April 9th 2002

To the Danish Committees on Scientific Dishonesty
att. Hanne Koktvedgaard

Concerning: Complaint over Bjørn Lomborg

I hereby send my rejoinder to the reply from Bjørn Lomborg. As it appears, I have ot increased the number of points of complaint.

My complaint herewith consists of three letters that altogether make up ca. 60 pages + enclosures. That must be abundant. My expectation is that I shall not hereafter take any further action myself, and that the proceedings of the case go on. I will obviously like to receive brief messages along the way on how the case is proceeding.

Kind regards

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April 9th 2002

**CONCERNING COMPLAINT TO THE DCSD AGAINST BJØRN LOMBORG**

**Rejoinder to Lomborg's reply of March 22nd**

GENERAL REMARKS

We do not agree

I note that Bjørn Lomborg has chosen only to reply to a part of the complaint, the part dealing with deforestation. Lomborg purely rejects all points of complaint and does not make any concessions. By way of reply I must say that I maintain all points of the complaint. Thus, the case rests as contention against contention, and I must leave it to DCSD to weigh out the opposing arguments.

This situation of contention-against-contention has unfortunately characterized the entire debate right since Lomborg's first article in Politiken in January 1998.

A fair number of points of complaint

Lomborg requests that the complaint be limited to a fair number of points, and asks DCSD to assess what else he needs to account for.

I would of course request DCSD to decide that Lomborg must take a stand on the whole of my complaint. My basic contention is that all of Lomborg's writings, right from the first article in January 1998, are pervaded by dishonesty. On that background, it must be seen as modest that I have only included one paper as a concrete point, i.e., The Skeptical Environmentalist, and that I restrict myself to the following aspects herein:

Chapter 10 on forests: 8 pages
Issue of hunger in Africa: 1 page
Issue of extinct species in Brazil: 2 pages
Section on sperm quality: 4 pages
Chapter 16 on acid rain: 3 pages
Altogether: Approx. 18 pages.

If I had merely complained about the 8 pages chapter on forests, it would be possible to claim that Lomborg might have had an unfortunate hand with respect to just that issue, and that it was not a general characteristic of the entire book. If it is to be assessed whether the complaint is relevant for the whole book, the complaint may hardly include less samples than what is the case here.

I do regret that each point demands reading a large amount of background material, for instance concerning the question of acid rain, covered by 3 pages in the book. However, I believe that this is unavoidable if the assessment is to be thorough, rather than a superficial assessment of whom of the opponents is the smartest controversialist.

Lomborg's criteria fall back on himself

It is a general feature of Lomborg's writings and newspaper letters that he consistently accuses his opponent of the errors which he himself is committing. Everything he is criticizing others for, you could – with even greater right – accuse himself of.

This is also apparent when Lomborg puts up various criteria for how subjects of this kind should be treated. He breaks his own criteria at his whim. He follows the criteria as long as it benefits his case, and as soon as it does not suit him, he drops them. In the beginning of his book, on p. 7, he writes, for example, that it is of fundamental importance to employ global figures instead of using single examples which just suit us. Following this statement, he most strikingly breaches this principle already on pp. 22-23. This tendency to change the criteria whenever this is convenient also plays a part in the subjects of my complaint.

Politically motivated harassment?

Lomborg accuses me of having political motives, and wants the complaint to be refused on these grounds. Here, his criteria apply to himself. I enclose a copy of a comment by Lomborg in Ingeniøren (The Engineer) (enclosure 1): ”It is really a fantastic argumentation: Simply by labeling the arguments of others as political, one may reject their presented documentation – without having to go through the exhausting process of relating to empirical facts, not to mention producing alternative documentation”.

How can Lomborg call me political, when he is far more political himself? He creates a general distrust in biologists; of his own accord, he contacts the person in line to become the next prime minister, and makes him cut down on institutions influenced by the biologists' way of thought, and establish a new institute tailormade for Lomborg himself instead. That is really political.

How can Lomborg think that if he smears and throws suspicion on biologists, then it is not political, but if I throw suspicion on him, then it is? In my complaint I clearly state that I am not making any statements on the political aspects of the case, but only the ethical. I ask DCSD to acknowledge that the case has some ethical aspects, and that those aspects are the ones which should be assessed.

Lomborg complains that I am exposing him to harassment. How can he say so, when he has harassed numerous named scientists and entire groups of academics to a much higher degree himself? He has thrown suspicion on and otherwise smeared a large number of persons, mostly for no reason at all. I know a number of persons who have felt extremely harassed by Lomborg. How can he then complain that he is himself a victim of “harassment”, when exposed to a well-substantiated complaint?

Lomborg complains that his defense is very time-consuming, and wants to be released from further defense because this would consume his time and resources. How can he afford to say that? As long as it has been of advantage to Lomborg himself, he has insisted that his opponents spend enormous amounts of time on taking a stand on his enormous number of details. For example, the headline of the above-mentioned comment in Ingeniøren reads ”Why not read my book?” And he has been able to insist because he himself has been able to work on these issues within his working hours, and because it has promoted the sale of his books.

Many of his opponents have given up spending more time on him after a few rounds of debate, because the debate never moves anywhere, and ends up as fruitless waste of time. Already in January 1998, Kim Carstensen from WWF-Denmark realized this and ended a letter on the web pages of Politiken by noting that he did not bother to write any more. Which instantly made Lomborg mock him: ”When someone comes around and opposes them, then they don't 'bother' to say any more” (enclosure 2).

Niels Erik Skakkebæk realized this from the beginning, and he has explained to me that he chose to keep out of any discussion because his research is very time-consuming and he has no spare time left. How does Lomborg react to that? In the feature article “Den gode vilje” [good intentions] (enclosure 3), Lomborg says about Skakkebæk: “We criticised his research. And he still has not found time to answer. Neither in Politiken nor in international journals, where the same serious criticism has been raised”. Incidentally, it is a wrong statement that Skakkebæk has not participated in discussions in international journals.

Stuart Pimm and Jeff Harvey have the same problem about time constraints. They are busy persons who in fact do not have the necessary time to handle such a complaint, but carry on with it anyway because they deem it necessary. Harvey has written to me: ”Lomborg is an awful time sink”, and he really is, because he insists on confusing presentations and endless diving into still more absurd details, instead of giving in on those points where he is in fact wrong.

I myself have felt this deeply. During 1999, I needed to spend 2 entire months without any salary in order to counter Lomborg (my contributions to “The Cost of the Future”, and related editorial work) , and this year I have spent close to 2 non-paid months of work (contributing to “Skeptical Questions and Sustainable Answers”, and this complaint). It is not because I did not have better things to do with that time; I have not had any personal benefit from it; I have done it solely for the sake of the case, because it vexes me that someone gets away with cheating, deceiving and manipulating, smearing a large number of named persons, creating public dislike of entire professional groups, and undermining all kinds of work for the world’s environment - not least the idealistic, non-paid work which I, among others, have spent lots of time on.

But in the moment when the picture changes - in that same moment when Lomborg has gained an important position and needs to spend his time on details presented by others, he instantly starts to complain that this takes time.

I have sacrificed around 4 months of my valued time, which Lomborg has stolen from me.  It does not bother my conscience that I may be taking Lomborg’s time as long as he has not spent a similar amount of time on me.

Lomborg’s indirect style

I ask the committee to assess Lomborg’s text not only by the writings themselves seen from a rigorous perspective, but also in light of the impression which readers will gain from reading the text. In my opinion, the crucial point is not exactly which letters the page contains, but the meaning which is transmitted to the reader. A text may be deliberately misleading or distorted in a subtle, indirect way.

When I ask for such a view, it is because Lomborg to a large degree shields himself from criticism through imprecise phrases. In general, Lomborg’s text seems to be written on the basis of the presumption that “this will be severely criticised”, and is therefore worded so that there are no direct assertions which the criticism may anchor on. Instead, the crucial points are expressed indirectly.

One example is when Lomborg, in ”Verdens Sande Tilstand” [The True State of the World] (and similarly in TSE), says that “the biologists have a clear opinion of where the debate between figures and models should end. There are many grants at stake”. From a rigorous point of view the sentence “Many grants are at stake” is true. But from a psychological point of view it says something rather different, i.e., that biologists en bloc are dishonest. However, if you criticise Lomborg for claiming that biologists are dishonest, he can always reply that he never wrote that. Even when at least 95 % of all readers would understand the text in that way.

That the text is indirect does not by any means prevent the message from coming through. On the contrary. When the Danish Prime Minister tells off experts and “arbiters of taste”, I sense the inspiration from Lomborg. When I hear about one after the other of the colleagues with whom I have collaborated so far being given notice, I feel that Lomborg’s indirect, insinuating wordings are deeply damaging to Danish research and Danish environmental efforts.

So I am asking the Committee not only to take into account the precise letters on the paper, but also the meaning which is expressed.

### In bad faith

It is a basic contention in my complaint that Lomborg has been writing in bad faith. To support this contention I would like to point to the process: To begin with, Lomborg and Ulrik Larsen had 4 feature articles published in Politiken. These were heavily criticised, but the criticism hardly left any traces in Verdens Sande Tilstand [The True State of the World], which was published half a year later.

Verdens Sande Tilstand [the True State of the World] was intensely criticised in the counter-book Fremtidens Pris [the Cost of the Future], but when he releases The Skeptical Environmentalist in 2001, Lomborg has only taken very few parts of the critique into account, and only half-heartedly. During 2001, Lomborg has seen the criticism, but still continues to write the same as before. In order to improve the Committee's ability to assess this aspect, I enclose a copy of the relevant pages of Fremtidens Pris [the Cost of the Future] (enclosure 4).

The majority of Lomborg's objections in Godhedens Pris [the Cost of Goodness] consist in evasive actions. He tries in every possible way to avoid taking a stand directly on the essence of our points of complaint. The same applies to Lomborg's reply to the present complaint.

## THE CHAPTER ON FOREST CLEARANCE

### Breach no. 1

I maintain all of my text regarding this point. The essence of it is that the most authoritative data (provided by FAO) unambiguously show that the world's forested area is being reduced, and this has been the trend as far back as figures can be followed (1980). Therefore, Lomborg speaks in bad faith when he repeatedly states that the forested area is constant.

It is correct that Lomborg does not directly say what the explanation is. But this is simply part of Lomborg's indirect style. The only possible explanations for Lomborg's assertion about constancy are either establishment of new plantations, and/or afforestation in the temperate zones. Lomborg mentions both possibilities, and nowhere distances himself from them – on the contrary. The reader is unable to gain any other impression than these are the factors explaining Lomborg's assertion.

