

Copenhagen March 22 2002

Regarding the complaint to DCSD (The Danish Committees on Scientific Dishonesty) by Kåre Fog (KF), case number: 612-02-0001.

As a background to the present plaintiff's contention that I should be scientifically dishonest ought to be mentioned the fact that over the years I have participated in a long and professionally oriented debate, where I have not cut off a discussion and shut down an exchange of views, but on the contrary sought openly to answer the substantial critique and tried to justify and argue in favour of my choice of data and interpretations. This is seen most clearly in the discussion of the book *Fremtidens Pris* (The Cost of the Future), of which KF, among others, was an editor, that I together with one of my students, Ulrik Larsen, responded to in only 3 weeks with a book on 185 pages, *Godhedens Pris* (The Cost of Goodness). (The book can be downloaded from <http://www.ps.au.dk/vip/lomborg/FP/svar.htm>). I have enclosed the relevant pages where we reply to KF's 5 chapters (as far as I am informed KF has not replied to these yet). As a background this should indicate that I have not in general desisted from participating in a debate and that I have sought to explain my points with further references and explanations.

Reply to the complaint

Fundamentally, I find that KF's complaint is obviously unfounded and that it thus should be rejected. Below I will briefly go through and comment on what he refers to as the 9 'breaches of ethical standards' and show that they in no way can support any kind of scientific dishonesty.

The numerous allegations, a further letter from March 3 2002 together with a promise that more allegations could be submitted, are of such a nature that it comes close to harassment. Seen together with the 9 central allegations, that are obviously unfounded, I do not find it a worthwhile use of my resources to refute these at the moment. Therefore I believe it is necessary to take a stand on the fair degree of allocation of resources for such a complaint and to my suggestions to DCSD regarding the procedure for a possible continued debate of the factual aspects.

KF's 9 central 'breaches of ethical standards'

Breach no. 1.

Lomborg says (p. 110): "Globally, the overall area covered by forest has not changed much since 1950, as can be seen in Figure 60.", p. 111: "Globally, forest cover has remained remarkably stable over the second half of the twentieth century.", and p. 115 right column: "But as we have pointed out, there has not been a decline in global forest area during this period". These assertions that the world's forested area is of constant size are accounted for by new plantations in the tropics and overgrowing of open land zones in the temperate zone, especially Russia. But this is contradicted by FAO.

FAO says: "Net deforestation at the global level was estimated at approximately 9 million hectares per year and gross global deforestation at approximately 13.5 million hectares per year". The difference between net and gross loss is accounted for by plantations and overgrowing of areas. FAO's table 1 shows that the annual net loss of 9 Mha constitutes 0.2% of the world's forest area. The figures also show that the net loss is equal to two thirds of the gross loss. Thus, the gains far from outweigh the losses. This is further elaborated on in the report's table 3.

Lomborg has read FAO's report. He probably also has seen table 3, since he uses certain figures which in the whole text are only found in one place - immediately below this table, on the same page. And his text is in direct conflict with FAO's report, including table 3. Lomborg does not mention this conflict at all, except a small remark at the top of page 112. Thus, I have to conclude that Lomborg is speaking in bad faith, and that his text is deliberately misleading.

KF finds that the text is deliberately misleading because I should apparently claim that 'tropical forest clearing is accounted for by new plantations in the tropics and overgrowing of open land zones in the temperate zone, especially Russia.' However, he does not give any reference and that is due to the fact that I in no place say that [The *only* place, which KF also refers to in Breach no. 2 is p.115: 'Similarly, many allege that although forest cover has remained constant, this is because we have less natural forest and more plantations.' However, this is part of a string that starts 'At the same time numerous false impressions exist regarding the condition of our forests.' It is thus clear that 'Similarly,...' does not concern a factual argument that lesser tropical forests should be balanced precisely with plantations.]

When KF quotes FAO to show a decrease in the overall cover of forest, then it is correct that surveys over the last 10-20 years have shown this, but that it is precisely *not* the case when looking at a longer period. KF finds that I only mention this in a small comment on the top of p.112,1 but partly ignores the fact that I clearly cite all the surveys in figure 60, p.111 (please also note that in the figure text I clearly emphasise that data is uncertain but the best available) and that I clearly describe all the studies in the text below (p.111):

