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Re: complaint filed by professor Stuart Pimm and professor Jeff Harvey March 15, 2002

I have been asked to comment on the complaint from professors Stuart Pimm and Jeff Harvey (P&H) March 15, 2002. It consists of copies of 9 papers and an "analysis of them" as an accompanying letter. I find it somewhat surprising that P&H have decided not to include or even take into account that I have responded in detail at my website to the two most prominent critiques of my book (P&Hs own *Nature* review, and the *Scientific American* reviews). Neglecting these replies also questions the purported motive with filing the complaint as merely exposing scientific dishonesty. As appendices, I provide a copy for the UVVU to judge themselves if there is much substance to these most prominent critiques.

The accompanying letter sums up what is probably a list of some of the 9 definitions of scientific dishonesty in the bekendgørelse 933, §3. Unfortunately, with the text's somewhat essayistic character it is often hard to see beyond the rhetoric and find where the actual claims of scientific dishonesty are logically established. This leaves me with the unreasonable task to reconstruct P&Hs arguments so that they constitute their best shot at indicating scientific dishonesty. However, taking into account that the plaintiffs have limited knowledge of the Danish system, I think it is reasonable to grant them some leeway with their arguments. On the other hand, it is not reasonable that I should reply to each and every claim made in this complaint. Some typical examples will be analyzed below. (Since many of the claims simply ignores that I have already answered these on my website, I will look primarily at the new claims.) Should the UVVU require me to respond to any more specific points I will be happy to comply.

1. Construction of data.

P&H claims that:
> There are many examples where Lomborg presents data or constructs them in such a way as to provide a pre-determined outcome. First of all, he switches back and forth between percentages and absolute numbers, depending on the trend which he wishes to highlight. This is clearly evident in the chapters on water and food production. For example, on p. 61, he says that the proportion of undernourished children in the developing world has fallen from 40 to 30 per cent over the past 15 years. However, data on the absolute numbers, which may have risen, are given only in the notes, as are data from a region where the percentage of starving have risen. Most readers will see only the main text with the relative numbers. Lomborg in response to this has always argued that it is proportions that matter. Responsible scholarship would note both the proportion and the numbers.

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Notice, how P&H actually point out that I've argued why I display relative numbers rather than absolute numbers (though they only say I've responded so, but actually in the book, I clearly stake out why in an entire box, p64).

Actually, their primary case (data from a region where the percentage of starving have risen) comes from their Nature article, here with my critique that was available to P&H:

> Lomborg's great optimism about humanity's future shows up in the way he presents statistics. In the hell-hole that is so much of sub-Saharan Africa, "starving people" constituted "38 percent in 1970 ... [but only] "33 percent ... in 1996. [The percentage is] expected to fall even further to 30 percent in 2010." The absolute numbers of starving are curiously missing from these paragraphs. Roughly, the region's population doubled between 1970 and 1996. To keep the numbers of starving constant, the percentage would have had to have dropped by more than half. The absolute numbers of malnourished in the region — as well as those whom fate will spare through their death from the myriad consequences of poverty (including AIDS) — are surely inconsistent with the first-listed "global trend" in a chapter entitled "Things are getting better".

I discuss in the book, whether morally, relative or absolute numbers are most important, and conclude that relative numbers are (p64):

Relative or absolute improvement?

When we look at a problem such as hunger or a shortage of pure drinking water, the question often arises as to whether we should use absolute or relative figures.

It is naturally a good thing for the number of people starving to have fallen both in absolute figures and as a percentage. Similarly, it would certainly be bad if both the number and the percentage had increased. But what if one figure increases and the other decreases?

My way of understanding this problem in moral terms involves setting up an ideal, moral choice situation. The idea is to imagine the problem from the point of view of an individual who must choose in which society he or she wants to live. The point is that the individual does not know his or her position in society (a sort of "veil of ignorance"). This ensures the universality of the moral evaluation.