Lomborg's contention that the forest area is constant is exclusively supported by the time series which go further back in time than 1980. These time series are based on data which are partly unreliable, partly misleading by including woodland. Right since January 1998, Lomborg has been intensely criticised for using these figures, and he has never been able to come up with any good explanation for why he uses them. There is no doubt that the true explanation is that only by employing these questionable figures, he may provide an optimistic impression.

In my view the long text which Lomborg cites on the lower half of p. 2 in his reply to my complaint (cited from p. 111 in TSE) represents a deliberate smokescreen. The text consists of confusing formulations (consciously confusing?). In his reply, Lomborg defends his use of the long time series by saying that the data are uncertain but the best available. This standpoint is unreasonable. The reasonable thing to say is that the data are useless. They are based on FAO's uncritical collection of data from the individual governments, and many of these data are unreliable.

Only from 1980 onwards, more reliable data exist based on FAO's own assessments. These data show an unequivocal decline year by year during the same period, whereas Lomborg's long data sequence shows a slight increase. So for the only period where we are able to check whether the long time series shows the correct tendency, it appears that it does not.

The inconsistency between the long time series which Lomborg prefers to use, and the more recent and more authoritative data, is crucial. I do not accept that Lomborg time and again avoids to state an open opinion about this inconsistency.

On the contrary, in TSE as well as in his reply, Lomborg spends one page after another throwing discredit on FAO's more reliable figures from 1980 onwards. The systematic discrediting of these figures which do not suit Lomborg, while being strangely uncritical towards the old data series which suit him well, shows Lomborg's deliberate onesidedness. It becomes quite absurd when Lomborg, for example in his note 767, says that the forest area in 1961 was $4.375086 \times 10^9$ ha. This illustrates Lomborg's general strategy of making the figures he likes, look very precise.

Breach no. 2

I maintain all of my text regarding this point.

I write that plantations on formerly open land constitute 1.5 Mha per year, and compare this to the 15 Mha of natural forest which is annually destroyed (by natural forest, FAO understands all forests which are not plantations).

The first number only constitutes 10 % of the last, i.e., the pressure against natural forest is only relieved by 10 % by plantations planted on open land. But I also write that in addition 1.5 Mha of natural forest is annually deforested in order to make space for additional plantations. That is, all of the so-called alleviation of the pressure is offset by the additional deforestation that takes place in order to make space for plantations in former forest. Together, the result is that the pressure on natural forest is not relieved at all.

These data are found in the FAO report's table 3, on a page in the report which Lomborg must necessarily have read.

Breach no. 3

Regarding this point I maintain my complaint, but I modify its contents.

The point concerns how much forest has in fact been cleared since man started to clear forests during the Stone Age. Here Lomborg and I argue in favour of figures around 20 % and 50 % respectively.

Lomborg's reply to my complaint is not convincing. He thinks that the inconsistency between the estimates of 20 % and 50 % is a technical question, not a question of dishonesty, and then immediately goes on to say that he in fact ***did*** discuss the contention of 50 % deforestation. But he is not facing the relevant question. He finds an opponent to whom he may attribute a particularly high estimate, and then goes on to fire against this opponent. The opponent is WWF, and the high estimate is 67 %. I do not see what kind of relevance this has for my complaint. I am speaking about an estimate based on WRI, and estimates around 50 %. This is rather another of the numerous examples of how Lomborg does not distinguish between organisations which are based on collection of money and therefore may have reason to exaggerate, versus statistical and scientific bodies.

In "Godhedens Pris" [The Cost of Goodness], Lomborg has answered in more detail (p. 78). Here, he mixes up WRI and WWF, claiming that the high estimates (50 % or 67 %) stem from World Conservation Monitoring Centre (WCMC), and that WCMC's figures are unreliable. How it could then happen that WRI has reached the result of 50 %, whereas WWF claims 67 %, is not explained. He states that he is referring to preliminary figures made available to WWF in 1997. It is unclear how it could in such case appear with WRI before 1997.

When you read WRI's 1996-97 report[1] (enclosure 5), you do not gain the impression that they have received any information from WCMC. On the contrary, reference is made to a report by Wilcox and Duin from 1995. This is a "draft report" from Institute for Sustainable Development. Thus, it cannot be the same report which WWF is referring to, and it is not so ghost-like that WRI is unable to make a concrete reference to it.

WRI also published a report two years earlier[2], a report covering 1994-95. When I wrote my contribution to "Fremtidens Pris" [The Cost of the Future], I mistakenly believed that this was the report which Lomborg had used. In my own text, I used the 1994-95 report, and Lomborg subsequently has checked it, which is apparent from note 386 in "Godhedens Pris" [The Cost of Goodness]. In this report (photocopies of the relevant pages are enclosed, enclosure 6), table 20.3 shows country by country that the estimated percentage of total forest lost, in most cases lies around 50 % or above. Figures covering Europe are lacking, but an estimate for Europe is found in the 1996-97 report; the estimated figure of lost forest lies around 60 %, an estimate which Lomborg cites in TSE.

Estimates regarding certain countries are missing in the table. Additionally, some of the figures concern certain forest types,  primary forest or forest of special natural value. Therefore, I did not try to calculate any total estimate for the entire world based on these figres in "Fremtidens Pris" [The Cost of the Future]. I calculated a total estimate for the tropical forests, and merely stated that the proportion of lost forests in the rest of the world was "similar".

In "Godhedens Pris" [The Cost of Goodness], Lomborg claims that the figures in WRI's report for 1994-95 must be wrong regarding the five most forest-rich nations in the world, i.e., the former Soviet Union, Brazil, USA, China, and Indonesia. But only two of these countries are tropical countries included in my calculation. Lomborg criticises data for Brazil using a formulation which I simply do not understand. The criticism regarding Indonesia is probably justified; to state that 51 % of Indonesia's original forests have disappeared is probably an exaggeration, though probably not a large one.

When generally comparing WRI's 1994-95 report with FAO's current figures for forest areas, the tendency is that the WRI overestimates the forest areas in South America, and understimates those in Asia, while the trend in Africa is mixed. Thus there is no general propensity to underestimate the current forest areas, and so neither to overestimate the forest losses. But it is true that the figures must be regarded as very uncertain; probably different definitions of forest have been used in different parts of the world.

Concerning the ***source*** of the figures in the WRI's 1994-95 report, it is apparent that the estimates stem from WRI in collaboration with UN and IUCN, and that they have been collected from many different sources, based on detailed inventories in the individual areas rather than quick estimates for the world at large.  World Conservation Monitoring Centre has contributed information on the areas of mangrove forests, however these areas are small compared to the total forest area. I am simply unable to see how Lomborg can assert that the figures are ***solely*** based on WCMC's estimates, referring to p. 328 in the WRI report.

Altogether, it is possible that the figures in WRI's table 20.3 to some degree reflect the loss of the biologically most valuable forest types, rather than the loss of all forest. But I do not see that Lomborg is justified in his complete rejection of the figures.

However, another factor is that the subject is treated by the most recent WRI report[3], covering the years 2000-2001. Lomborg must also have read this report, as he refers to it. This report says (photocopy enclosed, enclosure 7): ”Using this approach, Matthews (1983: 474-487) estimated that as of the early 1980s, humans had reduced global forest cover about 16 percent. Updating this study with more recent deforestation data available from FAO brings the total loss of original forest cover to roughly 20 percent. Historical forest loss could be much higher, however. A 1997 study by WRI, which used a higher resolution map of potential forest than the Matthews study, estimates that original forest cover has been reduced by nearly 50 percent (Bryant et al. 1997:1).” Bryant et al. is a WRI report which I have not seen.

Now if we consider the most recent data to be the most correct, then we must expect the “true” value to lie somewhere in the interval from 20 % to 50 %. In that case, the uncertainty is so great that it is impossible to decide whether Lomborg’s figure of 20 % or mine of 50 % comes closest to the truth.

Let us now return and repeat what Lomborg said in TSE p. 112: ”Globally it is estimated that we have lost a total of about 20 percent of the original forest cover since the dawn of agriculture. This figure is far smaller than the one so often bandied about by the various organizations. The WWF, for example, claims that we have lost two-thirds of all forests since agriculture was introduced, as mentioned in the introduction [TSE p. 16], although there is no evidence to support this claim [here Lomborg inserts a note telling that WWF however has also employed estimates saying that 50 % of the forests have disappeared]”.

He says that even though he is familiar with WRI’s report covering 2000-2001, which states that the estimates may vary from 20 % to 50 %.

I thus have to modify my complaint. I still do think that the figure of 20 % is put too low. But when the figure is still mentioned as a possible minimum value in the most recent WRI report, I will not deny Lomborg his right to quote the figure as a possible value. Only he needs to mention that figures of 50 % are likewise possible values. Lomborg cannot defend saying that there are no realistic values higher than 20 %. When he omits these higher estimates, it is a conscious omission.

Furthermore, Lomborg cannot allow himself to criticise WWF for employing higher estimates. He writes in bad faith when he says that ”there is no evidence to support this claim”. Yes, there is evidence, and Lomborg knows that. As regards WWF’s highest estimate of 67 %, this figure seems to be consistent with the figures found in WRI’s 1994-95 report, which it is not possible to blankly reject as Lomborg tries to do.

I maintain my contention that Lomborg has acted in bad faith when claiming estimates higher than 20 % to be unjustified.

Breach no. 4

I maintain my complaint regarding this point.

The point concerns how much ***tropical*** forest that has been cleared since man started clearing forests during the Stone Age. Here, Lomborg argues in favour of a figure around 20 %, while I argue for figures close to 50 %. I have calculated a figure myself, and I refer to the existence of other figures (here I am implicating the figures mentioned in Thorkil Casse’s chapter in “Fremtidens Pris” [The Cost of the Future]).

Lomborg provides only one source of his figure of 20 %. This source is p. 60 in Reid (1992)[4]. I enclose a copy (enclosure 8). In table 3.3, second column, you see data for the supposed original area of humid tropical forest, and the table’s third column shows data for all remaining tropical forests, humid as well as dry, in the 1980s. You notice that the ***total*** remaining tropical forest constitutes 80 % of the original ***humid*** tropical forest. In my first complaint I pointed out that such a calculation is not valid.

