Short reply to *Skeptical Questions, Sustainable Answers*

By Bjørn Lomborg, June 27, 2002

I was asked by Danish Broadcasting Company (DR) to comment on *Skeptical Questions*, which criticizes my book, *The Skeptical Environmentalist* (TSE). DR sent me an advance electronic copy of the book[1] three days ago, so I have only had time to read parts of the book.

In the Danish debate, the same people and organizations published a book against my Danish version. Together with a student I then wrote a complete answer to every claim and published it on the web in a free 185-page book (*Godhedens Pris*, available on www.lomborg.com). The book went through the Danish *Skeptical Questions*, and identified a mass of errors and inconsistencies, showed that the critique was generally either irrelevant or grossly misspecified and indicated how the book encapsulated a general unwillingness to read what I wrote and discuss it on a factual basis.

I am therefore somewhat surprised that the same people have had their criticism translated virtually unchanged, and that my lengthy reply is hardly even mentioned, much less dealt with.[2] Yet, it is worth noting that the lead authors have decided to simply leave out those chapters that I had criticized most, indicating that *Godhedens Pris* have done some good, even if it is not formally acknowledged. At the same time, the critics have added some extra material. **Unfortunately, though this material makes no new points it does manage to make a lot of new mistakes.**

Basically, I do not have access to the resources of the *Danish Ecological Council* to have all my refutations of the Danish edition translated and repeated for the English reader. Yet, as the *Skeptical Questions* will no doubt be widely read, I would like to indicate some of the serious problems still afflicting this translated and updated volume.

As the Danish Broadcasting Company informed me that the three most novel parts of the book was chapter 1, 3 and 11, I have concentrated on these chapters.

The general drift of the argument is that I allegedly should

- make a lot of errors
- manipulate my references
- pick and choose my numbers
- not confront my critics

In the following I will look at their strongest examples from the new parts of their book exemplifying these critiques, showing why I find these critiques rather unconvincing and of low quality.

Making lots of errors

Despite being one of their favorite claims there are fairly few concrete examples of my errors. Kåre Fog (KF) claims that I splice two incongruent data sets on starvation:

> "FAO has two data sets concerning this matter. One set tells that 38 % of Africa´s population *(Sub-Saharan Africa)* were starving in 1970, changing to 43 % in 1991. The other set has the figures that 37 % were starving in 1980, 35 % in 1991, 33 % in 1996, and 34 % in 1998. …
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> In his book, Lomborg combines the two data sets referred to above to give us the impression that the proportion of starving in Sub-Saharan Africa has shown the following trend: 1970: 38 %, 1980: 37

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[1] Being a advance electronic copy, it contains many spelling errors and copy editing comments and revisions, so therefore the quotes below may be slightly different from the final version.

[2] It is indeed only referenced trice, and only in footnotes.

> %, 1991: 35 %, and 1996: 33 %, i.e. that the situation is improving slowly but steadily. Thus, the reader observes a clear statement about seemingly exact figures, and will be misled to think that there is a decline, when in fact the trend is not known." (p203).

Apart from KF incorrectly referring to Sub-Saharan Africa as merely Africa in the first statement, the claim is mistaken since FAO just have one data series for malnutrition (one definition) but have published their estimates at different times where increasing amounts of available information have changed the estimates. These two published data series come from 1996 and 1999 and (as is typical) the latest year of the earlier data series deviate the most (generally because of lack of new information). Actually, I show the graph for *all* the regions (TSE:61), and an analysis show that both the data for the last period deviates the most and that especially Sub-Saharan Africa deviates on the 1991 estimate from 1996. However, and this KF conveniently forgets to tell us, the 1980 data estimates from both published series actually fit fairly well (estimating 41% and 37%, respectively). Thus, I published the data, which now must be considered most correct, the latest data, using the earlier data to supply the earlier 1970 estimate.

It is also curious that KF does not comment that this methodological decision leads to South Asia *increasing* its proportion of malnourished from 34% to 38% from 1970 to 1980, an equivalent percentage point increase to the decrease KF postulates I should have doctored. But of course, such observation would lead away from the preferred conclusion of erroneous data handling.

Likewise, Jesper Jespersen (JJ) allege that according to Lomborg

> "air pollution in developing countries is just a transient phenomenon. It will evaporate, as these countries grow wealthier, as it has (?) in the industrialized countries. But is this all that obvious? Increasing car traffic will leave ever more smog in the streets, especially in cities with high (summer) temperatures. Thus, several studies made by the Danish Clinic of Occupational Medicine have demonstrated that city traffic in Denmark creates an increasing health hazard for bus drivers." (p11).

