenhagen. However, without acknowledging his error, he repeats exactly the same story in the English edition!)

In another example of selective bias, Lomborg argues that extinction rates in Atlantic coastal rainforests of Brazil are much lower than predicted by area-extinction models. He bases this on a chapter by Brown and Brown in a book edited by Sayer and Whitmore that was published in 1990. This book was one of the primary sources for Julian Simon in his book "The State of Humanity". However, in 1997 Thomas Brooks (then one of Pimm's graduate students) and Andrew Balmford published a scientific correspondence in *Nature* which challenged the Brown and Brown article and provided evidence that the number of critically endangered species supports the levels predicted by the species-area analyses (*Nature* 380: 115, 1996). But again, Lomborg has failed to cite this corrective, and repeats the same discredited argument to this day.

Lomborg's selective bias is also demonstrated in his chapter on acid rain (Chapter 16). On page 181, he states:

Likewise, the Danish daily *Politiken* recently wrote, briefly and to the point: "Sulfur in the atmosphere produces acid rain. And acid rain kills forests". Simple. But not borne out by the evidence.

But which evidence is Lomborg citing? He bases this exclusively on a single graph extracted from a chapter in Simon's "The State of Humanity", and on the 1990 findings of the heavily criticized National Acid Precipitation Assessment Program (created during the Reagan administration which was notoriously hostile to the phenomenon). But even here, Lomborg selectively omits piles of studies that contradict his conclusions (for example, the Hubbard Brook Research Foundation states that:

"Recent research shows that acid deposition has contributed to the decline of red spruce trees throughout the eastern United States and sugar maple trees in central and western Pennsylvania".

Dr. Robert Bruck of North Carolina State University has examined acid rain's effects on forest health and concludes that:

"Acid rain and cloud deposition are the primary culprits in the loss of spruce and fir forests".

The US Environmental Protection Agency, along with Environment Canada, report that acid rain remains a potent threat to the health of North American forests..

EPA: "Researchers now know that acid rain causes slower growth, injury or death of forests".

Canadian Acid Rain Assessment, 1997: "Acid deposition exceeds critical loads across large portions of eastern Canada.... as a result, acidification of these areas will continue, the biodiversity of aquatic ecosystems will remain under threat, and more damage to forest health and productivity will occur unless further deep cuts in sulfur dioxide and nitrous oxide emissions in Canada and the US are implemented".

Lomborg also failed to cite an updated 1998 appraisal from NAPAP, which reads:

"Sulfur and nitrogen deposition have caused adverse impacts on certain highly sensitive forest ecosystems in the United States. High-elevation spruce-fir forests in the eastern United States are the most sensitive. Most forest ecosystems in the East, South, and West are not currently known to be adversely impacted by sulfur and nitrogen deposition. However, if deposition levels are not reduced in areas where they are presently high, adverse effects may develop in more forests due to chronic, multiple decade exposure."

An even more up-to-date reference that comments on sugar maple declines in the northeast is "Acidic deposition in the northeastern United States: Sources and inputs, ecosystems effects, and management strategies," by C. T. Driscoll et al., *Bioscience*, vol. 51, pages 180-198.

All of these sources clearly contradict Lomborg's glib optimism. Bearing in mind his claim to be seeking the truth, while lacking any kind of expertise in areas he covers so superficially, why does he ignore the conclusions of researchers who have worked in the relevant fields of research for many years? Are these omissions accidental oversights, for which we would expect Lomborg to issue a retraction, or intentional and selective omissions? In either case, he is wrong, but he will not admit his errors publicly. One can only conclude that, in spite of doing no research whatsoever in the relevant areas, that he must either be an environmental expert, (which even he admits that he isn't), or else he is not telling the truth. I leave this up to the committee to decide, but I feel that beyond any reasonable doubt Lomborg is being intellectually dishonest.

I would like to conclude my response with the last several paragraphs from John Holdren's reply to Lomborg's rebuttal in *Scientific American*. It encapsulates the feelings of frustration that are felt by many scientists whose work has been seriously distorted and misrepresented by Lomborg in *The Skeptical Environmentalist*. Holdren states:

The practice of science, which includes the packaging of findings from science for use in the public-policy arena, is governed by an unwritten code of conduct that includes such elements as mastering the relevant fundamental concepts before venturing into print in the professional or public arena, learning and observing proper practices for presenting ranges of respectable opinion and uncertainty, avoiding the selection of data to fit pre-conceived conclusions, reading the references one cites and representing their content accurately and fairly, and acknowledging and correcting the errors that have crept into one's work (some of which are, of course, inevitable) after they are discovered by oneself or by others.

Most scientists follow this code of conduct as best they can out of self respect and respect for the integrity of science itself. For those for whom these considerations might not be quite enough, there is little that can enforce the code other than concern with the cumulative harm to one's reputation and standing that comes from one's colleagues' awareness of a pattern of infractions, or fear of the public denunciation by colleagues that may follow in the rarer instances of someone's descending into more massive and willful disregard of accepted standards. Of course, for the deterrent effect of potential denunciation by one's colleagues to work against such massive violations of the scientists' code of conduct, it is important that the denunciation should actually happen in those instances when, occasionally, the deterrent fails. If the issue involves science for policy, moreover, a clear and forceful denunciation has the further purpose of avoiding an extreme and poorly founded interpretation of the relevant science being credited in the policy debate as lying within the range of respectable scientific opinion.