Lomborg does apparently not understand this objection. He replies: “It is not apparent why KF finds that the estimate of 20 % reduction is useless”. It is strange that Lomborg does not understand this, but then I will have to explain this point more clearly. Assume that little Bjørn has been given 10 apples. But he is not able to refrain from eating them, and after a short while only 5 apples are left. Soon after, his mother asks him how many of the apples he has got left. He would feel embarassed to have to say 50 %; but then he remembers that he has also got 3 oranges – i.e., he has 8 fruits altogether. And the 8 fruits comprise 80 % of the number of apples he was given. So he happily answers that he has got 80 % left.

If Lomborg is able to see the mistake in this arithmetical problem, he should also be able to see why one cannot calculate the remaining area of ***dry*** and ***humid*** forest as a percentage of the original ***humid*** forest.

By the way I must repeat what I said before, that 15 % of the tropical forests has disappeared only during the period 1980-2000. Nothing indicates that the clearing rate was much lower before that – Lomborg even gives numbers which might indicate that the clearing rates were formerly higher than now. If only 20 % altogether had disappeared, then the forest clearing should not have started until 1973. And even if we, like Lomborg, assume that “only” 9 % has been cleared since 1980, this would mean that the clearing should not have been initiated until 1956. Even Lomborg must be able to see that this can not be true. Far more tropical forest than 20 % must necessarily have been cleared. Whether the correct figure is 43 % or 49 %, or one of the other figures which I cited in Fremtidens Pris [the Cost of the Future], is hard to say.

Lomborg can not have been in good faith when he maintained the figure of 20 %, and rejected all figures close to 50 %. He, who is generally so clever at calculations, bases his estimate on the idea that one third of the world’s rain forests are found in the Amazon region, and that according to himself 15 % of the forest in this area has been cleared. If the average for all tropical forests were to be 20 %, then the average for forests outside the Amazon region should be 22.5 %. I do not believe that Lomborg himself believes that this is the case.

But if Lomborg finds it that difficult to believe the figures for the tropical countries which he has seen in WRI’s 1994-95 report, then as an alternative he may look at how large a proportion of the area of various tropical countries are included today in FAO’s very broad definition of forests, according to the most recent FAO report, which he has in fact studied.

Let us look at a number of countries in the tropical zone, which based on the natural conditions should be almost 100 % covered by forest. For such countries, the forest areas in the year 2000 constitute the following percentages of the total area:

| | |
|---|---|
| Guatemala | 26 % |
| Nicaragua | 0 % |
| Venezuela | 54 % |
| Ecuador | 37 % |
| Surinam | 86 % |
| Fr. Guyana | 90 % |
| Liberia | 31 % |
| Gabon | 82 % |
| Dem. Rep. Congo | 60 % |
| Congo (Zaïre) | 65 % |
| Bangladesh | 9 % |
| Burma | 52 % |
| Thailand | 29 % |
| Laos | 54 % |
| Cambodia | 53 % |
| Vietnam | 30 % |

| | |
|---|---|
| Malaysia | 59 % |
| Indonesia | 55 % |
| The Philippines | 19 % |
| Papua New Guinea | 66 % |

At first sight, the data seem to center somewhere around 50 %, and if – for the sake of fast orientation - we calculate a rough average (unweighted), the figure we get is 48 %. This is nicely consistent with the estimates that are generally in circulation, and with the figures which I have adduced, based on the 1994-95 WRI report. On this background, it seems incomprehensible that Lomborg is so vehemently critical towards WRI's figures, but unyieldingly adheres to the figure of 20 %, whose origin is a miscalculation.

In his note 812 he writes: "Several sources state that we should have lost more than 50 percent of the rain forest . . . Unfortunately, there are no references." Here, among others he omits the references in Fremtidens Pris [the Cost of the Future], found in my chapter as well as in Thorkil Casse's. Here references are given. The reference in my chapter is WRI, and WRI's data are not, as claimed by Lomborg, based on the allegedly questionable statements from WCMC. Lomborg therefore can not afford to ignore them.

I find it odd that Lomborg is utterly uncapable of seeing evident errors in the extremely weakly based figure which he relies on himself, while outright rejecting other, higher figures, which admittedly are still very uncertain, but at least better founded than Lomborg's own.

We are within an area where any estimate necessarily must be very uncertain, and therefore it does not hold water for Lomborg to exploit this uncertainty in order to reject figures which do not suit him, while ignoring the uncertainty when the figures are clearly too low, but suit him well.

I must ask the Committee to assess whether the extreme lopsidedness in Lomborg's evaluation of the figures is deliberate or not.

Breach no. 5

I acknowledge that Lomborg's figure of 7 % is not a misprint. Apart from that, I maintain all of my text regarding this point.

As supporting evidence that only 7 % of the tropical forest in South East Asia has disappeared since the year 1700, Lomborg encloses a photocopy from chapter 10 in "The earth as transformed by human action". It appears that the figure is not valid for forests, but for "Forests and woodlands", which is stated to be comprised of 7 different categories of "woodland" and "shrubland". The area figures in the table are thus not valid for forests in the normal sense of the word.

The table in question is taken from a chapter which only briefly touches on the subject, in its opening paragraph. The chapter does not contain anything else about the issue of forest clearance than what is apparent from the table. It would be more natural to consult that chapter in the book referred to which in fact treats the subject of forests, viz. chapter 11. Lomborg cites this chapter elsewhere, so he must have read it. I enclose a photocopy of parts of this chapter (enclosure 9). Here, the forest clearance in South East Asia is treated on pp. 188-189. However, the presentation is only based on Burma and Malaysia, which have been selected as examples. From table 11-5, it appears that from 1880 until 1980, the total area of "forest" in these two countries has shrunk from 52.6 Mha to 33.9 Mha. Thus, for these two countries alone, an area of 18.7 Mha have been lost only during the period from 1880 to 1980. It belongs to the picture that Burma and Malaysia are among the South East Asian countries which have conserved the ***largest*** proportion of their forests. The average forest loss for all of South East Asia must thus be a larger proportion that what is seen here. This may be compared to the table which Lomborg prefers to use, according to which only 17 Mha of "forest and woodland" has been lost in ***all*** of South East Asia between 1850 and 1980. This cannot possibly be consistent with close to 19 Mha having been lost only in Burma and Malaysia, which cover around 22 % of the region's area.

In my complaint, and based on other arguments, I have claimed that Lomborg should have been able to see very easily that the figure of 7 % could not possibly be correct. And here is an additional argument. The figure can not by any means be correct, and the correct figure must be closer to 50 %.

In the table which Lomborg is using as supporting evidence, it is possible to find many other figures which cannot possibly be correct. For example, Lomborg himself states that 30 % of the original forest in the USA has been lost. Converted into area, this must mean that approximately 97 Mha have disappeared in the USA.  But in the table which Lomborg is using here, a loss of forest and woodlands of only 74 million from 1700 onwards is indicated. This figure is thus smaller than the figure which according to Lomborg himself is valid for the USA alone, and therefore it cannot be true. Except, of course, if Lomborg allows himself to switch back and forth between various categories, for example between “forest” and “forest and woodland”, according to his current liking.

I do not wish to contend that conscious cheating is the case here, but at any rate gross sloppiness is concerned. It is no use when Lomborg thoughtlessly repeats the figures in a table without considering whether they can be correct at all.

Breach no. 6

I maintain all of my text regarding this point.

The point concerns the definition of forest, and whether it is reasonable or not to employ the category “forest and woodland”. Lomborg only counters this aspect in a single sentence, in writing that he does in fact explain to his readers what “woodland” is, namely in his footnote 770.

I do not agree. The note contains a kind of explanation of what is understood by “forest and woodland”, but it is not clear at all how “woodland” differs from “forest”.  We are only told that it has something to do with ”everything with regular tree trunks”. I think this description makes most readers imagine something similar to a forest. They do not form a mental image where only 5-10 % of the area is covered by tree crowns. Furthermore, Lomborg ignores here that “woodland” also comprises scrub without regular tree trunks.

I do not know from where Lomborg has the idea that FAO should provide three definitions of forest. FAO (see the previously enclosed photocopy of FAO's forest report p. 23) as well as WRI's 1996-97 report (Lomborg's source) only operate with “forest” and “other wooded land”. “Other wooded land” comprises areas with at least 5 % canopy cover, providing that the trees are at least 5 meters tall, as well as areas with small trees, scrub or shrubs provided that these cover more than 10 % of the area. That is, it includes maki and certain types of scrubby steppe. When Lomborg claims that it covers ”everything with regular tree trunks”, then it is misleading, since it also covers scrub etc.

Lomborg ***did*** read the concerned definitions. He does know what the term in fact comprises.

In the general understanding, forest clearance is concerned if you clear a forest and it is replaced by grazing areas with scattered trees, or by shrubby vegetation. Nothing at all in Lomborg's text helps the reader to divine that Lomborg is not including such situations under the concept of deforestation. I therefore have to maintain my conclusion that Lomborg's text is deliberately misleading.

Breach no. 7

I maintain all of my text with respect to this point.

The point concerns that Lomborg arranges data for the annual clearing rate for tropical forests in such a way that the reader is made to believe that the rate of deforestation is declining.

The text may be perceived in two ways. Either the way which Lomborg argues for in his reply, that it is about how the estimates have changed during the past 10-20 years, or the way which I argue for, a question of how the rate of deforestation has changed during the past 20 years. I am perfectly well aware that from a rigorous viewpoint, the text may be perceived in the way which Lomborg purports, but I believe that he has conciously phrased the text ambiguously and in such a way that “almost anyone reading this perceives the meaning to be that the rate of deforestation was...”. As I have stressed in the introduction to this letter, I ask the committee to consider the ***impression*** which the reader is left with. The ultimate test of Lomborg’s intention lies in whether he includes the estimate of 0.47 % for the 1980s. If he includes this figure, then the text is about the changes in the assessments. If he omits this figure and presents his text with the data series 0.8 %...0.7 %...0.46 %, then he tries to mislead the reader by giving the impression that the forest clearance shows a declining trend as we are nearing the year 2000.

Incidentally Lomborg replies that the ”evident” interpretation is supported by footnote 801. No, on the contrary. Note 801 says ”The loss of tropical forests ***is*** 9.2 Mha in the 1980s and 8.6 Mha in the 1990s. . ” The word ***is*** seems to indicate that we are talking about a fixed value, not an estimate.