Of course, one wonders why JJ does not just supply us with facts to show that I'm wrong but instead ask the rhetorical question "is this all that obvious?" But let us take a look at the two claims. First he claims that increasing car traffic will leave ever more smog in the streets.[3] Yet, even in my book I've supplied the data to show this claim wrong. For the UK, emissions of urban road particulate matter (the most dangerous emission, PM_{10}) has been *decreasing* dramatically since 1990 despite much increased traffic, and that this is indeed expected to *keep decreasing* at least till 2010 (TSE:169).

Second, JJ claims "that studies made by the Danish Clinic of Occupational Medicine have demonstrated that city traffic in Denmark creates an *increasing* health hazard for bus drivers,"[4] unfortunately without supplying a reference. However, when you check all the available references on PubMed[5] *none* have time series results, and thus they *cannot* support the claim of increasing air

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[3] It is initially unclear whether JJ refers to cities in the developing or developed world, but given that the next statement starts off with a "thus, … in Denmark" it must be a statement on the developed world.

[4] Italics added.

[5] Loft S, Poulsen HE, Vistisen K, Knudsen LE. "Increased urinary excretion of 8-oxo-2'-deoxyguanosine, a biomarker of oxidative DNA damage, in urban bus drivers." Mutat Res. 1999 Apr 26;441(1):11-9. Knudsen LE, Norppa H, Gamborg MO, Nielsen PS, Okkels H, Soll-Johanning H, Raffn E, Jarventaus H, Autrup H. "Chromosomal aberrations in humans induced by urban air pollution: influence of DNA repair and polymorphisms of glutathione S-transferase M1 and N-acetyltransferase 2." Cancer Epidemiol Biomarkers Prev. 1999 Apr;8(4 Pt 1):303-10. Autrup H, Daneshvar B, Dragsted LO, Gamborg M, Hansen M, Loft S, Okkels H, Nielsen F, Nielsen PS, Raffn E, Wallin H, Knudsen LE. "Biomarkers for exposure to ambient air pollution--comparison of carcinogen-DNA adduct levels with other exposure markers and markers for oxidative stress." Environ Health Perspect. 1999 Mar;107(3):233-8. Soll-Johanning H, Bach E, Olsen JH, Tuchsen F. "Cancer incidence in urban bus drivers and tramway employees: a retrospective cohort study." Occup Environ Med. 1998 Sep;55(9):594-8.

pollution health hazard. Moreover, this would also be contrary to all the available evidence from the OECD countries,[6] data that is also supplied in *The Skeptical Environmentalist* (chapter 15).

JJ also asserts that my data is wrong or irrelevant in the case of pesticides:

> "Lomborg still concludes on p. 248 that if all pesticides were removed from food production, 'it would probably also mean that we can avoid some twenty deaths a year'. A fairly precise number, based on very scant *historical* substance." (p13).[7]

He goes on to mock me for basing these 20 cancer deaths on *historical* data, since it will clearly take a long time for the pesticides to work their way into our food, our bodies and to kill us. Thus, my statement is an expression of an ill-advised rear-view mirror strategy.

However, JJ evidently missed the rest of the pesticide chapter, since it is clearly stated several times that the 20 cancer deaths is based on extrapolation from rodent carcinogenity tests.[8] These tests obviously give uncertain estimates, as do all cancer estimate procedures, but they are *not* backward looking. (Moreover, I severely doubt that it would even be possible to go back in time to document just 20 annual deaths in the US, this being *much* too low for detection.)

Much more often than claiming I make specific errors (perhaps because most of the figures come from UN and other respected sources) *Skeptical Questions* argues that my claims just don't make sense. For instance, JJ claims that:

> "Global averages are misleading indicators of the 'real state.' … What is the sense of a statement such as, 'We have seen a global reduction of people living in poverty', when it covers a dramatic deterioration in Africa, a growing number of street orphans in Brazil, more unemployed people in Indonesia, and heavily reduced old age pensions in Russia, outweighed by fewer hungry people in China?"[9] (p9)

First of all, the quoted statement does not occur anywhere in *The Skeptical Environmentalist*, [10] though I do point out several times that the poverty incidence in the third world has been declining. Second, JJ asks the rhetorical question 'what is the sense?' to make the statement less clear, but it is really obvious: The proportion of people living in poverty has been reduced, or to put it differently, ever more people are not living in poverty compared to the number of people living in poverty. This seems to me to be an entirely sensible statement. Moreover, it is also a statement, which the UN has used: "In the past 50 years poverty has fallen more than in the previous 500."[11] Does JJ also object to this usage?

