On the political economy of environmental survival versus collapse. Clarifying the work done by Tinbergen Hueting vis-à-vis Weitzman, Nordhaus and Stern

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Abstract

The Stern Review (2006) on the economics of climate change presented a cost estimate of perhaps even 20% of national income and subsequently was criticized by Weitzman and Nordhaus and others in a discussion that centered on the use of the calculus of variations and the choice of the proper rate of discount. The Tinbergen & Hueting (1991) approach deals with the wider environmental collapse, is not formulated in the form of the calculus of variations, and arrives at a sustainable level of national income of about 50% of national income. The Tinbergen & Hueting (TH) approach appears to be neglected by Weitzman, Nordhaus and Stern (WNS) but appears to be better grounded in economic theory, mathematically richer and empirically more relevant. This paper clarifies the misunderstandings and omissions in the work by WNS on environmental economics.

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Introduction

Jan Tinbergen's 1929 Ph. D. thesis – his own copy apparently for sale for EUR 3300 at antiqbook – contains a decent amount of analysis in the calculus of variations, see Boumans (1992). This mathematical approach is also used by Martin Weitzman, William Nordhaus and Nicholas Stern (WNS) to discuss sustainable development and the economics of climate change.

Tinbergen (1903 – 1994) was also involved with the more practical problems of data gathering, national accounting, model formulation and number crunching. His attention in 1969 and 1990 was drawn to publications by Hueting, then head of the dept. of environmental statistics at CBS Statistics Netherlands, and this resulted in the Tinbergen & Hueting (1991) (TH) paper *GNP and Market Prices: Wrong Signals for Sustainable Economic Success that Mask Environmental Destruction*. Their approach appears to be very important, but it is not formulated in the language of the calculus of variations (though some parts are). Remarkably, Tinbergen (1985) does not refer to Hueting's work but the explanation must be that he takes this work so for granted that it does not occur to him that a reference might be useful. Tinbergen (1985:118) discusses 'counterproduction' (sometimes also called 'double counting' but nowadays called 'asymmetric entries' by Hueting). An example would be a catalyst for the exhausts from a car: the value added in its production should not be included in national income since it only restores the clean air that existed before.

Young econometricians currently trained in environmental economics tend to focus on the mathematically elegant approach of the calculus of variations while they have come to neglect the Tinbergen & Hueting approach, and, in path-dependency, they continue to neglect it. Even the Stern Review with its ethical approach to the calculus of variations neglects the TH approach on sustainable national income. Major critiques on the Stern Review were on the rate of discount and the ethics within the framework of the calculus of variations, but none of the widely cited economists referred to the TH approach, see Nordhaus (2007a) and Weitzman (2007ab) themselves but also e.g. Dasgupta (2007a) and Tol (2006), and also Quiggin (2006) on this discussion itself. If this neglect of the Tinbergen & Hueting approach continues, a major resource and strand of economic thought is left unused.

This present paper wishes to clarify the situation. The best approach is to take the angle from TH and comment on WNS. This ought to help readers of WNS – if not WNS themselves – to better understand the value of TH. This present paper can be seen as a companion to Colignatus (2008) that reviewed the earlier history of the TH approach. The TH figure for environmentally sustainable national income (eSNI) is about 50% of national income (NI), while the Stern Review arrives at costs of at most 20% of NI. This sizeable difference caused me to look deeper into the Stern Review and its critics.

Economists are a bit reluctant, as I myself, to think in terms of survival and collapse. Dupont (2008:47), writing in Volume 50 of *Survival*, a journal of the International Institute for Strategic Studies in London: "In the security domain, strategic doctrines and defence budgets are frequently justified on the basis of far less observable evidence than we have about the climate future which awaits us." He mentions various ecological risks in the same way as will be done below, clarifying that the terms of survival and collapse are proper, and that this indeed is the framework of discussion.

The G8 in Japan July 2008 stated that emissions of greenhouse gases (GHG) should be reduced by at least 50% in 2050, though not stating explicitly from what base year, although the Chair later said to intend 2008. In that respect, one of the major steps towards recognition seems to have been taken. However, taking only the index of GHG or even temperature seems insufficient to guide policy and it seems best to have an indicator for environmentally sustainable income (eSNI) alongside national income (NI). The following discussion thus is not only important for understanding the issue of survival versus collapse but also for the selection of the proper policy indicators.