'Globally, forest cover has remained remarkably stable over the second half of the twentieth century. With the longest data series, global forest cover increased from 30.04 percent of the global land area in 1950 to 30.89 percent in 1994, or an increase of 0.85 percentage points over 44 years. With the somewhat shorter data series from 1961, global forest cover is estimated to have fallen from 32.66 to 32.22 percent. That is to say, it has fallen by 0.44 percentage points over the last 35 years or so. The UN carried out two global forest surveys in 1995 and 1997 and evaluated a more restricted limited of forest area for the period 1980-90 and 1990-95. The survey found that the area covered by forest had shrunk from 27.25 percent to 25.8 percent, or by 1.35 percentage points, although these figures are vitiated by considerable uncertainty. For example, an upward revision of the 1990 forest area was larger than the entire global decline in 1990-95 (or to put it differently – had the 1990 forest area not been revised, the period 1990-5 would have seen an increase in forested area). Moreover, Russia which has the world's largest forest cover, was not included in the survey. Thus, with these considerable short-term uncertainties it seems necessary to focus on the longest possible time periods. Those interested are referred to a longer discussion in the footnotes. In the newest forest study from 2001, FAO has changed the definition of forests once again and made a new estimate of forested area from 1990-2000 showing a small decline from 29.5 to 28.8 percent.'

Finally, KF dramatically underplays the idea that I should only discuss problems about data with 'a small comment' when as a matter of fact this is a reference to the longest footnote of the book, and the text contains a direct recommendation for continuing reading here: 'Thus with these considerable short-term uncertainties it seems necessary to focus on the longest possible time periods. Those interested are referred to a longer discussion in the footnotes.' The footnote reads:

The discussion of what constitutes the ‘correct’ definition of forest is a long one. The FAO has three definitions (see e.g. WRI 1996a: 222-3; FAO 1999b): ‘Forest’, ‘forest and woodland’, and ‘forest and other wooded land’. ‘Forest’ comprises only enclosed forest with 10-20 percent tree crown cover (20 percent in developed nations, 10 percent in developing nations). This applies to about 26 percent of the world. ‘Forest and woodland’ counts everything with regular tree trunks and accounts for about 32 percent of the world. ‘Forest and wooded land’ also covers forest fallows and shrubs, accounting for about 40 percent of the world.

The discussion about definition is also related to the way forest is measured. Measurement of global forest areas is notoriously inaccurate, as has been pointed out by many (Williams 1990; WRI 2000b: ‘The Food and Agriculture Organization of the United Nations [FAO] and the United Nations Economical Commission for Europe [ECE] recently published reports on the conditions of tropical and temperate forests with data on change in forest cover from 1980 to 1990. Such data are notoriously inaccurate and often recycled between reports simply because better data is not available.’) The reason for this inaccuracy is primarily that the data are generated by models (often based on population figures) and based on scant, out of date and inadequate data.

This inaccuracy is pretty obvious when one examines FAO’s data. ‘Forest and woodland’ has been used for almost 50 years, but even so, global reckoning for the same year has fluctuated by as much as 2 percent! (E.g. 1976, where ‘forest and woodland’ was estimated in the 1987 FAO Production Yearbook at 4,150 Mha, and in 1992 at 4,231 Mha). If one looks at the figures for 1990, FAO believed in 1995 that forest accounted for 3.442368e9 ha (FAO 1995a), and in 1997 they believed that the figure was 3.510728e9 ha (FAO 1997c:189). A change of 1.9 percent or a little more than the overall reduction found for 1990-5 with FAO 1997 figures of 1.6 percent. Had they used the old 1990 figures, the world’s overall forest area would have increased by about 0.3 percent for 1990 to 1995! On top of this, they failed to include Russia (FAO 1997c:17, table 4, note a), which has 20 percent of the world’s forest, and where there has actually been growth (see e.g. WRI 1996a:206-7).

Using short time-span series actually actually risks losing the general tendency in noise created by the individual adjustments. It has therefore been important to employ the longest time-span series available, and FAO long series from 1950 is the only one available. Unfortunately, the FAO database only provides access to figures from 1961 onward. Overall, it seems reasonable to consider all land with regular tree trunks as forest, and considering the above-mentioned problems with the accuracy of data, I find that the best description of the earth’s forest development can be achieved using FAO’s ‘forest and woodland’ figures. Even if one uses FAO’s narrow ‘forest’ definition, which has only been calculated three times beginning in 1980 and has the above-mentioned data problems, one reaches the conclusion that closed forest area fell from 1980 to 1995 from 27.15 percent to 25.8 percent of the earth’s land surface area, i.e. 1.35 percentage points.

In this connection it seems obviously unfounded that KF should claim that I write either against FAO or in bad faith, and it is obviously unfounded that should be deliberately misleading.

Breach no. 2

On p. 115, Lomborg concerns himself with the importance of plantations: ”Similarly, many allege that although forest cover has remained constant, this is because we have less natural forest and more plantations.”, and p. 117: ”Plantations . . . actually help relieve pressure on natural forest.”