Now, it is apparent from reading just the first few pages of *The Skeptical Environmentalist* that Lomborg proposes to make the case that not just environmentalists, but a considerable part of the heretofore respectable environmental-science community, have been misunderstanding the relevant concepts, misrepresenting the relevant facts, understating the uncertainties, selecting data, and failing to acknowledge errors after these have been pointed out – in other words, that the scientist contributors to what he calls “the environmental litany” (namely, that environmental problems are serious and becoming, in many instances, more so) have been guilty of massively violating the scientists' code of conduct. This would be interesting news indeed, if Lomborg could prove it. But reading further reveals that his attempt to do so is itself a richly populated pastiche of these very infractions. Every class of mistake of which he accuses environmentalists and environmental scientists who have contributed to the “litany” is in fact committed prolifically and indiscriminately in *The Skeptical Environmentalist* (except, of course, for refusing to acknowledge error – for this, one has to read his rebuttals).

That the responses of environmental scientists have conveyed anger as well as substantive content, then, ought to be understandable. Lomborg's performance careens far across the line that divides respectable even if controversial science from thoroughgoing and unrepentant incompetence. He has failed thoroughly to master his subject. He has committed, with appalling frequency and brazen abandon, exactly the kinds of mistakes and misrepresentations of which he accuses his adversaries. He has needlessly muddled public understanding and wasted immense amounts of the time of capable people who have had to take on the task of rebutting him. And he has done so at the particular intersection of science with public policy – environment and the human condition – where public and policy-maker confusion about the realities is more dangerous for the future of society than on any other science-and-policy question

excepting, possibly, the dangers from weapons of mass destruction. It is a lot to answer for.

3. John Rennie, editor in chief of *Scientific American*, writes in his reply to Lomborg's rebuttal in that journal:

Disappointingly, Lomborg has chosen to fill his print response with half-truths and misdirection. Perhaps in this brief space he felt that he could do no better, but critics of *The Skeptical Environmentalist* also find such tactics to be common in his book. He implies that he has been wronged in getting so little space; our 11-page set of articles is a response to the 515-page volume in which he made his case, and which was widely and uncritically touted in the popular media. (Long before our article, for instance, *The Economist* gave him four unanswered pages for an essay.) So far it is the scientists who are having a harder time getting equal space for their side. Anyone still interested in this controversy will find on www.sciam.com our original articles and Lomborg's detailed rebuttal of them, along with refutations to his rebuttal.

Lomborg and *The Economist* may call them "weak on substance," but our pieces echo identical criticisms that have been made in reviews published by *Nature*, *Science*, *American Scientist*, and a wide variety of other scientific sources—not venues where insubstantial criticisms would hold up.

Lomborg's stated proof that he understands the climate science is that he relies on the IPCC's report, but the argument of Schneider (and other climatologists) is of course that Lomborg picks and chooses aspects of that report that he wants to embrace and disregards the rest. Lomborg boasts that he isn't a global-warming denier, but how is that relevant? The criticism against him is not that he denies global warming but that he oversimplifies the case for it and minimizes what its consequences could be. The reference to Schneider's theories about global cooling reaches back three decades; all good researchers change their views as new facts emerge. How does this bear on the current debate except as personal innuendo?

As in his book, Lomborg repeats that the Kyoto Protocol would postpone global warming for only six years. This is an empty, deceptive argument because the Kyoto Protocol isn't meant to solve the problem by itself; it is a first step that establishes a framework for getting countries to cooperate on additional measures over time. The cost projections Lomborg uses represent one set of estimates, but far more favorable ones exist, too. Given that the additional antiwarming steps that might be taken aren't yet known—and so their net costs are impossible to state—it is premature to dismiss them as "phenomenally more expensive."

As Lovejoy's article and others have noted, Lomborg's simplistic treatments of biodiversity loss and deforestation are inappropriately dismissive of well-grounded concerns that those numbers could range far higher. (And why resurrect a claim in a paper that Lovejoy wrote 23 years ago when he and others have far more recent estimates?) Moreover, one problem of Lomborg's statistical methodology is that it tends to equate all items within a category regardless of how valuable or different the individual elements are. For example, there may be

more forest in 2100 than there is today, but much of that will be newly planted forest, which is ecologically different (and less biodiverse) than old forest.

When Lomborg restates the number of lost species as a percentage of total species, is he simply showing the true size of the problem or is he perhaps also trying to trivialize it? By analogy, in 2001 AIDS killed three million people, with devastating effects on societies in Africa and elsewhere. But that was only 0.05 percent of all humans. Which number is more helpful in setting a public health agenda for AIDS? The answer is neither, because numbers must be understood in context; Lomborg creates a context for belittling extinction problems.

Lomborg is being disingenuous when he protests that our authors did not even mention half his book. As our preface to the feature stated, we asked the authors to comment specifically on just four chapters. The flaws in those sections alone discredit his argument.

Environmental scientists are all in favor of setting priorities for action; Lomborg pretends otherwise because he disagrees with the priorities they set. Even if his effort to describe the “actual state of the world” (a naive goal, given the world’s complexity and the ambiguity of even the best evidence) is honest, his argument is not credible. And by sowing distrust of the environmental science community with his rhetoric, Lomborg has done a severe disservice not only to those scientists but also to the public he has misinformed.