Finally, there is a methodological and moral problem in JJ's quote. Of course, what he means to indicate is that just simply because things *in general* is getting better (more and more people being lifted out of poverty) does not mean that we should forget about the people that are still left behind. I find this to be an important and a morally decent point. This is what I have pointed out time and again in the book, even in one of the first headlines: "Things are *better* – but not necessarily *good*."[12] I write it even more clearly in the opening of the concluding chapter of my book:

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[6] "Air quality in OECD countries is vastly improved" as the World Bank concludes, TSE:175.
[7] Itallics added.
[8] These are described in depth in TSE:231ff.
[9] In the somewhat hurried article JJ writes "hungry people in China" though he evidently must mean "poor people in China."
[10] This perhaps explains the lack of reference in JJ.
[11] Quoted in TSE:71.
[12] TSE:4. Unfortunately, it seems that JJ must have missed this pervading point of the difference between better but still problems, as he writes somewhat unsuccessfully sarcastic: "If Lomborg had trusted his own conclusion, 'Things are getting better', then he had hardly needed to write another 110 pages on 'Tomorrow's problems." (p13)

> "On the global level, it seems obvious to me that the major problems remain with hunger and poverty. Although we have witnessed great improvements both in feeding ever more people, ever better, and bringing ever more people out of poverty, and although these positive trends are likely to continue into the future, there still remain some 800 million hungry people and some 1.2 billion poor people in this world. In terms of securing a long-term improvement of the environmental quality of the developing world, securing growth so as to lift these people out of hunger and poverty is of the utmost importance, since our historical experience tells us that only when we are sufficiently rich can we start to think about, worry about and deal with environmental problems." (TSE:327).

However, while it is important to acknowledge that there are still problems, it does not justify one to reject understanding the overall trend of ever more people lifted out of poverty. Doing so, exactly by naming such a large number of potentially worse off people risks missing the forests for the trees, and also adds to our common Litany of an ever deteriorating world.

We see this problematic argument repeated in JJ:

> "We can only elucidate global problems with global figures', which would sounds reasonable if all problems were shared equally among countries; enough on the face of it – after which Lomborg proceeds, 'If we hear about Burundi losing 21 percent in its daily per capita caloric intake over the past ten years [that could create] information overload.... The point is that global figures summarize all the good stories as well as all the ugly ones. On average, however, the developing countries have increased their food intake from 2,463 to 2.663 calories per person per day over the last ten years.' (p. 7). Yet, the increased food production in China will never feed the mouths of those starving in Burundi, meaning that such an aggregate figure is irrelevant. Moreover, there are lots and lots of instances of starvation and under nourishment in countries that, on paper (i.e. the national average) could supposedly feed its entire population. Here, Amartya Sen's studies of conditions in India make instructive reading. Sustainable development is also about limiting local collapses, which is blurred by global averages." p10.

Surprisingly, JJ quotes me somewhat out of context, because I *do* actually discuss the issue of increased food intake for different nations:

> "In the same way we can only elucidate global problems with global figures. If we hear about Burundi losing 21 percent in its daily per capita caloric intake over the past ten years, this is shocking information and may seem to reaffirm our belief of food troubles in the developing world. But we might equally well hear about Chad gaining 26 percent, perhaps changing our opinion the other way. Of course, the pessimist can then tell us about Iraq loosing 28 percent and Cuba 19 percent, the optimist citing Ghana with an increase of 34 percent and Nigeria of 33 percent. With 120 more countries to go, the battle of intuition will be lost in the information overload. On average, however, the developing countries have increased their food intake from 2,463 to 2,663 calories per person per day over the last ten years, an increase of 8 percent.
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> The point is that global figures summarize *all* the good stories as well as *all* the ugly ones, allowing us to evaluate how serious the overall situation is. Global figures will register the problems in Burundi but also the gains in Nigeria. Of course, a food bonanza in Nigeria does not alleviate food scarcity in Burundi, so when presenting averages we also have to be careful only to include comparable countries like those in the developing world. However, if Burundi with 6.5 million people eats much worse whereas Nigeria with 108 million eats much better, it really means 17 Nigerians eating better versus 1 Burundi eating worse – that all in all mankind is better fed. The point here is that global figures can answer the question as to whether there have been more good stories to tell and fewer bad ones over the years or vice versa." TSE:7.

Moreover, JJ's claim that "there are lots and lots of instances of starvation and under nourishment in countries that, on paper (could supposedly feed its entire population," seems to suggest that just stating the average caloric intake neglects all the people who are starving. But of course I also discuss this:

> "The calorie figure is, nonetheless, an average. It is not unthinkable that the figure conceals the fact that some people live better lives while increasing numbers of others just manage or even starve." TSE:61.