For the sake of argument, let us say that there are only two types of people—those who die of starvation and those who survive. We can thus describe society A and society B:

(A)   A world in which 500,000 die of starvation out of a population of 1,000,000.
(B)   A world in which 750,000 die of starvation out of a population of 2,000,000.

In society B, the absolute figure has increased but the relative figure has fallen. To me the obvious choice in this situation is that society B is better than society A (although a society without death would naturally be preferable). My risk of dying (of hunger) in society B is 37.5 percent, against 50 percent in society A. My argument, then, is that the relative figure is the more important in a comparison, in which the absolute and relative figures point in opposing directions.

One can naturally criticize this choice on moral grounds, and argue that the society with the lowest absolute figure is the best (i.e. that A is better than B). But a view such as this meets a significant challenge in the form of yet another hypothetical society:

(C)   A world in which 499,999 people die of starvation out of a population of 500,000.

In this situation the absolute point of view has the substantial weakness in that it would also prefer society C to society A. Very few people are likely to see this as the right choice.

Therefore, when the absolute and the relative figures each points in its own direction, the relative figure will probably be the more morally relevant way to evaluate whether mankind's lot has improved or deteriorated.

Thus, I present the relative numbers, (especially in this context where Pimm & Harvey has taken the quote, where the issue is whether the *Global Environment Outlook 2000* is overstating their case, claiming that crop yields could be cut by half within 40 years – totally contrary to every other prediction).

Somehow suggesting that not presenting also the absolute numbers is suspect disregards this relative/absolute discussion. Moreover, I devote a whole section to discuss the Sub-Saharan plight (p. 65ff). Finally, even for Sub-Sahara life expectancy has increased till 1990, and 'only' remains stagnant till 2010, not actually declining (p. 52, fig. 16).

I find that P&H here have absolutely no argument for scientific dishonesty.

2. Selective and hidden discarding of unwanted results

P&H claim among other things that:

> «And tropical forests are not being lost at annual rates of 2-4%, as many environmentalists have claimed: the latest UN figures indicate a loss of less than 0.5%.»
>
> This is most certainly not what the satellite imagery shows. The details of how much forest are clear-cut and how much forest is damaged in addition are also readily available in the pages of *Science* and *Nature.*

It is entirely unclear how this would constitute an expression of discarding unwanted results – I'm merely pointing out that environmentalists clamed the former figures, the UN the latter (this is actually not a quote from the book, but from my Economist article). Apparently P&H are challenging the UN figures (as I refer from the latest forest study in 2001 at p113, endnote 801), but at least one could want somewhat better sources than just blanket references to Science and Nature. Certainly it would also seem appropriate to indicate why citing the UN figures would constitute selective and hidden discarding of unwanted results

P&H also claim:

> «Thus, the current professional understanding, backed by the UN, centres on an estimate of 0.7% lost species over the next 50 years. And this loss will not escalate but more likely abate within the next 100 years. True, the loss of 0.7% of biodiversity is a problem -one among many mankind still needs to solve -but it is nowhere near the catastrophe of losing 25-50% of all species, which is still so commonly claimed.»
>
> Quite what "backed by the UN" means is uncertain. What is certain is that these estimates are orders of magnitude different from the majority of those in the profession. "Professionals" publish in and read such journals as *Science, Nature,* and *PNAS.* Lomborg's selectivity is amazing, given how much literature there is on this subject. For example, those who compile the lists of species thought likely to become extinct in the near future «50 years), typically fmd that 10% or more are (11 % for birds, 12% for plants; etc.) These *Red Lists* (as they are called) are numerous, easy to find, very detailed, and updated every few years. Lomborg does not tell us why he chooses the one number that is less than a tenth as large as all the other sources.