Furthermore, I do not think that Lomborg may justifiably claim that the estimates have been adjusted downwards as we are nearing the year 2000. In the above, I already once mentioned chapter 11 in ”The earth as transformed by human action” (enclosure 9), a book which Lomborg refers to several times. Here, p.191 contains a survey of the estimates of rates of deforestation in the tropics. It appears that during the entire period between 1978 and 1986, most of the estimates - at least the better defined estimates - lie around the size of 120,000  km2/year = 0.6  % per year. This figure is not lower than the figures which FAO mentions in its most recent report (ordinary method 0.7 %, satellite data 0.46 %, average = 0.58 %). No declining trend is seen in these estimates. Another issue is that Myers operates with a high figure of more than 2  % per year. This figure is higher than the others because it refers to something different, i.e.,  clearing of ***primary*** forest, which is relevant in the connection where Myers uses it.

This is what is apparent from a source which Lomborg relies on elsewhere.

Against this background, Lomborg’s own text on p.113 in his book seems severely biased. Especially his reference to Myers is biased in this place, which is also characteristic of Lomborg’s account in general. Myers is nominated by Lomborg as one of the great villains, which is unjustified. Myers has indeed been misused by others, among others WWF, to proclaim some messages a bit too loudly, but Myers himself has advanced his arguments in a scientifically tenable way, directly contrary to what Lomborg wants us to believe.

Furthermore, I would like to point out that the satellite-based estimates are not necessarily better than the others. From a satellite it may be hard to register if a forest parcel has been cleared, if the clearing has taken place more than perhaps 6 months ago (it is hardly possible to distinguish regeneration growth of 2 m height from 40 m tall, intact trees). Interpretation of satellite photos demands that the interpreter has great experience in how the main vegetation types in the concerned region look from above, and the results are particularly uncertain if the interpretation is done on a relatively gross geographic scale.

In my complaint, I have adduced that according to certain sources even FAO's ordinary estimates are too low. I have given a single reference in support of this. More could be given, if one reference is not viewed as being sufficient. WRI also throws doubt on FAO's figures. In their most recent report for 2000-2001 (enclosure 7) they discuss how much forest that is annually cleared in the entire world: "Although the FAO estimate of 130,000 km2/year is widely cited, more recent studies – notably of Indonesia and Brazil – suggest that it underestimates actual forest loss."

Breach no. 8

I maintain all of my text regarding this point.

The point concerns the fact that there is no significant relationship between economic growth and forest clearance. Therefore, Lomborg cannot afford to argue on the basis that such a relationship should exist.

I have argued that Lomborg must know that no such relationship exists, as he has seen the FAO report for the year 2000. I would like to further strengthen this argument by referring to an article by Shafik & Bandyopathyay, cited by Anders Chr. Hansen in Fremtidens Pris [the Cost of the Future] (enclosure 4). It discusses whether rising affluence (GNP) is followed by a trend towards a better environment (through an improved ability to afford ensuring the environment). Lomborg refers to this study (Shafik 1994)[5] on pp. 176-177 in TSE, in the section on the so-called Kuznets curves. He presents the 2 parameters where increased affluence results in a better environment, but omits mention of the 8 other parameters where no such relationship is found. Among these are deforestation. Lomborg knows that, as Anders Chr. Hansen pointed this out to him on p. 74 in Fremtidens Pris [the Cost of the Future].

Hence Lomborg knows that FAO as well as Shafik find that there is no relationship between GNP and deforestation. How come then that he is able to use Shafik's analysis regarding the two points where such a relationship is found, but ignores Shafik when his result does not suit Lomborg's book?

Furthermore, in his reply Lomborg tries to manipulate the term significance by claiming that it is not so important that there is no significance, if only the trend is in the right direction. I do not understand how a person who calls himself a statistician, may disregard significance to such a degree. The authors of the FAO report even write that the relationship is not even close to being significant. I do not see why it should be more correct to weight the numbers (numbers from a big country are hardly more reliable than numbers from a small one?), and I do not see why one should think that a trend which is not significant may become so if you weight the numbers. The trend is not significant. And that's that!

Breach no. 9

I maintain all of my text regarding this point. However I would like to regret a spelling mistake, as I have in one place written 1987 where it should have been 1997, which Lomborg very correctly points out.

The point is about the extent of forest fires in 1997 compared to other years. Lomborg says: "In conclusion, 1997 was in no way the year in which fire burned more forests than at any other time in history." He is severely mistaken here. I would like to document this in detail first. Then comes the question of whether he knew that he was wrong.

In his complaint Lomborg says that he would like to defend the independent German specialist Johan Goldammer against me in this context. However, he does not need to take that on his shoulders, as Goldammer is a co-editor of a FAO report from 2001 on forest fires in the world[6]. I enclose a photocopy with certain excerpts (enclosure 19), especially the section about Indonesia, which Goldammer has co-written together with Anja Hoffman. It appears that the forest fires especially occur during El Niño years, that the latest drought episode lasted from mid-1997 to mid-1998, and that during this episode approximately 9.7 Mha were burnt in Indonesia, of which 4.9 Mha consisted of forests or plantations. As regards the hardest hit area, Eastern Borneo (East Kalimantan), Goldammer and Hoffmann quote the data which a group of Germans has found, among these Hoffmann himself who works on Eastern Borneo. These data are based on satellite monitoring, and based on these figures the involved researchers write that ”the 1997-98 fires by far surpassed the 1982-83 disaster”[7] .

Lomborg defends the official Indonesian figure from 1997 at 165,000-219,000 hectares. It is perhaps a bit silly to defend this figure, since the fires became more widespread towards the end of the drought period, in 1998. Lomborg states the total extension to be 1.3 Mha, which is still much lower than the figures which today are thought to be correct.

Lomborg defends himself by saying that a report which is a tie-in with a loan from the World Bank is probably not trustworthy; the Indonesian government probably has not been able to withstand a pressure from the lenders. How can Lomborg permit himself to use such an argument? He has always insisted that we should solely relate to facts, and not throw in subjective interpretations. But precisely in this case, when it is of advantage to Lomborg, it seems that we are indeed allowed to brush aside facts with a reference to subjective judgements.

To my knowledge, Lomborg has no concrete basis at all for believing that the Indonesian government should have been pushed by the World Bank (or rather Asian Development Bank). And it is just as possible to make a judgement which goes in the opposite direction. The government has had a (short-term) economic interest in the timber companies’ overexploitation, and therefore has had a motive for downplaying the problems which the timber companies are causing. And this view is, contrary to Lomborg’s, supported by factual information that the Indonesian government ***did*** try to mislead. According to information passed on to me by Jeff Harvey, the government tried to stop independent experts from assessing the extension of the fires on Eastern Borneo. For the same reason, the researchers had to base their estimates on satellite data.

When I state in my complaint that such relatively certain data exist, how can Lomborg then brush them off and attach importance to subjective, unfounded presumptions instead?

Next, we arrive at Lomborg’s contention that much larger fires occurred in China and the USSR in 1987. We are able to find relevant information on this point in Goldammer’s FAO report. We read that in China 1.3 Mha burnt down in 1987. Concerning the Soviet Union we read that ”official statistical data on forest fires before 1988 were deliberately falsified for political reasons”, and that two different estimates for the fires in Siberia and the Soviet part of the Far East in 1987 were 6 Mha and 14 Mha, respectively. The lowest number is based on satellite data. By comparison, satellite data showed that in the same area 9.4 Mha burnt in the dry summer of 1998, towards the end of the El Niño episode. In addition, large, catastrophic forest fires in Brazil occurred during 1998. The total picture is that 1997-98 really were characterized by unusally large forest fires, and perhaps the most extensive fires until now in historical times.

So when Lomborg claims that 1997-98 ”in no way” differed from more normal years, then it is a clear untruth. Based on the above it is possible to see that Lomborg’s remarks on the subject in his reply to me are absolutely against the most recent knowledge. He is mistaken about Goldammer’s opinion, he is mistaken about the reliability of data from Siberia before 1988, he is mistaken when he thinks that more forest burnt on Borneo during 1982-83 than during 1997-98, etc.

Ought Lomborg to have known that he was writing something wrong? He has translated his 1998 text directly, without any signs of having checked whether new information had come forward. It seems odd that he has spoken personally with Goldammer in 1998 (Lomborg's note 835), after which the same Goldammer already in 1999 is co-author of an article according to which the fires on Eastern Borneo during 1997-98 ”exceeded the size and impact of the 1982-83 fires”. It seems rather as though Goldammer, immediately after he has laid down the receiver having ended his talk with Lomborg, suddenly changes his opinion 180 degrees around, after which we see him writing the opposite of what he has just said to Lomborg. Could this really be the case, or could Lomborg by any chance have misinterpreted what was said on the phone? At any rate it seems curious that when Lomborg is revising his book in order to publish it in English, he does not have the idea to phone his acquaintance Goldammer and ask if any new information has come up since last time. Or he ought to have found Goldammer's FAO report from 2001 on the Internet – when I was able to find it, I suppose that Lomborg could have done it too.

My contention is that Lomborg should have been prompted to carry out such a check after having seen FAO's report on forests in the year 2000 and some formulations therein, which are inconsistent with his own text on forest fires. Furthermore, in TSE Lomborg refers to the most recent report by WRI as well[8] (enclosure 7), so he must have seen that too. This report likewise says: ”Tropical forest fires were unusually severe in 1997-98, following less-than-average rainfalls due to El Niño. The number of fires in Brazil increased dramatically between 1995 and 1998 . . ”. Hence, in several places he has seen that his own judgement from 1998 is not shared by the persons who later on have written for FAO. According to Lomborg's own criteria, you should stick to the official statistical works as far as possible. But evidently this does not apply in cases where it speaks against Lomborg's theses.

Summary regarding forest clearance

Regarding none of the 9 points, I find that Lomborg's reply is strong enough to disprove my complaint. Regarding one point (breach no. 5) I still think that Lomborg may be accused only of gross sloppiness. Regarding the remaining points, I think that Lomborg's text is deliberately misleading. However, with respect to breach no. 4 I must modify this to mean that in my opinion the text is deliberately misleading, but this is based on a judgement, and I must ask the Committee to consider whether they judge the way I do. Regarding the remaining points, I think there is clear evidence that Lomborg is deliberately misleading. Regarding one of these points I must modify the contents of my complaint, but I maintain the contention of clear evidence for deliberately misleading text.