The two approaches

The Weitzman, Nordhaus and Stern (WNS) discussion has an interesting structure. In particular: (1) Weitzman (1976) determines the stationary equivalent of future consumption, which can be interpreted as sustainable income for the market sector only. Then Nordhaus (1995), referring to Karl-Gustaf Löfgren 1992, extends the calculus with non-market resources. Shadow prices follow from a well-defined production function. (2) While Nordhaus and Weitzman see no cause for urgent action, the Stern Review (2006) advises to more active policies, emphasizing the risks of climate change,

i.e. the catastrophies or events with low probability but high negative impact. The Stern Review uses a low rate of discount for the actual calculations, and subsequently Nordhaus (2007a) and Weitzman (2007a) criticize that low rate. (3) Weitzman (2007ab) concludes that the 'traditional approach' in the calculus of variations – as used by the Stern Review but in fact also developed by Weitzman himself – neglects uncertainty and risk with respect to catastrophies. The certainty calculus in the Stern Review would not fit the texts on the risks. Weitzman then actually reformulates the calculus so that we now have a variant that can deal with some uncertainty. Then the road to more active policies is open again. It appears that the Stern Review uses a 'certainty equivalent' or an 'ethical reduced form' of a mathematically proper 'uncertainty calculus of variations'. Order and decency in economic advice are restored.

The mentioned mathematical structure makes philosophical sense. Ethics has everything to do with survival. Ethical issues relate to the functioning of the group with respect to survival of the group and the species. Survival not only relates to the everyday economic chores for food and shelter, where there is always the distinction between basic needs and luxuries, but survival comes clearly to the fore in all urgency under catastrophies such as fires, floods, bad harvests and so on. Where the Stern Review apparently lacked the mathematical sophistication that Weitzman so handsomely provided the Review still made sense where it formulated the issue with the tools at hand.

The TH approach formulates standards for non-renewable resources and eight environmental functions (space, water, soil, concentrations of nutrients, radiation, temperature, toxids, localities), and imposes those standards on the model.

> Therefore, the following procedure is proposed for correcting GNP for environmental losses (Hueting 1986, 1989). First define physical standards for environmental functions, based on their sustainable use. These standards replace the (unknown) demand curves. Then formulate measures to meet these standards. Finally, estimate the money involved in implementing the measures. The reduction of national income (Y) by the amounts found gives a first approximation of the activity level which, in line with the standards applied, is sustainable. Needless to say a correction for double counting, mentioned above, must also be made. If the sustainable level is Y', the difference between Y and Y' indicates, in money terms, how far society has drifted away from its desired goal of sustainable use of the environment.
>
> Tinbergen & Hueting (1991)

Thus, in this piece of economic advice, on one hand there is the mathematically elegant approach of the calculus of variations and on the other hand there is the more practical and statistical approach. The two schools (with Tinbergen at bottom in both) have not yet come together, causing different policy advices, and this already lasts a number of years. Between Tinbergen & Hueting in 1991 and the Stern Review in 2006, both advising strong action, there are already 15 years. In this day and age those 15 years mean a population growth of 1 billion people. Where Tinbergen & Hueting in 1991 were worried already by the past change of the world population from 4 to 5 billion, we now are in the worries about the current change from 6 to 7 billion. Quick effective action, e.g. possibly by turning development aid into family and pension planning policies, can mean a lot for environmental sustainability. This earlier window of opportunity has now been lost, perhaps because of mathematical formulation or perhaps because of political will. With the new sophistication by Weitzman we can observe that the mathematically elegant approach confirms the precautionary but perhaps less elegant approach by Tinbergen & Hueting. Hopefully, the two schools (with Tinbergen at bottom in both) can come together and there can arise some consensus in policy advice now, and develop the particulars of that advice.

In fact, with the new Weitzman reformulation of the precautionary principle, the Tinbergen & Hueting approach stands rather vindicated and it would at least be curious

why advice with a proven track record of wisdom is neglected, not looked into, not referred to and forgotten.

Interestingly, Tinbergen was a mentor for Tjalling Koopmans (1910 – 1985), see the obituary by Scarf (not dated), and Koopmans was a mentor for Weitzman, see Weitzman (2001) dedicating that paper to him, while also Nordhaus has been affiliated consistently with the Cowles Foundation. It may be hoped that the Tinbergen and Koopmans way of doing economics finds new inspiration for their younger generations. The current neglect of the TH analysis is not fitting to this figure in the history of economics.

A main point to observe is that the models in the calculus of variations considered by WNS are very stylized constructs that omit the prisoners' dilemma and negotiation costs of non-market resources. Precisely the latter are the very core of the environmental problem. The problem of co-ordination within a nation and between or across nations are the crucial issues here. On this count alone, economists would already focus on the TH approach. The following comments thus are rather on the fringe, caused by the particular properties of the WNS approach, but nevertheless still interesting and relevant for graduate students in the calculus of variations and for readers desiring to understand the political economy of environmental survival versus collapse.