The FAO report tells us something quite different. New plantations, which succeed, constitute approximately 3 Mha per year. Half of these new plantations are established through re-planting of recently cleared forest; i.e. the other half - plantations on formerly open land - cover 1.5 Mha per year. And in the report's table 3 we notice that 15 Mha of natural forest are annually cleared. Thus, felling of natural forests is far from being offset by plantations, especially not plantations on open land. Giving the readers an impression of such a relationship is misleading.

The situation described above is corroborated by other sources.

As mentioned above, the first quote is found in a string of *misrepresented* allegations, and it is thus obviously unfounded when KF argues that this should be misleading. The quote concerning plantations being able to lift the pressure off natural forests is not at all being challenged by KF.

Breach no. 3.

Regarding how much forest that has been cleared since man started clearing forests during the stone age, Lomborg writes (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 figure of 20% is repeated on p. 117. As the source of this figure, he indicates 4 references, all of which are secondary literature, and of which several works are known to exhibit a certain lopsidedness.

Immediately the reader will think that the figure of 20% represents the experts' best estimate of the true value. But when you look through the official figures, they say that from 43 to 49% of the original forest has been lost. A report by World Resources Institute, WRI, thus cites a figure of 49% for lost forests in the tropics, and figures of the same size are cited from the temperate areas. These figures date back some years, so by now an additional 5% must have disappeared in the tropics, consequently altogether more than 50%. These figures must have been available for Lomborg, as he has used the same WRI report as one of his most important sources regarding deforestation in Europe.

The figure of 20% was mentioned in the Danish book and is repeated unchanged in the English edition – although anyone must be able to see that the figure can not be true. In large parts of the temperate zone, far more than 20% of the forest area has vanished, and even as regards Russia (including Siberia) the figure is somewhere around 20% (with considerable uncertainty); here, Lomborg himself says: somewhere close below 20%. The average for the temperate zones therefore must be above 20%. In the subtropical zone it must be considerably higher, and in the tropics it is around 50%, as mentioned above. Incidentally Lomborg goes through one part of the world after the other, and almost everywhere he finds that more than 20% of the total forest area has disappeared.

Thus, Lomborg's figures lie a world away from what is quoted by other, reliable sources. Still Lomborg uses the 20% to criticise the figures given here. And this is done in spite of the fact that he has read the WRI report, according to which his own 20% cannot possibly be the correct figure.

Here KF argues that my 20% claim must be wrong because KF can find documentation which can support arguments for a higher figure, and that I apparently should be scientifically dishonest *for that reason*. At best, however, this is a professional dispute and the argument is thus problematic.

Furthermore it is worth noticing that I actually *have* discussed the claim that 50% of the forest has been cleared (pp. 16-7):

Likewise, WWF in 1997 issued a press release entitled 'Two-thirds of the world's forest lost forever.' Both here and in their Global Annual Report 1997, they explained how 'new research by WWF shows that almost two-thirds of the world's original forest cover has been lost.' This seemed rather amazing to me, since most sources estimate about 20 percent. I therefore called WWF in England and spoke to Rachel Thackray and Alison Lucas, who had been responsible for the press release, and asked to see WWF's research report. All they were able to tell me, however, was that actually, no report had ever existed and that WWF had been given the figures by Mark Aldrich of the World Conservation Monitoring Centre. Apparently, they had looked at some maximum figures, and because of problems of definition had included the forests of the northern hemisphere in the original overview of forest cover, but not in the current one.

From this non-report, WWF tells us that: 'now we have proof of the extent of forest already lost... The frightening thing is that the pace of forest destruction has accelerated dramatically over the last 5 years and continues to rise.' The UN, however, tells us that the rate of deforestation was 0.346 percent in the 1980s and just 0.32 percent in the period 1990-95 – not a dramatic increase in pace, but a decrease.

WWF confides in us that nowhere is deforestation more manifest than in Brazil, which 'still has the highest annual rate of forest loss in the world.' In actual fact the deforestation rate in Brazil is among the lowest as far as tropical forest goes; according to the UN the deforestation rate is 0.5 percent per year compared to an average of 0.7 percent per year.

In more recent material, WWF has now lowered their estimate of original cover from 8,080 million hectares to 6,793 million hectares (some 16 percent), while they have increased their estimate of the current forest cover from 3,044 million hectares to 3,410 million hectares (some 12 percent), although their current estimate is still some 100 million hectares lower than the UN estimate. This means that WWF has lowered its estimates from 62.3 percent to 49.8 percent of the earth's forest that have been lost.