I go on to point out how malnutrition in the developed world has declined from 35% in 1970 to 18% in 1997,[13] and that is has probably been declining from about 50% in 1950 and will continue to decline to about 6% in 2030.[14] JJ's claim stands totally unsubstantiated.

Actually and right after, JJ summarizes that

> "Figures on global food production (e.g. Figure 2, p. 9) used to evaluate sustainable development are therefore misleading." (p10)

But if you check my Figure 2, p. 9,[15] you will see it does not at all show global food production but instead global and regional *grain yields*. Embarrassing.

Perhaps the lack of good documentation could be the reason why JJ shows such scorn for the truth:

> "It is completely unacceptable for someone in an academic environment to 'monopolise Truth'. Presumably, this is among the very first things any university teacher will instil in his or her undergraduates (or should I say 'ought to instil'?): that we shall never get anywhere near 'The Truth'." (p9)

First of all, it seems really strange to claim that I should try to 'monopolize' truth, since I've openly and clearly laid out all my sources and claims for others to refute. Actually, the book project of *Skeptical Questions* itself shows that anyone can participate (if with varying degrees of success) in the discussion of the real state of the world.

Second, it seems somewhat disturbing that a scientist, in claiming that I am wrong, have to resort to asserting that we will never get anywhere near the truth. While such a statement naturally relieves JJ of any burden of proof, it also negates the very essence of western science, which tries to get an ever more encompassing understanding based on not-falsified facts and theories.

Manipulating the references

Many times throughout the *Skeptical Questions*, I get accused of misusing my references. Let us take a look at some of their claims. Anders Christian Hansen (ACH) claims that my reference of IPCC is flatly wrong:

> "Matters get even more muddled when Lomborg says that a societal interest of 4-6 per cent '...actually means that we are making sure we administer our investments sensibly so that future generations can choose for themselves what they do – and do not – want'. (p. 314) A viewpoint which, once more, Lomborg underpins with a reference to the IPCC report – and another case of 'borrowed plumes', since no such argument is found in the report!" (p14-15)

Yet, see what I wrote:

> "These [previous] arguments indicate that it is probably reasonable to have a discount rate of at least 4-6 percent. But it does not mean … that we are saying to hell with future generations. It actually means that we are making sure we administer our investments sensibly so that future generations can choose for themselves what they do – and do not – want. (Note 2647: Wildavsky cited in IPCC 1996c:133.)" (TSE:314.)

And here is the quote from IPCC 1996c, page 132-3:

[13] TSE:61.
[14] TSE:24.
[15] It is the only figure on page 9, so there is no misunderstanding possible.

> "Over 100 years, an investment at 5% returns 18 times more than one at 2%. Thus, where some redistribution of future returns is possible, society would be foolish to forgo a 5% return for a 2% return. …
>
> Wildavsky (1988) explains the point in the context of health and safety regulations:
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>> Insofar as we today should consider the welfare of future generations, our duty lies not in leaving them exactly the social and environmental life we think they ought to have, but rather in making it possible for them to inherit a climate of open choices – that is, in leaving behind a larger level of general fluid resources to be redirected as they, not we, see fit."

It should be obvious, that Wildowsky exactly point out that we should leave the choice of actual consumption to future generations and that we should chose high returns over low returns. ACH's claim that I misquote the IPCC is clearly wrong.

Likewise, ACH write that I should claim that the Kyoto agreement is going to cost us precisely 1.5% and that I neglect other, lower cost estimates:

> "However, according to Lomborg, the share of GDP that the Kyoto agreement is going to cost us is 1.5 per cent. (TSE:303) This calculation assumes an agreement that completely disallows quota trade. Moreover, it is related to GDP in the year 2000. To be sure, such agreement has not been on the table since the mid-1990s, but of course 1.5 per cent does sound somewhat more forbidding than 0.13 per cent." (p46)

Yet, ACH is less than faithful to my text in his quote. Here is what I actually wrote:

> "The cost of the Kyoto Protocol is depicted in Figure 158. If no trade is allowed the cost is estimated at $346 billion a year around 2010. That is equivalent to about 1.5 percent of the region's present GDP. If trade is allowed within the Annex I countries, the cost drops to $161 billion annually. If trade is only allowed within two blocks of Annex I (EU and the others), then the cost increases to $234 billion. However, a large share of the cost is borne by the EU, since it is cutting itself off from the benefits of trade, whereas the US, Japan and the others will actually fulfill their Kyoto targets more cheaply, because they will not have to compete with the EU in buying emission permits. Finally, if global trade were an option (a problematic assumption, as we will see shortly), the cost could be cut even further to $75 billion." (TSE:303)