Rightly scaring people

My own way of scaring people in Holland is, see Colignatus (2007) (in Dutch), by pointing to the fact, not the risk, that in a "business as usual" (BAU) scenario the Greenland ice will melt, causing severe flooding of Holland. Raising dikes will be extremely costly, since sandy undergrounds require foundations, and more water filtering in from under the dikes anyway because of the increased pressure. The drowning of Amsterdam need not happen this current century but in BAU it does at some time. Check:

> "Some temperature triggers, like 3 or 4º of warming, could be reached this century if warming occurs quite rapidly. (…) This would commit the world to increases in sea level of around 5 to 12-m over coming centuries to millennia (…)" Stern (2007)

Weitzman (2007a) is informative of the risk that it actually happens this very century:

> "Translated into the language of the simple model used here, such rare disasters are far out in the right tail of very high ΔT, which corresponds to being far out in the left tail of the consumption-growth random variable g. The probability distribution of long-run ΔT is disturbingly spread apart, largely because of structural-parameter uncertainty about the unknown "climate sensitivity" multiplier that amplifies GHG concentrations into ultimate steady-state greenhouse warming. The recently-released *Fourth Assessment Report of the IPCC* (2007) predicts for one hundred years from now a mean temperature change of further planetary warming (from averaging six "equally sound" marker scenarios) of $E[\Delta T] \approx 2.8°C$ with a thick-tailed upper-end standard deviation $\approx 1.6°C$ (Table SPM-3). This means the probability that $\Delta T > 4.5°C$ is approximately 15% and the probability of $\Delta T > 6°C$ is very roughly about 3%. IPCC does not extend its projections beyond 2105 on the basis that predictions into the 22nd century are too uncertain, but it seems unavoidable that the reduced-form probability of $\Delta T > 6°C$ increases substantially above 3% after the next century just from the enormous inertial lags for what by then will be in the climate-change pipeline. Societies and ecosystems whose average temperature has changed in the course of a century or so by $\Delta T > 6°C$ (for U.S. readers: $\Delta 6°C \approx \Delta 11°F$) are located in the *terra incognita* of what any honest economic modeler would have to admit is a planet Earth reconfigured as science fiction, since such high temperatures have not existed for some tens of millions of years." Weitzman (2007a)

When discussing other scare factors, Weitzman (2007a) reads like literature:

> "There is little doubt that the worst-case scenarios of global-warming catastrophes are genuinely frightening. The *Stern Review* goes over several of these highly-unlikely poorly-understood threshold-crossing disasters associated with abrupt large-scale irreversible changes in the climate system: sudden collapse of the Greenland and West Antarctica ice sheets, weakening or even reversal of thermohaline circulations that might radically affect such things as the Gulf Stream and European climate, runaway climate-sensitivity amplification of global warming due to positive-reinforcing multiplier feedbacks (including, but not limited to, loss of polar albedo, weakened carbon sinks, and rapid releases of methane from the thawing of arctic permafrost). More gradual but still very serious examples of uncertain climate-change effects are: sea-level dynamics, drowned coastlines of unknown magnitude, very different and possibly extreme weather patterns including droughts and floods, ecosystem destruction, mass species extinctions, big changes in worldwide precipitation patterns and distribution of fresh water, tropical-crop failures, large-scale migrations of human populations, humidity-nourished contagious diseases, and the list goes on and on." Weitzman (2007a)

Dasgupta (2007b) explains that economists – well, not TH, but their exception is not mentioned – have been deaf to arguments by ecologists:

> "Proposition 4 reveals the limitations of overly formal analyses of the economics of climate change. (We should add to that the economics of biodiversity loss.) I personally believe that Humanity should invest sufficiently so as to keep global mean temperature from rising beyond another 2-3 degrees Celsius, even though I realise that the expenditure that will be required to constrain carbon emissions will be a lot bigger than the mere 2% of the GDP of rich countries proposed by Stern (2006) if advancements in global sequestration technologies and technologies using alternative sources of energy are harder to realise than is currently hoped. But I am unable to justify that belief from any formal model. Ultimately, it is a "gut feeling" about the awful things that could

occur if the global mean temperature were to rise another 5 degrees that should make us very scared.