Still, this is much more than the 20 percent commonly estimated. However, two independent at the University of London and the University of Sussex have tried to assess the sources and data used by WWF, the World Conservation Monitoring Centre and others in making such gloomy estimates of vast forest reductions. Considering the enormous amount of data, they have they have focused on the assessment of forest loss in West Africa, a place WWF/WCMC estimates a forest loss of 87 percent or some 48.6 million hectares. However, when looking at the documentation, it turns out to be based mainly on problematic bio-climatic forest zones, essentially comparing today's forest with where there may have been forests earlier. In general, the researchers find that 'the statistics for forest loss in general circulation today massively exaggerate deforestation during the twentieth century.' The result is that for West Africa the actual deforestation is about 9.5-10.5 million hectares, or about five times less than what is estimated by WWF/WCMC.

In the end KF refutes my 4 references with a somewhat remarkable contention: 'all of which are secondary literature, and of which several works are known to exhibit a certain lopsidedness.' Without further documentation this seems to be an absolutely unfounded contention. KF's allegation of dishonesty is altogether obviously unfounded.

Breach no. 4

The problems described above reappear when we reduce our field of vision to comprise only tropical forest. Concerning tropical forests, Lomborg says (p. 114): "Although precise figures are not available, the Conservation Union World, the IUCN, estimates that 80 percent of the original forest cover is still in place. Within historical times, then, just about 20 percent of all tropical forests have disappeared. Compared with the developed world, where we have cleared almost half of our forest, this is a relatively small figure."

However, the source which Lomborg refers to as saying that 80% of the tropical forests remains, is not reliable. The figure stems back from a chapter in a symposium report where a non-expert on the subject comes up with a rather loosely based estimate. This is definitely not an official IUCN assessment. The assessment is based on an elementary error, as the remaining area of all tropical forest, dry as well as humid, has been calculated in relation to the supposed original area of *rainforest*. Thus, the figure is utterly useless. Still this figure is Lomborg's only source on this point, and in his reference (note 812) he even adds: "Several sources state that we should have lost more than 50 percent of the rainforest . . . Unfortunately, there are no references." One of the mentioned sources is a WWF home page where literature references are of course not generally made.

It is curious that Lomborg himself can believe such a figure, since he elsewhere acknowledges FAO's estimate that throughout the 1980s, 8% of all tropical forests was cleared, and throughout the 1990s 7%. Thus, altogether 15% of all tropical forest has been cleared only since 1980. How is it then possible to believe that since the dawn of the world only 20% has been cleared?

It is hardly possible to calculate how much of the original tropical forest (wet + dry) is left. But when rainforest is concerned, it is possible due to the fact that the original area may be estimated from climatic data. As mentioned above, a WRI report already some years ago estimated that until now, 49% of the original rainforest has been cleared, and as mentioned Lomborg is familiar with this report, as he has cited it elsewhere in the same chapter.

When he employs a quite different figure from an unreliable source instead, and only that, the conclusion must be that he is writing in bad faith.

Again the critique rests on the large figures from WWF among others. They have already been discussed and I have given my reasons for the view that they overestimate the forest destruction considerably. Furthermore, the new FAO figures from 2001 do not seem to substantiate a 15% tropical forest destruction but rather somewhat less than 10% (p.113,1). It is not evident why KF finds that the 20% estimate cannot be used at all. And again, this is a matter of professional dispute at best, but as an allegation of dishonesty it is obviously unfounded.

Breach no. 5

In his examination of how much forest has disappeared in historical times in various parts of the world, Lomborg says (p. 112): "Southeast Asia, on the other hand, has only lost 7 percent over the last 300 years."

The incredibly low figure of 7% cannot possibly be true. Based on data annexed to FAO's most recent report it may be calculated that in Indochina + Indonesia, the forested area is annually reduced by 1.1%. The 7% would thus be attained in just 6 years. The same data show that the total forest area in this region comprises 46% of the total land area. It

must be assumed that almost all of the region has been forest covered, thus the forest loss must be around 50%. The above-mentioned WRI report states that 66% of the original area covered by rainforest has disappeared in South and Southeast Asia.

Lomborg's figure of 7% also appeared in the Danish edition of his book, and here it could have been a misprint (for instance the intended figure might have been 67%). But the figure has been continued in the English book, so it could not simply have been a misprint. At best, it is a case of sloppiness.

Here KF deviates from his allegation of dishonesty and calls it merely sloppiness. It is understandable that KF cites a smaller complaint as the central claim seems to be: 'this incredibly low figure of 7% cannot possibly be true.' I find it surprising that KF sees no reason to actually check my reference. For the benefit of DCSD and KF, I hereby enclose the reference, from which it clearly appears that I have quoted the researcher in question correctly. The count appears obviously unfounded.