First, P&H claim they don't understand what "backed by the UN" means, but this lack of understanding seems somewhat feigned as they are simply quoting a newspaper article (my 14/8/01 Guardian article). Yet, even there, I put out the web-version of the text, so that P&H could have

looked up what I meant by the phrase (http://image.guardian.co.uk/sys-files/Guardian/documents/2001/08/14/resources-biodiversity.pdf), where the endnote is placed *right* after the text: "backed by the UN 48," and endnote 48 reads:

48 "The rate of extinction today is hundreds, if not thousands, of times higher than the natural background rate" UNDP 1995:12. Using the invertebrate species lifespan of 11e6 years (May et al. 1995:3) and interpreting the quote to be from 200 to 2,000 times the natural background (the "if not thousands" means it could be thousands, not all the way up to 9,999): 200*(1/11e6)*50 = 0.09% and 2000*(1/11e6)*50 = 0.9%. The authors are not quite consistent, using a somewhat lower lifespan when comparing 1,000 times the background rate to 2 percent per 50 years (UNDP 1995:235), making the conclusion 0.4-4 percent over the next 50 years.

It appears to me to be hard to argue that this has not (even for a newspaper article) been made fairly simple.

After this, P&H claim that the 0.7% estimate is an order of magnitude greater, but this is comparing the number to other number that are less well correlated to actual extinction, as I already commented in my remarks to their Nature review:

> Exactly repeating Simon, Lomborg juxtaposes the small number of named dead species against the huge number of species for which we have no knowledge at all. After pages of confused argument, his extinction estimate of "0.7 percent over the next 50 years" is strikingly discordant with the 10– 40% of well-known species that teeter on the brink of extinction just from human actions to date. About 2% of well-known species are already so desperately rare that we don't know whether they do survive. Lomborg finds comfort when some are rediscovered. Like terminally ailing humans, their lingering survival does not allay fears about the unfolding epidemic.

Pimm & Harvey get unclear here, since they resort to 'a hypothetical critic' but presumably it is meant to be read as if it was me. But I do not juxtapose the small known number of extinct species to the large number of unknown species – both in the text and in the table, I point out that the known number of extinctions is a serious underestimate: "Note that because of the severe regulations for documenting extinctions these figures certainly underestimate their true number" (p. 250, cf. p. 252).

Pimm & Harvey then claim that the 0.7%/50yrs is strikingly discordant with the 10-40% threatened species, but these are two entirely different measures. Actually, the book documents an analysis of the 1000 birds claimed to become extinct, and finds that primarily because of conservation efforts – "relatively few of these species are likely to become extinct by 2015" (p255). Thus, it is likely that the 10-40% is a *vast* overestimate of the actual number of species that will go extinct. Pimm & Harvey could attempt to argue that the category 'threatened' is a better measure of biodiversity than 'extinct' (though methodologically probably much harder to keep constant over time) – but merely contrasting the two numbers to imply that I am wrong in my biodiversity loss estimate is plainly an incorrect argument.

I find it unsubstantiated for P&H to claim that the above examples should constitute selective and hidden discarding of unwanted results.

6. Plagiarizing
Throughout much of the former text P&H also claim that these examples also show that I am guilty of plagiarizing, as:

> Lomborg admits at the bottom of footnote No. 2011 (where it is hard to find!) that "This chapter is to a large degree based on Simon and Wildavsky 1995" (fitting your criteria under item 6).

Apart from the somewhat bizarre implication that placing a blanket reference in an endnote is done to make it hard, such accusation seems entirely unfounded, as I have included many other references besides Simon and Wildavsky (mainly the UN Global Biodiversity Assessment) and that I exactly *do* state that it is based on Simon and Wildavsky. Moreover, it appears unreasonable to imply that I should be 'admitting' basing the chapter on other work, as this exactly appears in the endnote.