## ACID RAIN AND FOREST DEATH

In my letter of March 4th I included this subject in the complaint, and I enclosed quite a few enclosures. At that point, I had not yet got hold of a report which Lomborg cites, namely the report from the European Environmental Agency (EEA): The second assessment. I have now come by it, and I enclose copies of the relevant pages (enclosure 11). I find that in fact the sentences which Lomborg quotes are found in the report, although he does not in all instances quote them absolutely correctly. I find that Lomborg naturally has selected precisely those sentences which play down the problem most effectively. He might also have quoted with a bias on the pessimistic side, and written: ”Extensive damage to trees, in the form of defoliation and discoloration, has been reported . . ”. When these damages can not be attributed to acidification with certainty, it is beause other negative influences simultaneously exist resulting in the same effects, however from this it is not possible to infer that acid precipitation is not a problem, as Lomborg does. If you know that phenomenon A is caused by phenomenon B and/or phenomenon C, you may not infer that phenomenon B is harmless. But Lomborg’s inferences are precisely of this illogical character.

The EEA report also concerns point 9 in my earlier letter, in which I wrote: ” without having seen the report in question, I can not know whether the report is misleading, or whether it is Lomborg who misleads”. Now I have seen the report and I find that it reads: ”This may partly be due to the ageing of forest stands.” In Lomborg’s text, this becomes: ”. . the cause may instead be due to the ageing . . ”. So the word “partly” has silently slipped away, and the word “instead” has replaced it. Further I wrote that I find it hard to believe that the EEA report really ignores the possibility that the acidification effect is accumulated in the soil year by year. And indeed the report does not do so; the above-mentioned sentence about ageing of forest stands is immediately followed by the following sentence: ”Soil acidification is a slow process, however, and will still continue in areas where critical loads are exceeded, with possible long-term effects.” Naturally, Lomborg has avoided quoting this addition. The required balancing of different arguments are thus found in the EEA report, but has disappeared in Lomborg’s text.

Lomborg has not answered directly to my complaint on the forest death chapter in TSE, but since it is hardly changed compared to the similar chapter in his Danish book, Verdens sande Tilstand [the True State of the World], we may look at how Lomborg at that time replied to Gundersen’s criticism of this chapter in Fremtidens Pris [the Cost of the Future] (enclosure 4).

Lomborg has enclosed a copy of Godhedens pris [the Cost of Goodness], in which we may read his reply to Gundersen concerning air pollution and acid rain (pp. 115-119).

In this reply, I first notice that Lomborg in fact did consult the reference which I think he should consult. I wrote (March 4$^{th}$) that Lomborg ought to have checked a source which is mentioned by Gundersen et al. in their rejected feature article manuscript. The course is a ”conference summary statement”, written in unanimity by 12 researchers, as conclusion on a large-scale conference on the subject in 1995[9]. It now appears that Lomborg in fact did check this source. When he still writes as he does in TSE, it must be even clearer than before that he writes in bad faith.

I have to say about Lomborg’s reply in "Godhedens Pris" [the Cost of Goodness] that the first pages are characterized by a certain reason, but towards the end, and especially on p. 118, it becomes a matter of insane nonsense. I would rather just cut through and state that it is a waste of time to reply on something which is plainly insane, but since this viewpoint hardly brings me anywhere, I suppose that there is no way out of the tedious slow torture of taking apart all of the nonsense.

On p. 117, Lomborg uses the NAPAP project in his defense. He may in fact not do that, as he to my knowledge has not read the comprehensive report. He cites a single experiment with young plants of not especially acid-sensitive trees, and believes this to be sufficient documentation. However, the example is taken from Simon’s notorious book, i.e.,  from a book accused of being biased and in which we must assume that he has chosen his examples on a selective basis. If Lomborg’s true intention really was, as he asserts, to check whether Simon is right, then he can not afford to present evidence taken from Simon’s book in order to say that he is right. A single example is not sufficient in any case. We must look at the overall trends, as Lomborg says, not at single examples.

NAPAP's conclusion that there is no known situation in North America where acid rain has been the primary cause of forest death, is simply a lie. Gundersen (p. 250) mentions precisely an example from Great Smokey National Park, and Lovejoy mentions in Scientific American ”red spruce in the Adirondacks and sugar maple in Pennsylvania”. Lovejoy says that Lomborg's contention is “simply untrue”. When NAPAP's conclusion looks the way it does, it must, as Gundersen states, be because it is enmeshed in political compromises. However, Gundersen's objection has not made Lomborg modify the text in TSE compared to the Danish edition.

At the bottom of p. 117 in Godhedens Pris [the Cost of Goodness] we arrive at the sentence “only in a few cases, air pollution has been identified as the cause of damage”. It is clear in Gundersen's texts that this should be understood in the way that direct effects of air pollution on the tree parts above ground mainly have been demonstrated in The Black Triangle in Central Europe. The sentence does not concern the indirect effects, which seem to be widespread. In spite of this objection, Lomborg has kept his text unchanged on this point without pointing out that the citation only concerns the direct effects.

Lomborg ends in absolute diaster on the top of p. 118 in Godhedens Pris [the Cost of Goodness]. Here, he refers to the following text written by Gundersen et al.: “In 1982, an assessment of damage extent was conducted which showed damage to 8 % of the forest area. By 1983, damages had increased to 34 % and by 1984 to 50 %. The hitch with these figures was that they were not based on completely identical assessment methods and therefore left an impression of the progression as being more extreme than was indeed the fact.” This is commented with hindsight by Lomborg as follows: Today we know, however, that this was purely due to a change in the method of calculation. And as reference on this issue he provides two sources; one is Abrahamsen which I mentioned in my previous letter; the other is Gundersen et al.,namely their rejected feature article manuscript. In this feature article manuscript, Gundersen uses exactly the same wording as in Fremtidens Pris [the Cost of the Future]. What Lomborg is doing here, is thus to criticise a text which Gundersen has written in Fremtidens Pris [the Cost of the Future], and as reference supporting this criticism he uses a source where Gundersen himself has written exactly the same. So he lectures Gundersen on his mistakes by referring to the fact that Gundersen earlier on has written exactly the same as he is writing now. With such a ”defense” Lomborg has left the path of common sense and is on the road to insanity.

In the following section Lomborg argues on the basis that at the same time as the state of the forests deteriorates, we see a declining $SO_2$ pollution. Therefore the two phenomena cannot be connected. Here, Lomborg demonstrates that he knows nothing about the subject, and apparently has not grasped the significance of delayed effects and the concept of ”critical load”. Well, then the rest of us must tell him what it is all about: That the critical load constantly is exceeded, means that there is a continued progressing acidification of the forest soil, and that its buffer capacity is gradually used up. This progressive worsening advances merely a bit slower than before, but it continues to advance. Therefore one can not argue in the way that Lomborg does at all.

The story about the old photos I already touched on in my earlier letter, and I have mentioned that they concern individual trees which do not necessarily illustrate the general trend. Lomborg's sentence saying that foliage loss “in reality only” is an expression of well-known diseases, is nonsense, as was already apparent in my letter submitted earlier.

Then comes a section in which the aforementioned ”conference summary statement” is mentioned. The content of this statement is boiled down to “we presume that...acid rain causes damages on forests”. To extract this as the essence of the researchers' statement is wrong. For instance, in my earlier letter I pointed out that according to this statement, it is certain that acid precipitation damages the roots of the trees.

Lomborg's text in The Cost of Goodness continues in a section called "Cost-benefit". It starts by raising doubt about Gundersen's figures for costs related to damages on buildings. Gundersen has taken his figure from the above-mentioned conference summary statement, but Lomborg does not attach any importance to this as there are no references. However, of the conference report's four volumes, one does contain a major number of contributions treating this subject. A "summary statement" is precisely an attempt to draw an overall conclusion based on the many contributions. The figure is thus founded on numerous concrete statements. But Lomborg evidently has not understood this.

Lomborg goes on to say that he has indeed paid due regard to the costs related to $SO_2$ pollution in his book, as he has estimated the damages on human health. It is incomprehensible how this can be an excuse for not taking damages on buildings and cultural heritage into account.

In the following paragraph, Lomborg says that we have fought against the $SO_2$ pollution based on a wrong understanding. That is nonsense. The pollution of lakes in Norway and Sweden was in fact a crucial argument for intervening. The subject was an issue of debate early on, although it was difficult for the Scandinavians to gain a hearing internationally, until other types of damage started to occur too. Then appeared the incipient forest death in Central Europe, which led to greater motivation for the combat of the pollution, especially in the areas near The Black Triangle. If these interventions had not taken place, we would naturally have seen much more forest death in these areas. And even after reductions had been realised, the critical load for acidification of forest soil is still exceeded in very large areas in Europe. However Lomborg has not perceived this, as he evidently has not understood the term critical load. As is described by Gundersen, the pictures of the large dead forest tracts from Central Europe were the factor which ultimately sparked the fight against pollution. It is a curious assertion that these pictures should be irrelevant for the $SO_2$ pollution. When Lomborg claims that the ony rational argument for intervening is in order to reduce the mortality of humans, then this only shows that on this point he is an idiot who does not understand the subject he is writing about.

Lomborg claims that our prioritisation is wrong, but he does not provide any documentation at all supporting this assertion. He totally ignores certain kinds of efforts, for instance the comprehensive administration of lime to lakes in Scandinavia and forest soil in Central Europe, without which the damaging effects would have been greater than they are now.

Altogether, my conclusion is that Lomborg's defense consists of insane nonsense, written by a person who hardly knows anything about the subject and who does not understand the processes which acid precipitation initiates in nature.

## THE DECREASING SPERM QUALITY

### Introduction

When I sent photocopies of relevant articles on March 4th, there were some which I was not able to get hold of/had not yet come by at that point. I wrote that I would submit more copies later. Meanwhile I have considered whether my first complaint text was solid enough, and I have chosen to substantiate it further. I therefore enclose further material concerning this point.

Lomborg has not treated the subject of sperm quality in TSE in his reply; but as its text is almost identical to the text in his Danish book, Verdens sande Tilstand [the True State of the World], we may use Lomborg's defense of his text in Godhedens Pris [the Cost of Goodness], where the defense is found on pp. 133-134.

Lomborg's defense in Godhedens Pris [the Cost of Goodness]

At first, Lomborg assures us that he does not ignore the testicular cancer indeed. In a way, that is correct. It is mentioned as the object of a preposition phrase in a single sentence at the bottom of p. 238. Thus, the testicular cancer is allowed to cover around 0.7 % of the main text on the subject. Add to this a footnote, note 1860, which admittedly starts by saying that the increase in testicular cancer is "substantial", but then he makes much of downplaying the connection with sperm quality. I return to this in a while.