Climate change has been taken seriously by all economists who have studied the science since the late 1970s. Even the now-famous "hockey-stick", displayed by time series of carbon concentration in the atmosphere, appeared some time ago (Bolin, 1989: fig. 5). Moreover, the Second Assessment Report (1996) of the Intergovernmental Panel on Climate Change should have made us acknowledge climate change to be one of the most significant environmental issues facing Humanity. To be critical of the "economics of climate change" is not to understate the harm Humanity is inflicting on itself by degrading the natural environment - not only in regard to the stock of carbon in the atmosphere, but also in regard to so many other environmental matters besides. But the cause is not served by misplaced concreteness, especially not when parameter values are so chosen that they yield currently desired answers.

For many years ecologists - more generally, environmental scientists - have asked economists to consider the "precautionary principle" seriously. We did not do so. I believe what they meant by the term was that we should not play down the possibility of environmental catastrophies - owing to climate change, species extinctions caused by habitat destruction, and so forth. The writings of Paul Ehrlich, James Hansen, John Holdren, Peter Raven, and E.O. Wilson have been critical here. What environmental scientists meant was that the uncertainties associated with the economic effects of environmental degradation are very great. But, as the uncertainties were meant to cover 200 years and more, no attempt was made to estimate those uncertainties. Our colleagues in the environmental sciences were correct not to have done so. Proposition 4 shows us the dangers of misplaced concreteness. (…)

Economics helps us to realise what we are able to say about matters that will reveal themselves only in the distant future. Simultaneously, it helps us to realise the limits of what we are able to say. And that too is worth knowing, for limits on what we are able to say are not a reason for inaction. Climate change and biodiversity losses are two phenomena that are probably not amenable to formal, quantitative economic analysis. We economists should have not pressed for what I believe is misplaced concreteness. Certainly, we should not do so now." Dasgupta (2007b)

Note that Dasgupta's claim "Climate change and biodiversity losses are two phenomena that are probably not amenable to formal, quantitative economic analysis" is in conflict with the Tinbergen & Hueting (1991) approach, recently updated by Hueting & De Boer (2001) and Hueting (2008). But Dasgupta does simply not refer to that line of research.

Where Dasgupta states "But the cause is not served by misplaced concreteness, especially not when parameter values are so chosen that they yield currently desired answers" there (a) the "desired answers" are derived from an analysis of risk, which is the actual economic analysis, both proper and sound, while the mathematical model is only a tool to enhance consistency, (b) he apparently does not see that the Stern Review choice of parameters reflects certainty equivalence, where, as said, a 'certainty calculus of variations' has to do the work of an (at that time not yet available) 'uncertainty calculus of variations', (c) the odium of "misplaced concreteness" falls on the critics of the Stern Review who do not see (a) and (b).

Possibly entertaining people

Following the 'stick and carrot' philosophy, and having mentioned the scare above, it seems proper to allow for some entertainment too. Weitzman's remark on 'science fiction' is tempting as well. In order to maintain the serious character of this paper, these more entertaining remarks are put in **Appendix A**.

Definitions of uncertainty and risk

Weitzman (2007a) explains his notions of uncertainty and risk: "The cost of low-g disasters from high-ΔT scenarios more properly constitutes uncertainty in the sense of Knight or Keynes than risk, because the scale and probability of these disasters are both unknown."

Earlier, Colignatus (1999, 2001) explained that this use of terms by Knight (or Keynes) is contrary to standard English:

> "The commonly adopted definitions of risk and uncertainty generate conceptual problems and inconsistencies, and they are a source of confusion in general. However, alternative and proper definitions are: (1) First there is the distinction between certainty and uncertainty. (2) Uncertainty forks into known (assumed) and unknown probabilities. (3) Unknown probabilities forks into known categories and unknown categories. (4) Known categories forks into 'including the uncertainties in the probabilities by explicitly assuming a uniform distribution' (Laplace) or neglect (or use other non-probabilistic techniques). Note that the term 'risk' has not been used in the 4 points above, so that an independent definition is possible. 'Risk' can be defined as the absolute value of probable loss, i.e. as (rho) $\rho = -E[X; X < 0]$. Also, relative risk is the probable loss with respect to a target t, giving $\rho(t) = t - E[X; X < t]$. The definitions provided here are directly in line with the Oxford English dictionary. It turns out that textbooks generally can keep their mathematics but will best rewrite their texts to these definitions. Not only the students and the general public will benefit from this sudden clarity, but eventually also statistics and economic theory themselves." Summary of Colignatus (1999, 2001)

We can be uncertain about parameter values, but that is not uncertainty *per se*. If the word "uncertain" causes conceptual difficulties here, say "unknown parameter values".

A fat tail is not needed to get scared

Weitzman (2007a) elaborates that the uncertainty in the probabilities causes a reduced form with a fat tail, (exactly) like a normal distribution with unknown dispersion causes a Student-*t* distribution.