Here I clearly indicate that there is a range of costs depending on the trading, not just one option. It is remarkable that ACH does not find it necessary to even indicate this. Moreover, my estimates use an average of all the major models, ensuring that we act on the background of neither the most optimistic nor the most pessimistic model.[16] This clearly is a more robust estimate than that of a single model, which however gives ACH a result he seems to prefer. (It doesn't help either, that ACH's *only* reference for this model result is nowhere to be found in his literature.)[17]

KF criticize me for only quoting the results of an article when they allegedly are favorable to my preconceived notions:

> "A cornerstone of Lomborg's argumentation is that when we give priority to economic growth, we will better be able to afford doing something for the environment. In support of this idea, he writes about the socalled Kuznets curves which tell that when economic welfare has reached a certain level,

[16] "In 1999, economists representing 13 different models were assembled by the Stanford Energy Modeling Forum to evaluate the Kyoto Protocol, by far the largest effort to look into the costs of Kyoto. Half the models were American, half were from Europe, Japan and Australia. Since these models of course have different assumptions about future growth, energy consumption, alternative costs, etc., their findings often diverge by a factor of 2-4. However, they generally found much the same picture in relative terms. Moreover, since each scenario was estimated by many models, the figures reported here are the averages – representing neither the most optimistic nor the most pessimistic model." TSE:303)

[17] ACH only refers to this model in endnote 15: [Burniaux, 2000 #51], on page 54.

> the environment starts to improve. He cites a study where BNP has been studied in relation to 10 indicators of the environment. He tells us that two of these indicators did show a Kuznets curve. But he omits to tell his readers that in the 8 other indicators such a relationship could not be demonstrated." (p204-5)

This however is manifestly incorrect, as KF could also have checked for himself. I do show the two important graphs for air pollution (SO_2 and particulates) where the Kuznets connection is clear (TSE:177) but I *equally* show how the connection is *opposite* for water (TSE:202) and waste (TSE:206).

Pick and choose my numbers

A lot of scorn and criticism is piled on professor Nordhaus of Yale University, because I use his models, despite him being the most prominent figure in the integrated modeling community and despite his models giving the same basic conclusions on global warming.

Here is a typical quote from ACH, indicating that I've merely picked my numbers from a highly unreasonable model by Nordhaus and Boyer:

> "On the whole, it would be quite hard to convince readers that the somewhat academic premises, on which Nordhaus and Boyer based their calculations, are realistic. For instance, … they assume that potentially disastrous climate changes resulting from a doubling of the atmosphere's CO_2 content would merely result in a 1 per cent loss of the world's total incomes. … Given such assumptions, we do not really need to carry out the calculations to know the result." (p44-5)[18]

Clearly, a model that only assumes 1% GDP loss at a catastrophic outcome must seem ridiculous, and clearly this is why it generates the outcome of advocating only moderate CO_2 reductions. However, ACH has problems citing Nordhaus and Boyer correctly. Actually, they assume that disastrous climate changes resulting from a doubling of the atmosphere's CO_2 content would result in **not 1% but 22-44% loss** of the world's total income.[19] However, Nordhaus and Boyer also include the expert assessment of the *risk* of catastrophic climate change – catastrophic climate change is actually not estimated to be very likely. Despite seriously increasing the expert assessed risk[20] the net impact on the world, given that catastrophic climate change will almost surely not happen, is then estimated to 1-7% of GDP.[21] (Notice how ACH only gives us the low figure, presumably to increase the ridicule.) Thus, ACH in two ways seriously misstate the model when claiming that catastrophic climate change is modeled to only cost 1% of GDP.[22]

Shortly thereafter, ACH claims that my entire point with the climate change chapter of the book is based on sand, and other estimates I cite could support the opposite conclusions:

> When using current methods to calculate the cost of global warming we can arrive at just about any figure we desire. Lomborg estimates, in another chapter,[23] a figure of USD 480-640 billion a year to be the cost of the damage caused by global warming. Apparently, he is unaware that this would amount to a total present value of USD 20-27 trillion. [Note: Using the same global discounting factor as used by (Nordhaus and Boyer 2000) in the calculations of the RICE model.] The point is not that this figure is

[18] I find it curious that given all the critique of me not being sufficiently academic, the same adjective is here used pejoratively against Nordhaus and Boyer.

[19] Nordhaus & Boyer 2000:4:43, see http://www.econ.yale.edu/%7Enordhaus/homepage/web%20chap%204%20102599.pdf.