Then there is the point concerning the importance of the period of abstinence. Lomborg defends himself by stressing the uncertainty of the results which may stem from uncertainty about the abstinence period. But he constantly dodges the main argument, that the demands on abstinence period have been the same all the time. I treat this aspect in much further detail below.

The third point in Lomborg's defense is the question of what it means that the sperm counts (allegedly) is higher in New York than elsewhere. Lomborg here repeats his assertion that if you remove the New York information from the data material, there is no longer any decrease in sperm quality. This is wrong, which is apparent in for example the papers by Swan et al., including the paper from 1997, which I report on in more detail below.

The question of testicular cancer

The question of testicular cancer is one of the few points where Lomborg has adjusted his text in TSE in comparison to the Danish edition, albeit not in the main text but in a note. Is this adjustment now carried out in a balanced way?

In 1998, Lomborg wrote in the note on this point: "The connection between testicular cancer and sperm quality is possible though not obvious". This wording may perhaps be defended if seen in the light of the information which was available to Lomborg in 1998[10]. Since then, however, new information has come forward. Lomborg knows that, since Christian Ege points out an investigation entitled "Risk of testicular cancer in subfertile men"[11] (p. 282 in Fremtidens Pris [the Cost of the Future]) (enclosure 4). The article demonstrates a statistically significant relationship between testicular cancer and low fertility and concludes that "The association between male subfertility and subsequent risk of testicular cancer is strong and consistent with the hypothesis of a common aetiology." On this background, Lomborg can not afford to maintain the remark from the Danish book by writing in TSE note 1860, that a relationship is "possible though not obvious". But he did it.

Since Lomborg has expanded his note by new information, this must be taken as a sign that he has searched for new literature on the area. Through such a search, he must have encountered some of the papers showing a connection between sperm quality and testicular cancer. Many have come forward [12], [13], [14], [15], [16], [17], [18]. But Lomborg has not come by any of these. On the other hand, he cites five papers serving to downplay the connections which Skakkebæk attaches importance to. First, a paper which purportedly justifies that a greater part of the testicular cancer cases are discovered today than formerly, i.e., that the increase in incidences is exaggerated. However, the paper in question[19] does not substantiate this at all. Moreover, four papers discussing that testicular cancer is related to other factors than sperm quality and estrogens. One of these[20] points to tobacco smoking as a cause. It is written by Johs. Clemmensen, who is now very old and unfortunately perhaps a bit senile. At any rate his paper does not in the least substantiate a connection with smoking. In case the reader does not believe me, I enclose a copy of the paper (enclosure 12). On the contrary, the case has been investigated and it has been found that there is no causal relationship at all between testicular cancer and smoking[21] (relevant pages are enclosed as photocopies; enclosure 13). Next, Lomborg quotes a paper in which the hypothesis is advanced that iron in the nutrition may play a role. It is only a hypothesis, and it must seem questionable. Finally, there are two relevant references to the importance of fat diets and the lack of exercise, respectively. A relationship with these factors is confirmed by many other papers and is consistent with what Skakkebæk and his co-workers have published.

Of Lomborg's 5 references to new literature on the subject of testicular cancer, 3 are thus seen to be questionable or irrelevant. Lomborg has thus been very uncritical regarding the inclusion of literature which points away from the connection between testicular cancer and sperm quality. And simultaneously he has most remarkably avoided to mention literature which points to this connection, including literature which has been pointed out to him. It seems very biased that when Lomborg at last brings his text up to date on a point, then he does it in an extremely selective way which evidently first and foremost serves to create a distance to everything Skakkebæk stands for, rather than serving any purpose for Lomborg's readers.

The estrogen effect

The Danish team of researchers led by Professor Skakkebæk have to my knowledge never asserted that the drop in sperm quality were due to artificial estrogen-like substances in the environment. They have merely suggested this as an important possibility.

The possible relationship is mentioned in a paper by Sharpe and Skakkebæk, which Lomborg refers to[22]. A photocopy is enclosed for information (enclosure 14). On the second page of the paper, there is a survey of "Routes of human exposure to estrogens that have changed in the past half-century". It contains 7 points. Lomborg represents them in a shortened version on the top of p. 239 in TSE. Regarding those points which Lomborg has an interest in stressing, he has changed the wording so that the effect seems more certain. On the first point, Lomborg has changed "may increase" into "seems to increase". On the second point, "can increase" has become "increases". But when the artificial estrogen substances are concerned, the changes are in the opposite direction – now a totally misplaced "perhaps" is put into Lomborg's text. Then Lomborg comments this survey by emphasizing that the artificial estrogens only make up "a subset (and one of the most uncertain)" of all the possible explanations, but "it was this story that the media chose to circulate". However, Lomborg concludes that on the background of having omitted two sorts of artificial estrogen sources from the survey, and without reason having pictured the increase in the consumption of artifical estrogens as being very doubtful.

So the conclusion is that this part of Lomborg's text is slightly biased and manipulated, too.

Information has turned up which indicate a certain connection between PCB, DDT, and low sperm quality, which indicates that known environmental poisons really do play a role[23]. Still it may be reasonable to say that the public focus on artificial estrogen-like substances has been exaggerated. The reality is that we do not know the cause of the falling sperm quality, and that the quest for possible causes should be widened to encompass other groups of chemicals and life style factors as well. It may be discussed whether it has been reasonable to impose limits on estrogen active substances already now. The authorities have used the principle of precaution, and I suppose for good reasons, since a decline in sperm quality is such a serious case that all thinkable causes must be prevented.

Is the New York investigation credible?

In my previous complaint letters, I mentioned that Lomborg in his presentation has attached much importance to an investigation by Fisch et al. from New York, although Skakkebæk had warned him against giving it too much credence, as it exhibits a lopsidedness to the optimistic side.

In note 1873 in TSE, Lomborg further details Skakkebæk's attempt to warn him against using Fisch's articles. One may gain the impression from the note that since Fisch's conclusions do not suit Skakkebæk, Skakkebæk tries to throw suspicion on Fisch's honesty and independence instead of assessing the professional standard of the paper. Lomborg writes: "Asked directly, Skakkebæk does not himself possess any knowledge that could suggest any question of conflict of interest." This is definitely not consistent with what Skakkebæk has told me. Skakkebæk has reason to suspect that Fisch was under the influence of a certain chemicals company. As a researcher, Skakkebæk naturally does not possess enough resources to investigate whether such a suspicion is tenable, and therefore had to content himself with non-verified assumptions which in the nature of the case may not be cited publicly. This puts Skakkebæk into an uncomfortable dilemma, which Lomborg evidently exploits by impudently asserting that Skækkebæk "does not himself possess any knowledge", an assertion which is untrue, but which Skakkebæk does not have the opportunity of denying in public.

The question now is whether Lomborg should have avoided attaching importance to Fisch's investigation, as it has come to his knowledge that it is suspicious. If we were to employ Lomborg's own criteria, then the answer would have to be yes, he ought to have avoided that. What makes me say that is Lomborg's reply to me regarding a quite different issue, i.e., the forest fires in Indonesia. Here, Lomborg puts forward a rather loosely sketched presumption that the Indonesian government had been put to financial pressure in order to sign a report on the forest fires' extension, and that the report is therefore untrustworthy. He has no concrete basis for this, but believes that the suspicion alone is sufficient for him to reject the report. Must we then not reject Fisch's investigation even more categorically, as the suspicion in this case is founded on a concrete basis?

One may of course not reject anything simply by launching a casual unfounded suspicion; but if the suspicion is well-founded, at least one might not attach the highest importance to the suspect paper.

If Skakkebæk has given Lomborg the same information as me – and I must assume that he has – then Lomborg has not been in perfectly good faith when he wrote his section on sperm quality, mainly based on Fisch et al.

The article by Swan et al. from 1997

When I wrote my earlier letters, I still had not procured the article about decline in sperm quality by Swan et al. [24], which Lomborg mentions. I have got hold of it now, and it is enclosed as a copy (enclosure 15).

The contents of the article is important for the issue discussed here, and I am going to report a part of it in the following.

The main contents of the article are to bring clarity concerning the trends in the sperm quality development. Skakkebæk's critics have claimed that linear regression is not the best way of carrying out a meta analysis of the present investigations, and they have argued in favour of other methods of analysis which show that there has been no decline in sperm quality after 1970. What Swan et al. do is to perform four different types of regression on one and the same data material, in order to see whether the critics are right that linear regression is inappropriate. With multiple regression, they reach R_ = 0.80 for the linear model, versus 0.79 and 0.72 for two non-linear models. So the models are about equally good, and it is not possible to say that the linear model results in a lesser degree of explanation than the others. Thus the possibility that a constant, linear decline in sperm quality is concerned may not be excluded.

The so-called spline model, where the regression line is allowed to “break” once, viz. in 1970, shows that the gradient is rather similar before and after 1970, especially when the USA is analysed separately. On the other hand, Swan’s analysis neither rules out the so-called step model, according to which the sperm quality has been constant after a decline around 1970.

It appears from Swan’s article from 1997 that the decline in sperm quality is significant for Europe separately as well as for North America separately. In spite of Lomborg having read Swan’s article, he writes in his note 1901 that ”Looking at Europe separately, we see a decline, which, however, is not statistically significant.”

It is possible that Lomborg has carried out a statistical analysis himself, and has reached a different result than Swan did, but in that case he must 1) substantiate his assertion by explaining in a note which kind of analysis he has performed, and 2) be obliged to mention that Swan et al. reached the opposite result. He does neither, and therefore I must conclude that he breaches the ethical standards of science on this point.

Is the sperm quality still declining?

On the bottom right of p. 239, Lomborg stresses in italics that ”since 1970 no change in sperm count can be demonstrated.” As regards the USA, he adds that there has not even been any decline during the last 60 years. As appears from the above review by Swan et al., which Lomborg has read and uses for support, it is very misleading by Lomborg to express himself like that. He is writing in bad faith. The correct thing would have been to say that we are unable to decide whether there has been a steady decline throughout the period, or a sudden decline around 1970.

It must be added that our knowledge about the issue has improved since 1997. For example, the year of birth of the participants is now being focused on rather than the year of their sperm analysis. This turns out to reduce the unclarity resulting when different age groups of men are examined on the basis of the same year. In some cases, this results in a significant decline in sperm quality over time, although a regression based on the year of analysis did not show a decline[25]. Danish data have also been re-analysed according to year of birth, and when this is done, various trends become clearer, including not least the trend of a steadily progressing, continued decline in sperm quality[26] (enclosure 16). A recent investigation[27] shows that among the youngest age groups of Danish men, the sperm quality has fallen even further, and it is now so low that 40 % of the men must be assumed to have impaired ability to have children with a woman. The investigation is published in the year 2000, and Lomborg might thus have had sufficient time to include it into TSE if he had bothered to search for “semen quality” on Medline.