However, it is not true that a fat tail is required to get scared about catastrophes. It suffices to conclude that the probability is not zero, and then the extremity suffices for the impact. You may have to include above risk measure $\rho = -\mathrm{E}[X; X < 0]$ as a separate entry in the utility function to become aware of this, though. This is precisely what Chapter 8, "Measuring Utility" by Colignatus (2001, 2007) does.

This discussion somewhat suffers from what Dasgupta rightly calls the fallacy of misplaced concreteness, though perhaps in a slightly different way. The point is that mostly everything is uncertain and that there may exist little certainty anyway. For example, I feel pretty certain that tomorrow the beach at Scheveningen will still be there, but, of course, neither I nor the beach may be there anymore. Where the Stern Review uses the 'certainty calculus of variations' it is mathematically proper to criticize it for not using the 'uncertainty' version, and it is mathematically impressive to create such a version, but it misrepresents the original idea that the whole exercise was intended to deal with the uncertainties of the future. It basically misunderstands that it is standard procedure in economics to use the 'certain' tools at hand, even while everyone knowns that subject matters in economics are generally uncertain.

Note that pure certainty is caught in the "Definition & Reality methodology", that uses definitions to say something about the uncertain future – see Colignatus (2005). That piece of analysis is in fact presented as a somewhat new approach, given that normal analyses deal with uncertainties.

In the same vein it would be incorrect to criticize TH for not even using the calculus of variations or not inventing the right kind of calculus. It would be a valid mathematical observation but it would not be relevant for the economic analysis that is under concern. Instead of getting lost into this kind of critique, economists would do better in studying

TH and improve on the economic analysis, using adequate mathematical and statistical techniques.

With respect to the Stern Review, one of the conclusions by Weitzman is:

> "However, in my opinion Stern deserves a measure of discredit for giving readers an authoritative-looking impression that seemingly-objective best-available-practice professional economic analysis robustly supports its conclusions, instead of more-openly disclosing the full extent to which the Review's radical policy recommendations depend upon controversial extreme assumptions and unconventional discount rates that most mainstream economists would consider much too low." Weitzman (2007a)

This is a valid mathematical criticism but not correct for a professor of economics. The economic analysis is primarily in the evaluation of the risks while the mathematical implementation is only a way to enhance consistency and clarity. Perhaps the hot potato is passed on to "most mainstream economists" who have not read TH and who have been neglecting the ecological warnings for years. But being in a majority only carries the weight of a majority and we should be more interested in some Elo-rating as in chess. Somehow, the profession has not yet found a way to define a tournament but that in itself would be a strange kind of excuse in a discussion like this.

Facts, of the past and of reality

Above, I used the phrase "the fact, not the risk, that in a "business as usual" (BAU) scenario the Greenland ice will melt". Some readers may object to this use of language, since in their opinion facts occur only in the past, not in the uncertain future. Even a BAU scenario might contain an unforeseen discovery of cheap energy while a sustainable scenario might contain an unforeseen collapse.

Admittedly, I like to keep my use of language as strict as mathematics itself and thus the following comments can be clarifying. The phrase "the fact, not the risk, that in a "business as usual" (BAU) scenario the Greenland ice will melt" is an exact verbal translation of A = "Greenland ice will melt" and $\pi = P(A \mid \text{BAU}) = 1$, where the BAU scenario is defined as a certainty equivalent. The statement and context are rather not an issue of risk but rather an issue of conditionality.

The colloquial term "fact" tends to relate to the past but allows also for "reality" in which there is also a future. The dictionary by Hornby (1985) gives:

> "**1** [C] sth that has happened or been done (…) **2** [C] sth known to be true or accepted as true (…) **3** [U] reality; what is true; what exists (…)" Hornby (1985)

When the discussion context is the future, then people generally understand that the word "fact" is not used in the sense of referring to the past. It is reasonable to expect that people understand the word as an expression of truth and reality.

Let us consider a patient asking a medical doctor whether he will die. A generally acceptable answer is: "Eventually you will die for sure, but, *when*, my prognosis is (…)". It would be generally considered a bit of humour or sarcasm, depending upon the patient-doctor relationship, when the answer would be: "You will not die when they invent an immortality drug and you keep out of the way of fatal accidents."