[20] Doubling the risk, increasing the loss and including risk averseness, Nordhaus & Boyer 2000:4:25.

[21] Nordhaus & Boyer 2000:4:44.

[22] Both neglecting that catastrophic climate change is assumed to cause a much greater impact of 22-44% and when weighted with risk will cost from 1-7%.

[23] Notice that it is not in another chapter – it is actually just 10 pages before Nordhaus & Boyer's model and the $5 trillion is presented, TSE:301.

> ’better’ than the USD 5 trillion; it merely serves to demonstrate that using slightly different assumptions (which Lomborg himself accepts in other contexts), the same calculation would produce precisely the opposite conclusion: With these figures it would more than pay to control global warming in Lomborgian calculus.” (p45)

Here ACH makes two surprisingly blatant errors. First, he claims that the $480-640 billion per year from the IPPC would by the RICE model discounting be estimated at $20-27 trillion.[24] This would also be true, *if the cost of the global warming was already at full strength in 1995 and continued throughout the 21st century*. Making such an assumption should clearly have caused alarm bells to ring, even though ACH had apparently missed that both IPCC and I clearly write that the cost estimate of $480-640 billion is only valid after a doubling of CO_2, or depending on scenario, somewhere between 2060 and after 2100.[25] The magnitude of ACH’s blunder is staggering. If instead the correct damage profile is used, a figure much closer to the $5 trillion comes out.[26]

Second, even if his first assumption was correct, that the true cost of global warming would be $27 trillion, he cannot thereby conclude that it would pay to control global warming, *since whether it would pay or not is a problem decided on the marginal cost and benefit*: the question is not whether global warming will be costly (say, $27T) but whether doing something (at a cost of, say, $5T) will reduce the costs of global warming by more than the cost of doing something (that the cost of the reduced global warming will be, say, either $15T or $25T). If the reduced cost will be $15T, it would indeed have paid off, but if it was $25T it would be a bad idea. However, ACH seems to have compared the marginal cost of an action (say $5T)[27] with the absolute cost of global warming ($27T), an error which typically would be considered terribly embarrassing to make for an economist.

Let us just conclude with another long and problematic critique by KF:

> “Lomborg criticises those who postulate a connection between synthetic chemicals and breast cancer and states that virtually no one dies of cancer caused by organochlorine pesticides such as DDT, lindane or dieldrin. However, there are papers that find a relationship between these pesticides and breast cancer as well as others that do not find a relationship. One of the papers cited by Lomborg lists 11 investigations, of which about half find a significant relationship between organochlorine pesticides, mainly DDT, and breast cancer. But Lomborg does not mention that the paper contains this list. To be unbiased, Lomborg would have had to cite studies of both kinds to the same extent, but he did not. Concerning lindane, there is a clear conflict between reality and Lomborg´s text: “Of the three studies that have examined . . . lindane, none has found evidence for an association with increased risk of breast cancer”. Actually, a Finnish investigation from 1990 showed a tenfold higher risk for breast cancer in persons who had elevated levels of a lindane residue. And, most remarkably, Lomborg cites a Danish investigation as a reference that DDT is of no influence, but refuses to mention that the same investigation found a significant relationship between the pesticide dieldrin and breast cancer. This conflicts with Lomborg´s optimistic view that there is probably no relationship between breast cancer and the pesticide dieldrin.[Note 30: A. P. Høyer et al. (1998): Organochlorine exposure and risk of breast cancer. The Lancet 352: 1816-1820. Lomborg´s argument for omitting the information on dieldrin is that he considers the relationship found to be accidental. His argument for this is that if the relationship between cancer and chemicals is studied, one out of 20 correlations will reach significance at the 5 % level. He counts the many different PCB isomers analysed as different chemicals, and

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[24] By my calculations, the estimate is $15.6-20.8 trillion 1990$, or adjusted to 2001$, $21.6-28.8 trillion.
[25] See the temperature graphs for the major IPCC scenarios, TSE:265.
[26] Depending on assumptions of occurrence of 2.5ºC and of leading up cost, the cost lies somewhere from $1.7-7.0 trillion.
[27] Which of course furthermore cannot be compared, simply because if the numbers stem from a different model, the marginal costs and benefits should also be estimated on this model.