(I have to point out to the committees that now when I have returned to this source I see that I ought to have made a note to my discussion of European forest destruction which I estimate at 50-70%, but which this source estimates at 7.8%. It is, however, worth noting that I here go against KF's general allegation of use of sources against less dubious information.)

Breach no. 6

Concerning the rate of deforestation throughout the 1900s, Lomborg tries to play down the negative tendency by employing a very broad definition of the term forest. FAO operates for one thing with the term "forest", which means areas at least 10% covered by canopy, and for another with "woodland", where less than 10% is covered by canopy, but which still contains trees with intact trunks. Lomborg presents some long time series from 1948 and 1961, respectively, for the total area of "forest + woodland", based on information given by the individual nations. These two time series dominate his figure 60 (p.111). And based on these time series, he concludes on the same page: "With the longest data series, global forest cover increased from 30.04 percent of the global land area in 1950 to 30.89 percent in 1994 . . .". Thus, he concludes that the forest area has increased slightly.

The problem about these figures lies within the definition of "forest". If the canopy cover is reduced from 100% to 11%, then it is still "forest". If the canopy cover now declines below 10% to for example 1%, then it is "woodland", but it is still included in the statistics and thus no change is apparent. Lomborg briefly comments on this problem (p.111), but concludes that his data are "the best information on the global forest area", or, in the legend: "Data availability is poor but by far the best available". This judgement is hardly shared by many others. In his note 767, Lomborg mentions that his text concerns "forest and woodland" up to 1994, but does not explain what "woodland" is, which implies misleading the readers.

Usable sequences of numbers covering what we may in fact call forest (FAO's definition of "forest" with 10% canopy cover) are only available from 1980 onwards. Lomborg's figure 69 also contains FAO's graphs regarding "forest" from 1980 onwards. It is apparent that in the overlapping period (1980 to 1994), the curve rises for "forest + woodland" (marked "FAO database"), while the curve for "forest" is steadily declining. During the time period where we are able to check whether the "forest + woodland" curve shows something meaningful, we thus find that it does not. Since it is based on rather

questionable data, only little importance should be attached to it. But in Lomborg's presentation, his graph is absolutely dominated by "forest + woodland", and the text is similarly focused in order to give the impression of a positive situation. Since Lomborg very well knows that the forest area is in fact diminishing, this is a case of manipulation, i.e. misleading of the readers.

Basically the allegation is that, 'Since Lomborg very well knows that the forest area is in fact diminishing, this is a case of manipulation, i.e. misleading of the readers.' This is naturally a correct conclusion (*if* I know I am lying, then naturally I am dishonest), but an empirically empty statement. I actually mean what I say in the book and honestly try to emphasise that the longest data series probably gives a clearer impression of development of the global forest area. At best this is a matter of professional dispute, but it is clearly unfounded as a complaint of dishonesty.

Moreover, it appears to me that KF here is rather biased. KF states that I should conclude that 'the forest area has increased a bit', despite the fact that I give two long periods of time which show a small increase and a small decrease and conclude that 'since World War II [forest] area has not changed much' (p.117,2).

KF states that the two long 'periods of time dominate his figure 60 p.111' as if this constitutes a case of manipulation – this is not necessarily the case when time is shown along the X-axis.

At the same time KF claims that Lomborg does not explain 'his readers what "woodland" is which means that the readers are being misled' – but this is simply incorrect. In footnote 770 (p.375) I cite all three definitions of forest.

Summing up, this breach is also obviously unfounded.

Breach no. 7

Concerning how much of the remaining tropical forest is being cleared annually, Lomborg provides the following text concerning the development during the past 20 years (p. 113): "The usual FAO estimates put net deforestation in the tropics in the 1980s at 0.8 percent a year, declining to 0.7 percent in the 1990s. With the new 2001-study by FAO based on accurate satellite imagery, the estimate has declined even further to 0.46 percent."

I think that almost anyone reading this will perceive the text to mean that the clearing rate was 0.8% during the 1980s, 0.7% during the '90s, and 0.46 around the year 2000, i.e. a steadily declining trend. Only if you consult Lomborg's note 801, and consider what it says, you find that the situation is quite different. All of the figures (given as absolute areas being lost per year) are found in one report, FAO's report from 2001, which I also referred to earlier.

From this report, the following data on how much tropical forest is annually cleared, may be deducted:

	1980s	1990s
Ordinary inventory	0.8 %	0.7 %
Satellite data	0.47 %	0.46 %

The "ordinary" inventory was used in FAO's earlier reports from the 1990s, and it is based on observations on the land surface, whereas "satellite data" are based on photos from space of selected, representative sample areas. Possibly these satellite data are the most reliable, but this is not for certain. More accurate observations of vegetation types may be carried out on the land surface than from space. On the other hand, land-based

observations may be lopsided in favour of localities near human settlements. It belongs to the picture that others think that even FAO's ordinary data regarding forest clearing are too low.