Let A = "Greenland ice will melt"
BAU = a "business as usual" scenario, defined as certainty equivalence
SUS = a "sustainability" scenario, defined as certainty equivalence
u background risk from cases and probabilities not considered
other variables defined as in Table 1

Table 1. Clarification of certainty equivalence

Certainty equivalence, BAU and SUS	*Uncertainty,* BAU* and SUS*
$\pi = P(A \mid \text{BAU}) = 1$	$0 < \pi^* = P(A \mid \text{BAU}^*) < 1$
$\varsigma = P(A \mid \text{SUS}) = 0$	$0 < \varsigma^* = P(A \mid \text{SUS}^*) < 1$
$p = P(\text{BAU}) = p^*$ (alt. $p = p^* \pi^*$)	$p^* = P(\text{BAU}^*)$
$q = P(\text{SUS}) = q^*$ (alt. $q = q^* \varsigma^*$)	$q^* = P(\text{SUS}^*)$
$u = u^* + q^* \varsigma^* - p^*(1 - \pi^*)$ (alt. otherwise)	u^*
$P(A) = p\,\pi + q\,\varsigma + u = p + u$	$P(A) = p^* \pi^* + q^* \varsigma^* + u^*$

In terms of uncertainty, we would consider $p\ \pi$, which under certainty equivalence reduces to p. Some may hold that such certainty equivalence is not possible, since the future is always uncertain. In the present state of environmental developments they are rather like a M.D. who seriously considers the chance of an immortality drug. In that case they presume a BAU* scenario as the true scenario, with $\pi^* \neq 1$ and some p^*. However, we may define the BAU case with $p = p^* \pi^*$, and hence there actually is a certainty equivalence. It is clearer, though, to also include the SUS scenario and choose

equal probabilities $p = p^*$ and $q = q^*$, which has been taken as the default case in the table. Note that there is a social welfare function (SWF) in each scenario so that the choice or the regime switch can be represented by a Meta-SWF(BAU, SUS), with the probabilities possibly seen as weights, see Colignatus (2000).

Scenario analysis and cost-benefit analysis

In this respect, there also appears to exist a crucial issue when we consider scenario analysis and cost-benefit analysis. Note that with a Meta-SWF(BAU, SUS), as just mentioned, each path has its own utility function and parameters, and that the switch is an overall-preference. Colignatus (1992, 1995) used the notation SWF(x, I) where $I \in \{0, 1\}$ represents information or the regime. This approach helped me to understand the TH approach that I later encountered. When we observe the economy of a democratic nation, it is tempting, with Samuelson, to see the results as revealed preferences. For TH, these results may however also be revealed blockages. Prisoners' dilemma and negotiation costs may hinder the expression of the true preferences. In fact, there is uncertainty as to what the real preferences are. By consequence, it will not do to use a single utility function, to assume that the economy is in the Pangloss optimal state and then perform a cost-benefit analysis to another Pangloss optimal state – and this will not work since the original state is already optimal. Instead, we have to use different utility parameters for the different paths, allow each path to be optimal, and let the costs and benefits from switching be conditional, for example, if BAU is optimal with respect to SWF_{BAU} then it is suboptimal with respect to SWF_{SUS}, and if the latter would be the true SWF, then we can calculate the net advance from the costs and benefits of a switch from BAU to SUS. See Colignatus (2000) for an example.

Note that the widely cited Bovenberg & De Mooij (1994) and the De Mooij (1999) thesis on the 'double dividend' use only one SWF and thus are a bit less relevant for the proper policy question. Note too that when such analyses were to be performed with the calculus of variations, that there would also be another confusion to avoid, when translating the results to the real world (or a realistic model of the real world). The simplest models use uniform taxation so that the marginal rate is also the average rate. It would be standard economics, and fitting to the framework of optimization, to draw conclusions on the marginal tax rate. However, for reality, we should keep in mind that

tax schemes have exemptions and are indexed on inflation or the level of subsistence. Therefor, the translation should not be to the statutory marginal tax rate but to the proper 'dynamic marginal tax rate', that actually would be closer to the average rate, see Colignatus (1992:272) or Colignatus (2005:140-145).

The Sterner & Persson approach

Sterner & Persson (2007) criticize the single sector (corn model) approach in the traditional form of the calculus of variations:

> "While we find no strong objections to the discounting assumptions adopted in the Stern Review, our main point is that the conclusions reached in the review can be justified on other grounds than by using a low discount rate. We argue that nonmarket damages from climate change are probably underestimated and that future scarcities that will be induced by the changing composition of the economy and climate change should lead to rising relative prices for certain goods and services, raising the estimated damage of climate change and counteracting the effect of discounting." From the Summary of Sterner & Persson (2007)

This argument thus has the same structure as the certainty equivalent to an uncertainty calculus of variations model. In this case the true model is disaggregated but it can be aggregated into an 'ethical reduced form'. Of necessity, these authors state: "If we were to have both low discount rates and changing relative prices, we would find even stronger support for firm and immediate abatement measures."