But he may have done just that. Out of 9 recent original investigations (published in 1998 onwards) which are found on Medline[28], 7 show a continued decline in sperm quality or fertility, and 2 show an increase. Altogether, a continued decline is the overriding trend. Lomborg cites one of these 9 papers, viz. one that shows an increase (Lomborg's note 1903). So Lomborg ***did*** adjust the text, but only by adding a paper which supports his preconceived viewpoint. When the man ***has*** tried to find more recent literature, you may with justice criticise him for omitting the literature which does not suit him. If he has searched for literature on "semen quality" he must have found the review article by Swan et al. from the year 2000, a paper which absolutely contradicts Lomborg. But he has not quoted that.

Time has already run out for Lomborg's contention that the decline in sperm quality only occurred before 1970. The overriding trend is a continued decline even during the most recent years. But by citing selectively, Lomborg still tries to preserve his optimistic picture.

The importance of the period of abstinence

A rather crucial part of Lomborg's argumentation is that the decline in sperm quality may be explained by a reduced abstinence period (shorter time between ejaculations) now than formerly. Skakkebæk and his research team are of a different opinion. As stated above, he has informed me that a PhD report elaborated at his institute was unable to find any evidence supporting that the frequency of ejaculations has changed over the past decades. I have now come by the report in question, and enclose copies of the relevant pages[29] (enclosure 17). It contains a survey of investigations of the frequency of ejaculations, and the picture which appears is very different from the one Lomborg is painting. Some of the figures of the frequency of sexual intercourse/masturbation are rather low, in the old as well as recent investigations. A British investigation from 1990-91 showed a frequency of 2.3 times per week for 16-19 year old men; by comparison, Kinsey's interviews from 1948 showed 3.75 times per week for the same age group in the USA, but Kinsey's interviewed persons may not be regarded as representative. In short, any statement about trends in the development over time is based on great uncertainty.

Among others, Lomborg refers to figures in Hunt 1974 (published by Playboy Press!), according to which the frequency of sexual intercourse among married 30-year olds increased from 1.9 to 3.0 times per week between 1940 and 1970. That sounds as a substantial increase, but when you look into the notes you see that the figures presented are the median values. The average values only show an increase from 2.5 times to 2.8, and that is admittedly not quite as impressive. It may be discussed whether the median values are the most relevant in this context. But regardless of whether you use the median or the average values, you should be consistent and do so both for ejaculation frequency, abstinence period, and sperm cell count. When the median values show a greater shift than the average values (skewed distribution with a long tail in the direction of long abstinence periods), then it is possible to get a relatively big change in abstinence period by using the median values, and to get a relatively small change in sperm cell count by using the average values. In that way you may make the respective changes in abstinence period and sperm cell count look like they fit better together than they actually do.

Lomborg's source supporting that the sperm cell concentration shows a strong relationship with the abstinence period is Swan et al. (1997) (his note 1880) (enclosure 15). If Lomborg wanted to check the authenticity of this relationship, one might expect him to do so by checking on some of Swan's references. The first reference she mentions is MacLeod et al. (1952) [30]. I enclose excerpts as copies (enclosure 18). On p. 298, you see information about the frequency of sexual intercourse: ". . the admitted "intercourse rate" throughout married life until at least the age of 40, is almost invariably twice to three times weekly, with twice weekly being by far the most frequent reply . . ".

A frequency of 2-3 times per week in 1952 does not indicate any change since then.

However, the important point is not the abstinence period under normal conditions, but the abstinence period before a sperm sample. The photocopy of MacLeod's paper shows that even 50 years ago, 20 % of the persons were not able to observe an abstinence period of more than 3 days, and that those who had a very long abstinence period of 8 days or more, only amounted to 22 %. Lomborg says that if the abstinence period reaches 10 days or more, the increase in sperm cell count becomes substantial. But as you see, 50 years ago only a rather small proportion of the men had such a long abstinence period.

Reading Lomborg's text, one sentence particularly catches the eye, i.e., : "A Swedish survey showed that the period of abstinence fell from 7.5 days to 4.4 days between 1956 and 1986, equivalent to an increase in frequency of around 70 percent." This really seems to be a marked change in behaviour which might explain a large part of the decline in sperm quality. And on p. 240 in TSE, Lomborg points out that the reduction of the abstinence period of 3.1 days in Sweden approximately fits in with the observation that the average observed decrease in sperm cell count might be explained by a reduction of the abstinence period of 3.6 days. Thus the information about the reduction of abstinence period in Sweden is rather central to Lomborg's argumentation. Lomborg's special interest in this piece of information is in fact apparent in his note 1889, which states that he has found the Swedish figures in the paper by Swan et al. (1997), and that he contacted Swan by phone in order to have the correct reference. So Lomborg has tried to get hold of the reference. Whether he has read it I do not know, but since he spoke to Swan about the reference he has hardly failed to become informed of what the reference contained.

The reference to the Swedish investigation is Bendvold et al. (1991)[31], and I enclose it here as a photocopy (enclosure 19). Admittedly a substantial reduction in the average abstinence period is seen between 1966 and 1976, i.e., around the time of the "sexual revolution". But this reduction of abstinence period may not explain the decreasing sperm quality at all. The authors have divided the persons into those with an abstinence period of up to 5 days and those with an abstinence period of 6 days or more. The total number of sperm cells in a sample from those with short abstinence period was much lower in 1986 than in 1956. The change which seems to have occurred between 1956 and 1986 is that in 1956, 3-5 days of abstinence was evidently sufficient in order to refill the body's stores by new sperm cells, whereas in 1986 3-5 days of abstinence was not at all enough to replenish the stores. This may be interpreted to mean that the production of sperm cells has become slower. The paper clearly demonstrates that the decline in sperm cell count is marked and strongly significant even after the abstinence period has been allowed for.

The fertility of men not only depends on the number of sperm cells per sample, but also on the condition of the sperm cells (whether they are mobile and of normal appearance). These parameters do not increase or improve with the abstinence period. This is clear from the Swedish investigation, which also shows a marked and significant drop in the proportion of normal sperm cells in 1986 compared to 1956. As we are concerned here with a parameter of paramount importance for the fertility of men, the decrease is important.

The Swedish investigation thus shows that even in cases where the abstinence period has been strongly reduced, the decline in sperm quality is way beyond what may be explained by the abstinence period.

These considerations are confirmed by the above-mentioned paper from 1952 by MacLeod et al. If you read the entire paper and not only the pages which I have photocopied, you may study the division of men into normal and sick/infertile. In the normal persons, the sperm cell count increases strongly with increasing period of abstinence, but in the sick the sperm cell count for a given abstinence period is not nearly as high, and does not increase nearly as much with time. The drop in sperm quality (in this case due to some disease) thus happens regardless of the length of abstinence period. Furthermore, the paper relates that the proportion of mobile sperm cells falls slightly with increasing abstinence period, as though a part of the sperm cells become “too old” when too much time has passed since last time. This confirms that some indicators of fertility do not increase with increasing abstinence period.

In his book, Lomborg only mentions the drop in number of sperm cells per sample or per ml sample. But parallel to this, many researchers also find a significant drop in the proportion of normal sperm cells, or in the proportion of mobile sperm cells. Such drops may not be explained by the tendency of a shorter abstinence period.

The conclusion is that even if the abstinence period has gone down during recent years, then this may not explain the decline in sperm quality. It is not correct as Lomborg says in his above-mentioned feature article in ***Politiken*** (enclosure 3), that “....the whole discussion about the alleged decline in sperm quality is presumably due to a combination of statistical problems and a – conscious? – omission of the strongly increased frequency of sex, which together form an explanation”. The question now is whether Lomborg has been in good faith when he wrote about the frequency of sex. We know that he has tried to procure the Swedish paper by Bendvold et al., and that if he in fact has read it, he must be in bad faith. Furthermore, we know that he spoke to Shanna Swan. Since Swan has read the papers in question, and has published work saying that the decline in sperm quality cannot be explained by the changes in abstinence period, based on a discussion with her Lomborg ought to have understood that the explanation of abstinence periods does not hold water. Furthermore, we know that Lomborg has read the paper by Swan et al. from 1997. It discusses whether the abstinence period is a “likely confounder”, i.e., whether differences in periods of abstinence may have influenced the observed tendency over time. In her paper the answer is yes, but the implication is that since the abstinence period increases with the age of the persons, and since relatively many older persons participate in the most recent investigations, the average abstinence period may be increase today compared to formerly. This is downright opposite of what Lomborg writes.

Altogether, regarding this point I can not ***prove*** that Lomborg has acted in bad faith, but it seems likely that he has.

Organic farmers

A Danish investigation demonstrated that organic farmers have a better sperm quality than other sections of the population. In a box entitled “Organic farmers” on p. 240-241 in TSE, Lomborg discusses whether this is correct. The first section of the box is translated almost directly from the Danish edition, while the last section is new.

Lomborg first says that the organic farmers were compared with persons living a more stressful city life. Therefore, he says, the difference might be due to something entirely different than the ecological life style, i.e., a relaxed life in the countryside. In support of this interpretation he writes: ”A survey . . later showed that traditional (non-organic) greenhouse gardeners also had better quality sperm than numerous other professional groups.” In this way, the reader is given the impression that the explanation of ecological lifestyle hardly holds water – it is probably just the healthy country life which makes the difference. Only if you look up Lomborg’s note 1887, you discover that the greenhouse gardeners had 20 % lower sperm quality than the ecologists. The same piece of information had been tucked into the notes in the Danish edition.

On p. 311 of Fremtidens Pris [the Cost of the Future] (enclosure 4), I criticized this structure of Lomborg's text, under the headline "Inconvenient data are hidden in the notes") (photocopy enclosed). I thought that most readers in this way are given a misleading impression. However, this criticism has not made Lomborg change his text in the English edition.