Thus, two different estimates exist regarding the development in time, and the two data series may not be directly compared. But by providing the data in the sequence 0.8 – 0.7 – 0.46, Lomborg is able to arrange the figures in such a way that it looks like a regular decline.

Belonging to the picture is the fact that FAO provides an uncertainty factor on the numbers (approximately 15% uncertainty), and on this background FAO clearly concludes that the trend of declining clearing rates is not significant.

A statistician like Lomborg ought to note that a significant change is not concerned here, and inform his readers about it. The ethical standards of science also dictate that you remember to present information which speaks against your own thesis. Lomborg does not do that. Instead, he arranges the numbers in a way that seems misleading and which suggests a strong trend, where there is no significant trend.

KF finds me dishonest because he thinks most people would read another meaning from my text than the apparent one which is also supported by the footnote (#801). On its own this is an extremely vague argument even for sending me an e-mail suggesting a change of the text, and naturally obviously unfounded as an argument for me being dishonest. Moreover, it is conspicuous that KF starts his quotation of my text three sentences after the contextual meaning begins, which makes a misinterpretation considerably easier:

The well-known biologist Norman Myers estimated as recently as the early nineties that 2 percent of all forest was being destroyed every year and believed that by the year 2000 – in just nine years at the time of his prediction – we would have lost about a third of the tropical forest area. Actually, he claimed that 'in just another few decades, we could witness the virtual elimination of tropical forests.' Estimates in the same range of 1.5-2 were common among biologists. Today we know that these estimates went way over the mark. The usual FAO estimates put net deforestation in the tropics in the 1980s at 0.8 percent a year, falling to 0.7 percent in the 1990s. With FAO's new 2001-study, based on accurate satellite imagery, the estimate of net tropical deforestation has declined even further to 0.46 percent.

Here it clearly says that

1. High Estimates of forest clearance were common among biologists
2. Today we know that these estimates went way over the mark
3. The usual FAO estimates were 0.7-0.8
4. The new FAO estimate is even lower

It is evident that the discussion is about estimates over the last 10-20 years. It is also very evident that the decrease mentioned in the last sentence (which is what KF's complaint is directed at) is about the *estimate* ('the estimate ... has declined even further') and not rate of forest destruction. This is emphasised further by the summary in the conclusion (p.117,2): 'Tropical forests are being deforested, though on levels much below the feared 1.5-4.6 percent per year – the most recent data

from FAO indicate an annual rate of 0.46 percent.’ Here is clearly stated that I speak of the fact that the *estimate* has been decreasing.

Therefore KF’s allegation of dishonesty is obviously unfounded.

Breach no. 8

Lomborg argues that the forests are best conserved by ensuring economic growth. He says (p. 117): ”Exploitation is due both to individual poverty and to poor government finances. Both problems are really rooted in poor economic conditions, and solutions therefore need to include solid, economic growth, in order to ensure that, in future, developing countries will be able to afford the resources to establish a broader perspective on forest development.” He provides no references supporting that economic growth should enable less deforestation – and you might as well suppose the opposite, i.e. that increased economic growth would lead to increased deforestation.

FAO’s report in fact throws light on this question. In the report’s table 4, some parameters such as the gross national product (GNP/capita), have been correlated with the rate of change in forest area for a number of countries. None of the parameters show any significant correlation with the deforestation. The correlation between GNP and deforestation is + 0.21, and this is not significant. When Lomborg read the report, he must have seen this, and when he argues that the best way of conserving the forests over the long term is to support economic growth, he thus argues in bad faith.

Here KF finds that because I write that higher income leads to less deforestation or reforestation then I am dishonest because ‘you might as well suppose the opposite, i.e. that increased economic growth would lead to increased deforestation’ and because FAO in their non-weighted analysis does not find a statistically significant correlation between deforestation and GNP.

This is wrong. I do not refer to the FAO survey, but if I did then the correlation is actually *positive* (i.e. the claimed correlation), and as this is a population survey (ostensibly covering all countries) it does not make sense to make a significance test (because it would entail an assumption of a theoretical distribution of countries, which does not exist) – there does exist a correlation in this world we live in of +.21. [If you choose to interpret the correlation as a manifestation of a theoretical correlation in many worlds a traditional measurement of significance would still be rather conservative, and therefore still not necessarily refute the finds. Finally, it is astonishing that data have not been weighted, which undoubtedly leads to a far larger variation – which makes it harder to obtain significance.]