Of the various papers mentioned here, the Sterner & Persson paper comes closest to the TH approach, and thus might be a bridge towards understanding.

A small note on calculating the damage

Weitzman (2007a) recalls the way how the damage due to climate change is calculated in this kind of study:

$$D(t) = Y^*(t) - Y(t) = f(\Delta T(t))\ Y^*(t)$$

"where t is time, D is the total damages of greenhouse warming, ΔT is atmospheric temperature relative to the base period, Y^* is potential GDP (or NDP, no distinction being made here) in the absence of any greenhouse warming, and Y is actual GDP with

greenhouse warming." If I understand this correctly (but this may also be a formulation that is confusing to me) this approach assumes that there is some autonomous growth in Y^*, say $g = 2\%$, that is unaffected by environmental degradation. Thus:

$$D(t) = Y^*(0)\,(1 + g)^{\,t} - Y(t)$$

However, it is a bit strange to assume that 'no global warming' can come about without additional costs. Only when we are prepared to make costs then we may reduce global warming. Potential growth has to take place in an environment where growth becomes increasingly difficult due to environmental change. A Holland that builds dikes has less time to paint sunflowers.

Rather, one would prefer the Hueting & De Boer (2001:46) approach: "We work towards these goals by discussing a series of cases of increasing relevance to our problem: (1) preferences for environmental functions are unimportant because functions are abundant; (2) functions are scarce and preferences are such that the optimal path (computed by the model) approximates the actual path; (3) preferences for the environment are stronger than in the second case, but there are blockages preventing their full expression; (4) preferences are as strong as in the third case, but the blockages have been overcome; and (5) the special form of the last case in which preferences for sustainability are general and dominant."

Apparently, $D(t) = Y^*(t) - Y(t)$ gives the difference between case 1 and 2, and can be denoted as $D_{1,2}(t) = Y_1(t) - Y_2(t)$. As said, this is only very hypothetical since path (1) is pure phantasy. Relevant are the costs of sustainability $D_{2,5}(t) = Y_2(t) - Y_5(t)$, where (5) creates the case that has temperature under some control.

Admittedly, there is a sense in which people regard 'the cost of temperature rise', as a conditional. Note that $Y(t) = Y(t \mid T(t))$. Then $D(t \mid s) = Y(t \mid T(s)) - Y(t) = f(T(t) \mid s)$, for e.g. $s = 0$ or $s = t - 1$ or $s =$ a value that gives sustainability. Thus when people see damages depend upon the temperature then this is rather a counterfactual than a potential. Controlled temperature might only be a potential if we were to invest in prevention, but it is not a given, something that can be seen as falling from the sky like manna. With $T(t) = T(t\text{-}1) + \Delta T(Y(t))$ we also note that production causes CO_2 exhausts

and then a rise in temperature (likely with more lags), so that $Y(t \mid T(s))$ is not realistic for $s \neq t$.

It might be that the two approaches – either assuming some constant growth in "potential output" or the latter approach based upon the real production function – would be equivalent in practice with respect to the calculation of "costs". But the latter approach remains more tractable and true to fact.

Rate of discount

(a) Note that market rates of discount do not substract for the non-market loss of environmental functions. For example, we would frequently like to see $r \approx g$, and the correction of g for environmental deterioration would similarly apply to r.

(b) In that sense, it may very well be that Nordhaus (2007a) who emphasizes the use of the market rate of discount too, has been less sensitive to Nordhaus (1995), who considers the non-market sector. See the next section.

Nordhaus and sustainability

Professor Nordhaus has contributed importantly to environmental economics, with Nordhaus & Tobin (1971) *Is growth obsolete* and Nordhaus (1976) *Economic Growth and Climate: The Carbon Dioxide Problem* – with the apt statement "Unlike many of the wolf cries, this one, in my opinion, should be taken very seriously" – and with Nordhaus (1995) extending the calculus of variations with non-market resources, and subsequently the DICE model and geographical modeling.

There are three main points to observe.

(1) Nordhaus (1995) starts out with a promising paragraph:

> "With growing concern about our crowded globe and increasing awareness of global environmental problems, environmentalists and governments have launched a crusade for "sustainable economic development". This concept, popularized by the report of the Brundtland Commission (1987 / TC) and often adopted by critics of economic growth, was defined as "development that meets the needs of the present without compromising

> the ability of future generations to meet their own needs". The general notion here is that humanity is wasting its natural endowments – "natural capital" such as appropriated natural resources like energy resources, nonfuel minerals, and soils; appropriated renewable resources like forests and aquifers; and vital environmental resources like clean air and water, the stock of genetic material, and the present climate. The dangers range from mundane ones of trash to the more ominous ones of economic decline or even climatic apocalypse."