1&4 Construction of data and consciously deceptive use of statistical methods

P&H maintains that the following is a "clear example of distorting facts by constructing data to produce a specific result":

> Lomborg uses FAO data sets to calculate global forest cover since 1950. He stitches together data using different methodologies, and the FAO even says their surveys were discontinued in 1994 because they were "unreliable" and not meant to be used for this purpose anyway. More accurate UN Forest resources Assessment data suggests that 4.2% of global forest cover was lost in the 1990's alone, and this total is based only on forests permanently converted (and not land either in various stages of regeneration or designated for replanting). It excludes (among other things) forests that were burned, selectively logged, fragmented, etc. This is a clear example of distorting facts by constructing data to produce a specific result.

However, I clearly in the forest discussion (which UVVU has already seen in the debate with Fog) discuss *all* the data sets, as well as present them in my figure (Figure 60). I also argue why I find that the longest data series would be the best long-term indicator, as can be seen in the quote below. Moreover, it is unclear why some of the foremost scholars on forests (such as Richards and Williams) have presented the same data series from FAO since 1950 and apparently are not guilty of using deceptive statistics and constructing data.

> "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 percent 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 limited definition of forest area for the period 1980-90 and 1990-5. 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 upwards revision of the 1990 forest area was larger than the entire global decline in 1990-5 (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 definitions of forest once again and made a new estimate of forested area from 1990-2000, showing a small decline from 29.5 to 28.8 percent." (p111)

Again, I find no evidence to back P&Hs claims of construction of data and consciously deceptive use of statistical methods.

The next claim of P&H is:

> Moreover, Lomborg (Gleick, UCS, p. 5) takes data with considerable degrees of statistical uncertainty -such as forest cover -and uses this data to make his points. Some of the data are incredible, as when he argues that "global forest cover has increased from 30.04 to 30.89 percent between 1950 and 1994' (TSE, p. 111). Since our ability to measure forest cover is well below the prediction [should probably read precision, BL] of these data, Lomborg's claim that "forest cover has increased 0.85% [I do of course correctly write percentage points, as is apparent above, but the meaning is clear, BL]" is a deliberate obfuscation, and what is more startling is that it is made by a purported statistician. Again, clear evidence of bias to make a specific point.

This claim neglects that I state right after (see also above) that "the area covered by forest had shrunk from 27.25 percent to 25.8 percent, or by 1.35 percentage points." If P&Hs claim should hold up, I'm here demonstrating the *opposite* bias, but this they fail to note. Of course, this makes the entire claim vacuous.

The last example shows, according to P&H:

> Gleick (UCS, p. 4) reveals that Lomborg combines data sets on access to drinking water and sanitation that he admits were collected using different defmitions, different time periods, and different combinations of countries, then attempts to draw a logistic "best fit" to the data. These data are incompatible.

Of course the actual graph is entirely honest about comparing these different estimates:

> Figure 5 Percentage of people in the Third World with access to drinking water and sanitation, 1970-2000. Light, broken lines indicate individual, comparable estimates, solid lines is a logistic best fit line – a reasonable attempt to map out the best guess of development among very different definitions. (p22).

There is absolutely no need for Gleick to "reveal" this, because I note it clearly in the caption. Thus, again there is no evidence for construction of data and consciously deceptive use of statistical methods.

7. Deliberately distorted representation of others' results.
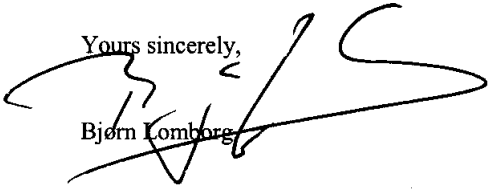The chapter does not actually give any examples, only state broadly that:

> The work he criticizes is that of Nobel prize winners, and those who have won international prizes deliberately designed to complement Nobels in field where they are not awarded.

Of course, besides lacking any argumentative power, this is exactly the opposite of science. The person with the most awards or most prestigious awards is not necessarily the one with the correct argument, and to even imply otherwise seems antithetical of science.

With the above selection of claims that utterly fail to uphold the claim of scientific dishonesty I move for the UVVU to dismiss the case.

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Yours sincerely,

Bjørn Lomborg