KF thus cites the argument for there being a provable correlation between higher income per inhabitant and less deforestation in this world. KF’s allegation of dishonesty is not only obviously unfounded but also directly in support of my contention.

Breach no. 9

On p.116 Lomborg treats the issue of forest fires, and especially the contention that in the el niño year of 1997 unusually large forest areas caught fire in Indonesia. He argues that the catastrophe was not nearly as big as it was made out to be, and he gives two different estimates of the magnitude of forest fires in Indonesia; one is an official governmental

estimate of around 200,000 hectares, the other a larger number close below 1.3 Mha based on satellite data. He adds that an independent expert said that "there is no indication at all that 1997 was an extraordinary fire year for Indonesia or the world at large." Further down on the same page, Lomborg makes this conclusion his own as well.

This account has been disputed in *Scientific American* by the biologist Thomas Lovejoy. Lovejoy writes: "Lomborg's discussion of the great fire in Indonesia in 1997 is still another instance of misleading readers with selective information. Yes, the WWF first estimated the amount of forest burned at two Mha, and Indonesia countered with official estimates of 165,000 to 219,000 hectares. But Lomborg fails to mention that the latter were not in the least credible and that in 1999 the Indonesian government and donor agencies, including the World Bank, signed off on a report that the real number was 4.6 Mha." The report in question is explicitly mentioned in the list of references in a recent article in *Nature* where the 1997 fires are labeled "The largest fire disaster ever observed". Lomborg may possibly have overlooked this information, but we do know that he has read the 2001 FAO report which contains an entire chapter in four sections with a clear headline "Forest fires". Lomborg must have noticed that.

In this chapter it is mentioned that the fires in 1997 were unusually extensive, and it says that "Fires were widespread in Indonesia in 1999 and 2000, but not on a scale comparable to 1997-98." Lomborg's contention that the fires in 1997 were just on the usual level is thus not true. Further down the report says: "Comprehensive global statistics on wildland fires required to make a reliable comparison of global fire occurrence in the 1980s and 1990s do not exist." This is inconsistent with what Lomborg says, i.e. that the fires in the USSR and China during 1987 were much more extensive than those of 1987. FAO says that data which may permit such a comparison do not exist.

Again the conclusion is that Lomborg is speaking in bad faith.

Regarding Lovejoy I have already made an answer on the internet which I presume KF has seen. Basically I do not find it trustworthy that a report which was included as a tie-in with a loan from the World Bank should give the most reliable estimate of Indonesian forest fires (it is relatively unrealistic that Indonesia in their economically weak situation would have opposed any estimate, and thereby running the risk of not getting a loan). Again, this is a professional discussion at best, and to claim that this should constitute any form of dishonesty is obviously unfounded.

Then KF writes that I should have read the FAO report, and that this should contradict my two points. To begin with because FAO writes that 'fires were widespread in Indonesia in 1999 and 2000, but not on a scale comparable to 1997-1998.' From this KF concludes that it is wrong to claim that the Indonesian forest fires in 1997-98 were not extraordinary. [It may be worth emphasising that I am only referring to the fact that Goldammer is of this opinion, and not that it is true, which is also why KF does not quote the passage, but in this connection I would not mind defending Goldammer against KF.] This is naturally a fallacy. Just because 1999-2000 saw fewer forest fires than 1997-98 you *cannot* conclude that 1997-98 was a special year. [It would correspond to 50 cm of rain falling in a typical year, also in 1997-98, that 40 cm of rain fell in 1999-2000, and that KF because of this concludes that 1997-98 was an especially rainy year.]

Finally KF does not contest the validity of my claim that the forest fires in 1997-98 were much smaller than the single fire in China/USSR in 1987. [KF writes incomprehensibly that 'This is inconsistent with what Lomborg says, i.e. that the fires in the USSR and China during 1987 were much more extensive than those of 1987' and I presume that this is a typist's error and that it should have read 1997 in the end.] The argument is that FAO writes: 'Comprehensive global statistics on

wildland fires required to make a reliable comparison of global fire occurrence in the 1980s and 1990s do not exist.’ This is another fallacy. Just because we do not have comprehensive global fire statistics to compare forest fires in the 1980s with the forest fires in the 1990s it does not mean that we cannot make individual comparisons. [KF’s argumentation corresponds to saying that because we do not have sufficient data to compare the precipitation in the entire world over the last 20 years, then we cannot determine whether one amount of precipitation has been larger than another.] I am only comparing the WWF survey of all forest fires in 1997, which could probably be considered a maximum estimate (with this survey WWF tried to document that 1997 was the ‘year when the world burst into flames’). This I compare to a survey of *one single fire* in China/USSR which was much larger (p.116):