> therefore argues that more than 20 chemicals were analysed. This argumentation is not valid, however, because it would require that the quantities of the PCB isomers varied independently.] " (p205)

However, it would perhaps be worth looking at what I say. I first talk much about the connection between breast cancer and synthetic estrogens. Then I discuss the latest findings by the UK advisory committee and the US National Research Council:

> Nevertheless, the real issue of course is whether synthetic estrogens can be causing breast cancer. Generally, it is correct that the total amount of estrogen a woman is exposed to during a lifetime contributes to cancer.[28] Typically, this hormonal exposure comes from the woman's own body (greater effect due to later first births, earlier menarche, etc.) and from oral contraceptives.[29] The connection between pesticides and breast cancer is thus theoretically based on the idea that some of these pesticides can mimic estrogens, increase the female estrogenic load and cause excess cancer. However, there are several problems with this interpretation.[30] For one thing, DDT, DDE and PCB are weak estrogens and it is known that they can have both a boosting and an inhibiting effect on cancer in animals.[31] For another, high occupational exposure of PCBs and other organochlorines to women does not seem connected to any increase in breast cancer frequency.[32] Third, the incidence of breast cancer has been *increasing* while concentrations of DDT, DDE and PCB in the environment have *fallen*.[33] In the words of the National Research Council: "It seems unlikely that a declining exposure would be responsible for an increasing incidence of cancer."[34]
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> Moreover, a study from the National Cancer Institute of breast cancer incidences for different regions of the US for blacks and whites showed a surprising result. Whereas the white women of the Northeast have higher relative breast cancer mortality rates, the rates for black women in this region is not higher than in other regions. This indicates that "widespread environmental exposures are unlikely to explain the higher relative breast cancer mortality rates observed for U.S. white women in the Northeast."[35]
>
> Already in 1994, a meta-study of the five small, available studies on breast cancer and synthetic estrogens concluded that "the data do not support the hypothesis that exposure to DDE and PCBs increases risk of breast cancer."[36] The National Research Council in its latest review reached the same conclusion.[37]
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> Since then, seven large studies (with more than 100 women) and four smaller studies have been published.[38] In 1999, the British advisory committee on carcinogenicity of chemicals to the UK Department of Health published its conclusions, based on the available studies on breast cancer and synthetic estrogens. For DDT, it found that only two, relatively small studies had found an association, whereas one large study had found a *reverse* association (more DDT, less breast cancer).[39] Thus, in conclusion, the committee stated that "overall, there is no convincing evidence from epidemiological studies for an elevated relative risk of breast cancer in association with DDT."[40]
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> For dieldrin, only two studies had tested the connection, one finding no relationship, the other finding a positive association. However, the study in question had tested 46 different associations,

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28 Hulka and Stark 1995.
29 Hulka and Stark 1995.
30 Safe 1997a, 1998; Davidson and Yager 1997.
31 NRC 1999:243-4.
32 NCR 1999:258ff.
33 See also Crisp *et al.* 1998:23; NRC 1999:263.
34 NCR 1999:263.
35 Tarone *et al.* 1997:251.
36 Krieger *et al.* 1994:589.
37 "Overall, these studies published prior to 1995 do not support an association between DDT metabolites or PCBs and risk of breast cancer." NCR 1999:250.
38 Large studies: Lopez-Carrillo *et al.* 1997; Hunter *et al.* 1997; Veer *et al.* 1997; Høyer *et al.* 1998; Olaya-Conteras *et al.* 1998; Moyish *et al.* 1998; Dorgan *et al.* 1999, and small studies: Sutherland *et al.* 1996; Schecter *et al.* 1997; Liljegren *et al.* 1998; Guttes *et al.* 1998; see COC 1999:5; NRC 1999:251-5.
39 Veer *et al.* 1997; cf. NRC 1999:256.
40 COC 1999:6.

> making it plausible that the single, statistical find was a "chance finding."[41] Moreover, in studies of rats and mice, it has not been possible to show any estrogenic activity of dieldrin.[42] Finally, occupational studies of dieldrin show no excess cancers.[43] Consequently, the committee finds that "there is no convincing evidence from epidemiological studies for an elevated relative risk of breast cancer associated with dieldrin."[44]
>
> Of the three studies that have examined β-HCH and lindane, none has found evidence for an association with increased risk of breast cancer for either compound.[45]
>
> In 1999 the National Research Council of the American Academy of Sciences, sponsored by the US EPA among others, examined the evidence for synthetic estrogens' effect on cancer risks.[46] Its summary conclusion on breast cancer sounded much like the British verdict: "An evaluation of the available studies conducted to date does not support an association between adult exposure to DDT, DDE, TCDD, and PCBs and cancer of the breast."[47]" (TSE:243-4)

Let us then look at KF's text. It is an incomplete statement when KF says "there are papers that find a relationship between these pesticides and breast cancer as well as others that do not find a relationship" because there is also studies that find the *reverse* connection (more DDT, less breast cancer.)