The investigation of the greenhouse gardeners is also commented by Christian Ege on p. 283 in Fremtidens Pris [the Cost of the Future] (photocopy enclosed; enclosure 4). Here it is told that follow-up data treatment has been carried out, which points in the direction that the lower sperm quality in greenhouse gardeners compared to the organic farmers in fact might have something to do with their profession. This is apparent partly from the reference given in Fremtidens Pris [the Cost of the Future] (a Danish-language report), and partly by a more recent English-language paper[32]. The more the gardeners are exposed to pesticides in their everyday life, and the more years they have been exposed to it, the lower their sperm quality. It is also told that only in winter the sperm cell count of greenhouse gardeners was 20 % lower. In summer, the difference between them and the organic farmers was even greater.

These pieces of information must be said to be essential for the interpretation of the sperm quality of greenhouse gardeners. And this information has been pointed out to Lomborg. Yet he ignores them completely in TSE, and the relevant publications are not cited in it.

It would just barely have been possible to excuse this if Lomborg had been under time constraints and did not have the energy to elaborate on the text in the box in question. But that is not the case. He has found the time to read an additional Danish investigation and to comment it, too. And he uses it to establish in a final way that the difference between the organic farmers and the others is not real, using the wording "Finally, in 1999, a large study . . settled the issue."

However, his report of the recent Danish investigation from 1999 is not correct. It is not correct that "14 sperm quality parameters were indistinguishable". 9 sperm cell parameters were measured, of which 8 showed the best values for the organic farmers. In one of these parameters (proportion of sperm cells with a normal appearance), the difference was very strongly significant in favour of the organic farmers. As this parameter is related to fertility (ability to become a father), it is an important result. Thus it must be said that the thesis that the organic farmers have a better sperm quality than others was confirmed, although significance was obtained within a different parameter than last time. Lomborg instead prefers to attach importance to a different aspect, i.e., that the relationship between sperm quality and an indirectly calculated index of pesticide load did not come out in the way which should be expected if the pesticides were harmful. However, it must be said that the load of the pesticides in question was so low for all groups that no significant negative effects were to be expected anyhow. Additionally, the parameter showing a relationship with certain pesticides was the percentage of dead sperm cells. As mentioned above, at long abstinence periods it may happen that a part of the sperm cells become "too old", so this parameter does not necessarily testify to a low fertility.

It is strange that Lomborg, who contends to be a statistician, uses the word "indistinguishable" about the differences found; the correct thing would be to write "non-significant". "Indistinguishable" must indicate that even if one were to analyse a lot more samples, one should not expect to see any difference at all. Against this speaks that the difference showed a p-value of 6 % for one of the parameters. Only with very few additional samples, this parameter might have become significant. To use the word "indistinguishable" here is definitely misleading.

All things considered, my conclusion is that Lomborg's text inside this box is consciously misleading. I reach this conclusion particularly on the basis that he writes "Finally, a large study settled the issue". In this way, he makes the reader believe that he has investigated the final conclusion of whichever follow-up investigations that may have been carried out. But simultaneously, he has deliberately failed to mention the follow-up report about greenhouse gardeners, despite that it has been pointed out to him and despite it contradicting his text.

Conclusion on Lomborg's section about sperm quality

Lomborg concludes his section on sperm quality by backing down a bit. On p. 241 (right column) he makes modified statements, like for example that the increasing frequency of sexual intercourse is "at least part of" the explanation of the decline in sperm quality, i.e., it is not necessarily the entire explanation. However, these modifications do not prevent the reader of the text at large from receiving the overall impression that the risk is exaggerated and that hardly anything is going awry. As Lomborg writes at the end: "It is, however, even more essential to point out that today we know for certain that the scary vision of the general, overriding reduction in sperm quality was mistaken." When Lomborg actually inserts the words "for certain" into this sentence, we may permit ourselves to decide that the sentence is downright wrong. The overall conclusion which is passed on to the readers, is really wrong. The same applies to the conclusion which Lomborg passes on to Danish newspaper readers (cf the above quotation from a feature article in ***Politiken***, cited in the mention of the importance of the abstinence period).

I conclude the following about the individual sub-issues:

Testicular cancer: Lomborg knows that a number of researchers regard the increase in testicular cancer and its relationship with sperm quality as an indication that the decline in sperm quality is real. The evidence of such a relationship was still rather weak in 1998, when Lomborg first treated the subject, but in Fremtidens Pris [the Cost of the Future] he was able to read that more certain evidence had now come forward. Instead of mentioning this, he chooses to cite a number of papers pointing to alternative relationships, even though most of these papers are either questionable or irrelevant. My conclusion has to be that Lomborg is consciously biased here.

The estrogen effect: Lomborg's report of the paper by Sharpe and Skakkebæk from 1993 is slightly distorted. Such a distortion is a conscious process.

Is the New York investigation reliable? In spite of what Lomborg has been told by Skakkebæk, he chooses to lend credence to the New York investigation (Fisch et al.). He might perhaps be forgiven for making this choice if he were consistent. But on the contrary, when other investigations are concerned (Indonesian forest fires), he is fast to reject them based merely on a quite diffuse and unsubstantiated suspicion. In my view, this lack of consistency testifies to a deliberate lopsidedness.

The article by Swan et al. from 1997: Lomborg relies quite a lot on this article, but yet his writings are in conflict with the contents of the article, in particular as regards whether the drop in sperm quality is significant when we look at Europe separately. With respect to this point I therefore find Lomborg's text consciously misleading.

Is the sperm quality still declining? In this case, Lomborg's text is misleading. When he even emphasizes in italics that ***since 1970 no change in sperm count can be demonstrated*** (p.239), then the wording is consciously misleading, as a fair summary of Swan et al. would have resulted in a different wording.

The importance of the period of abstinence. In this case, Lomborg's text is misleading. I cannot prove that it is deliberately misleading, but there are indications pointing in that direction.

Organic farmers. In this case, Lomborg has deliberately omitted information speaking against his thesis, and has rendered the text misleading in other ways, too.

**CONCLUDING ASSESSMENT**

I contend that the approximately 18 pages in TSE which I have complained about, are representative for all of Lomborg's written production from 1998 onwards. This contention ought to be strengthened by the fact that in their complaint, Pimm and Harvey treat many other issues.

I hope that my small sample of his production illustrates what I mean when I claim that Lomborg's writings are pervaded by dishonesty. In some of Lomborg's chapters, the biased character shows in every sentence; if one were to unravel what is wrong with the text, it would be necessary to look at the text sentence by sentence and to criticise almost every word. Almost in all places where there is any scope of manipulation, manipulation has indeed taken place. Hence the text would not become usable simply by correcting a few essential errors. The text is so thoroughly infected with manipulation that all of it must be discarded.

The differences between me and Lomborg do not simply concern some technical assessments of whether a given figure should be 53 % or 46 %. No, this is a matter of huge differences. Has 20 % of the tropical forests been cleared, or is it rather 50 %? Did 1.3 Mha forest burn down in Indonesia, or was it 4.9 Mha? Does a progressive impairment of the condition of European forests take place, or is forest death simply a myth? Has the quality of sperm samples become reduced by approximately 50 % in the course of 50 years, or has it not become reduced at all? "Godhedens Pris" [The Cost of Goodness] and Lomborg's recent reply to me show no movement at all in the direction of narrowing the gap between the points of view. As always, Lomborg lacks the ability to acknowledge the viewpoints of others. Nothing indicates any willingness in him to ever reach agreement with his opponents. Exactly this quality makes him unfit for a leading position where he needs to cooperate with many others.

I believe that I have demonstrated that Lomborg's text is biased on the optimistic side. In theory, the reason might be that by accident he has only got hold of the information which points in that direction. But when the same trend continues page after page, chapter after chapter, book after book, then it may no longer be explained away by coincidence. Such a lopsidedness may ***only*** occur through ***deliberate*** sorting out those pieces of information which do not suit the man. And such sorting out is inconsistent with the ethical standards of science, including popular presentations of science.

When the text is biased on the optimistic side in the section on sperm quality, then it can not be because Lomborg accidentally has only come by information which points in that direction, as we know that the selection which he started out reading was sent to him by Skakkebæk.

What happens if Lomborg is confronted with new information demonstrating that his text is biased? Nothing! Given the choice between truth and manipulation, he prefers manipulation. This is clear in the case of deforestation, where Lomborg has read the new FAO report on forests from 2001, but still does not adjust his text so it becomes consistent with the new facts.

What happens if Lomborg is confronted with criticism pointing out errors in his text? Not much. Primarily, and at any cost, he avoids any crackling of the picture of the situation which he has already painted. His image of the harmless clearance of rain forest in Eastern Brazil would for example become much less trenchant if he had to give up his point that no species at all have become extinct in the area. So he tucks the annoying information into some notes where only few people see it. His text is first and foremost suggestive, it tries to trigger certain feelings in the reader: feelings of resentment towards the self-righteous environmentalists. This suggestion can not be preserved if it appears that the environmentalists are often right. In order to work, the suggestion demands people to believe that the environmentalists are always wrong.

The suggestion demands everything to be positive. It is not enough for Lomborg that the number of starving people is decreasing in most parts of the world. He wants the situation to be positive in all of the world, which is why the numbers for Africa must at any cost be presented in such a way that the picture looks positive. If the text said that the number of starving people in Africa has nearly doubled in 30 years, this would mean that the environmental pessimists were right on some points, and that there was reason to make an active effort in order to make the world a better place. If there is a serious problem in one place in the world, then peace is disturbed, and you may not lean back in your deck chair and enjoy life. The crucial point for Lomborg seems to be to maintain the suggestion that ***all*** worries are unfounded.

Thus this is not just a matter of optimism, but of a fundamentalistic kind of optimism. Either the optimism must be absolute and all-embracing, or Lomborg's picture crackles. For him, only these two options exist.

I am in favour of a certain degree of optimism. A mixture of 50 % optimism and 50 % pessimism would probably be appropriate. The pessimism is necessary as a driving force – if you do not fear that anything may go wrong, then there is no incentive to do something actively in order to prevent the calamities. Pessimism has been precisely the motivation for my own efforts in nature conservation, an effort which has rendered very positive results, which could not have been obtained without a pessimistic approach. In my field of experience, pessimism leads to something good, while optimism leads to calamities. Therefore, the total absolute optimism is utterly unacceptable to me.

Lomborg divides things sharply into black and white. In his mind, only two kinds of attitude exist: The right attitudes – i.e., his own – and some diametrically opposite attitudes, viz. the wildly exaggerated, hysterical pessimism which he claims to be a characteristic of all those who think differently. This division in black and white he may only maintain through manipulation, and by expressing himself in bad faith. Why he is doing it I do not understand. The problem exists in Lomborg's own mind, and has nothing to do with the true state of the world.

Kåre Fog

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