Altogether, however, the WWF’s figures are nowhere near the 2.4-3.6 million hectares of forest that burned on just the Indonesian part of Borneo in 1983-4, and the figure is well below the 13 million hectares that burned in China and the former USSR in 1987. Actually, it is estimated that fires each year burn some 10-15 million hectares of boreal and temperate forests, 20-40 million hectares of tropical forests, and up to 500 million hectares of tropical and subtropical savannas, woodland, and open forests. Russian forest fires alone are estimated at about 12 million hectares each year. In conclusion, 1997 was in no way the year in which burned more forests than at any other time in history.

I therefore only conclude that there is data that, with a rather clear margin of error, can show that other years have seen much larger forest fires than the ones in 1997. KF only presents a fallacy, and KF’s allegation of me thus being dishonest is obviously unfounded.

General conclusion on the complaint

With the above I find that KF’s counts in the above only in very few instances can be said to have a suggestion of a professional dispute, however, most often they are based on fallacies, wrong quotes, or incorrect arguments. Far more importantly *none* of the cited counts do even approach suggesting any kind of dishonesty – in some of the cases they even support my points. Therefore I also find that these counts as a whole are obviously unfounded and should be rejected.

General effort against (de facto) harassment

KF, however, brings forward even more contentions and has submitted another letter dated March 4 2002. I therefore find it necessary to ask the committee to pay attention to KF’s political motivation and real intentions, as well as adopting a more general resource view.

In his letter of March 4 2002, KF writes that ‘I did not have any knowledge of the existence of DCSD until February 20, and since I judged it better for the cause if I lodged a complaint February 21 at the latest, at the same time as the closing date of applications for the position as director of the new Environmental Assessment Institute, my complaint was written hastily.’ By using the wording ‘better for the cause’ (an understanding which is also to be seen in the original complaint under the headline ‘the purpose of this complaint’ with considerably more words, however) KF clearly shows that the primary goal of this complaint is political, i.e. avoiding me being employed as the director. (KF sent an additional copy of the complaint to the Ministry of Environment and explicitly asked the ministry to take the complaint into consideration if Bjørn Lomborg was one of the candidates for the new position as director, and at the same time the complaint was published in ‘Ingeniøren’ (‘The Engineer’).)

At the same time KF writes that the new complaint could be but one of many: ‘I would like to emphasise that provided that DCSD cannot draw a conclusion on the basis of the material I have sent it is better if you request further examples of dishonesty than coming up with an unclear

conclusion. The number of possible examples are almost unlimited; what sets the limit is how many I have time and strength to explain.' This underlines that almost no matter how many examples I can reject, KF is ready to send in even more.

The problem here is that despite the obviously unfounded character of the 9 charges that I have treated above, the defense takes up extraordinarily much time. The obvious political motivation along with promises/threats of a constant flow of new charges of an obviously unfounded character raises the question of whether it is fair to continually take up my time and resources in order to refute what more and more seems to be politically inspired de facto harassment.

It is clear that there should be confidence that there has been put a serious and detailed effort into a complaint to DCSD which plainly and logically exposes the alleged dishonesty – especially out of respect for the essential work carried out by the committee. It appears to me that KF has broken this confidence which I have documented above for each of the nine charges. Perhaps it is best illustrated by KF's 'Breach no.5' where it is claimed that I am dishonest, but where KF does not even check my reference, (which – of course – is cited correctly and supports my claim.)

At the same time it should be clear that DCSD cannot have any interest in its method of work being abused by people having political motives and much time.

Therefore I would like to recommend to DCSD that they in this concrete case, in the light of the above made objections against KF's first nine counts, in the light of the obvious ungroundedness of the allegations, in the light of the obvious political motivation, and in the light of the threat of even more allegations, consider the reasonableness of me having to continue answering the many assertions.

In this connexion I would like to recommend to the DCSD that KF's remaining complaint is turned down.

In the alternative

Finally I will explain my position shortly, should the committee against expectations choose to debate the factual aspects of KF's complaint.

I would like to recommend to the committee that you, in a concrete weighing out of a free access to complain and a protection of the accused against arbitrary harassment, select a reasonable amount of counts so that I can debate their factual aspects in progressively more detailed levels while even more counts might be sorted out. In this connexion I would like to ask the committee to please point out the counts in KF's complaint, if there are more to which I ought to adopt an attitude.

At the same time I would like to know whether DCSD finds that the '9 breaches' commented upon in the above have been elucidated sufficiently for the committee to make a decision.

Kind regards,

Bjørn Lomborg