KF claims that there are other papers that indicate a connection between breast cancer and synthetic estrogens. This is true and I do point this out, but I also use the meta-research of the British advisory committee on carcinogenicity of chemicals and the National Research Council. Thus it seems entirely unreasonable to claim that I should also mention the individual paper's own attempts on meta-studies, when these are smaller, older and contain much fewer of the large scale, new studies. Moreover, from my text, it is clear that I am simply quoting the meta-studies of the British advisory committee and the National Research Council.

KF further claims that

> "concerning lindane, there is a clear conflict between reality and Lomborg´s text: "Of the three studies that have examined . . . lindane, none has found evidence for an association with increased risk of breast cancer". Actually, a Finnish investigation from 1990 showed a tenfold higher risk for breast cancer in persons who had elevated levels of a lindane residue." (p205)

Of course, I merely refer to the findings of the British advisory committee on carcinogenicity of chemicals:

> "There is very little epidemiological information available on lindane (γ-HCH) and its possible association with breast cancer. Of the three recent studies published after 1995 which considered lindane, none found evidence for an association with increased risk of breast cancer. The available evidence for environmental exposure to lindane suggests that body burdens of this chemical are very small, being undetectable in most individuals. It is therefore unlikely that further epidemiological investigations of breast cancer based on assessment of levels of lindane in adipose tissue, blood, or breast tissue would provide additional relevant information."[48]

Finally, KF claims that

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[41] COC 1999:5, a so-called Type I error. (This was the problem discussed in the file-drawer problem in chapter 1, p 000). The NRC (1999:257-8) makes the same observation.
[42] COC 1999:2; NRC 1999:258.
[43] NRC 1999:258.
[44] COC 1999:6.
[45] COC 1999:6.
[46] NRC 1999.
[47] NRC 1999:6.
[48] COC 1999:6, http://www.doh.gov.uk/pub/docs/doh/ocbreast.pdf.

> "most remarkably, Lomborg cites a Danish investigation as a reference that DDT is of no influence, but refuses to mention that the same investigation found a significant relationship between the pesticide dieldrin and breast cancer."

This claim is clearly wrong. I do state, "For dieldrin, only two studies had tested the connection, one finding no relationship, the other finding a positive association." KF actually admits this in his endnote: "Lomborg´s argument for omitting the information on dieldrin is that he considers the relationship found to be accidental. His argument…."[49] However, he then claims that *I* make an unreasonable claim that it is likely to be a chance finding.

But if one reads the note (here note 41) it is clear that this is not *my* argument but the argument of the British advisory committee on carcinogenicity of chemicals and the US National Research Council:

> "The Committee considered the result with dieldrin may have been a chance finding in view of the large number of statistical comparisons (46) undertaken in this study." [50]

and

> "Of the 46 compounds analyzed, however, only dieldrin showed a positive association with breast cancer risk: odds ratios of 1.96 and 2.05 in the top two quartiles of the distribution of values. Whether this is a biolgically significant or a chance occurrence, given the 46 different compounds analyzed and the multiple comparisons, is difficult to know."[51]

Thus, KF's frustration with such statistically reasonable argumentation would be more correctly directed towards the British advisory committee on carcinogenicity of chemicals and the US National Research Council.

Not confront my critics

Throughout the book it is claimed that I just don't reply to the critique leveled at me. JJ express this very clearly:

> "The present book [*Skeptical Questions*] was just one (among several) of the responses to the original Danish edition, which, apparently, Lomborg chose to ignore when reworking his book for the English -language edition." (p7)

But I have to wonder. *I actually replied to each an every claim in their original Danish book with a 185-page reply, available on the internet.* How can one possibly claim that I chose to ignore their book?

When I didn't use much of their criticism, it was because so little of it was not riddled with errors or based on blatant misreadings. (Actually, some minor but correct points were incorporated into the English version of *The Skeptical Environmentalist*.)

Moreover, I am aghast that they have chosen to get their text translated without even commenting on their many documented errors. With much more time and resources, I would like to have gone through their document much more carefully and expose many more of their claims. However, I hope that the small sample of inaccuracies, errors, blunders, misquotes and misrepresentations will indicate why I find *Skeptical Questions* to contain such questionable skepticism.

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[49] Notice, if KF claims that I cannot use the test as an indicator for negative connection to DDT but then leave it out for positive dieldrin, he makes an intuitive but wrong argument. Since 46 substances have been tested, the risk of a false positive is much higher, but the risk of a false negative is still well regulated.

[50] COC 1999:5.

[51] NRC 1999:257-8, http://books.nap.edu/books/0309064198/html/257.html#pagetop,