Subsequently, however, the paper (i) defines a mathematical notion of "sustainable income", (ii) fails to define and estimate environmental use, (iii) presents a notion of "knowledge" as a non-market resource relevant for "sustainability", (iv) measures this resource from total factor productivity (TFP) that still excludes the environment, and (v) then concludes "that consumption has historically been far below sustainable income". The reasoning is that future generations will have so much knowledge that translates in TFP that past generations have been a bit irrational in saving so much for descendants who will be rich anyway. This approach reminds of the 1928 lecture by John Maynard Keynes *Economic Possibilities for Our Grandchildren* (included in *Essays in Persuasion* – no reference needed). Nevertheless, it is a bit absurd to start out with the problem of environmental sustainability and the risk of apocalypse and see those disappear in the discussion and the conclusion.

In sum, the Nordhaus (1995) paper is enlightening for the concepts and mathematics involved but at the same time very confusing for the issue of environmental sustainability, i.e. what the Brundtland Commission (1987), Ahmad, El Serafy and Lutz (eds.) (1989), Hueting (1989) and Tinbergen & Hueting (1991) are concerned about.

While many economists neglected the environment, Nordhaus stands out as one who took it serious, but his serious endeavour apparently obscures the fact that he did not take it serious *enough*. **Appendix B** clarifies this, starting with Nordhaus & Tobin (1971) and following the history of this line of research.

An important point in this appendix is: "According to Tinbergen & Hueting it is not a relevant discussion what to choose, either NI or eSNI, and economists should use both figures. The idea is to provide people with information about the state of the economy, and not to impose, as a caste of know-all economists, what kind of "income" people have to use."

(2) Nordhaus (1995) refers to Ahmad et al. (1989) – in retrospect a major publication on implementing sustainability in environmental accounting – and he refers specifically to the first 3 chapters but not specifically to Hueting (1989), chapter 6 in that same volume. Economic science may have missed a crucial meeting of minds here. Nordhaus (1995), in other references, refers to again other authors on sustainability but neither to Hueting's other writings nor to TH. Also, there is no adequate channel from TH towards Nordhaus, as Hueting (2001) clarifies that various of these authors have crucial misunderstandings about the TH approach.

(3) Nordhaus (2007b), *Key Potential Improvements in Statistics and Data for Policies Concerning Global Warming: The Role of Federal Statistical Agencies*, was prepared for the US National Research Council, Committee on National Statistics. It is relevant to note that Hueting developed Dutch environmental statistics and (in a personal communication to the present author) in the past has visited the US Bureau of Economic Analysis (BEA) and found little response to his suggestions.

Appendix C contains my own selection of Key points in Nordhaus's Key points. Here, it suffices to restate his summary recommendation:

> "(33) The summary recommendation here is that U.S. federal statistical agencies need to become even more active in the international statistical system if we are to improve international socioeconomic data for research in global warming. The quality of our models with a global public good like global warming is in a deep statistical sense a "weakest-link" technology. Obviously, the U.S. should not neglect its own data needs or improvements in its own system. However, in the global warming area, there would be a large payoff if the major federal statistical agencies could share their expertise to help countries with limited expertise and resources to improve methodologies and data systems." Nordhaus (2007b)

Apparently, Nordhaus considers Holland to have "limited expertise and resources" since the TH approach was not considered relevant to look into. It is also remarkable that the recommendation is formulated in 2007 while the issue is known since the 1970's. I don't intend to sound humourous or sarcastic, depending upon the author-reader relationship, but I don't think that it would be so advisable that the US comes to Holland to 'help out' with the Dutch environmental statistics – though it would really help out when some American students would be willing to listen and study.

Conclusion

This paper compared a Harvard – Yale approach with a The Hague – Voorburg approach. Jan Tinbergen was present at the roots of both approaches and would have wished integration.

The Stern Review (2006) scared the public and policy makers with costs of climate change that might even rise to 20% of national income. The Review also scared economists for its use of economic theory. Nordhaus (2007a), Weitzman (2007a), Dasgupta (2007a), Tol (2006) and others formulated strong critiques, see also Quiggin (2006) on this discussion and possibly Lomborg (2007) for a discussion for the larger public. These critics have a track record in economic publications on the environment and sustainability, and it is striking that precisely these environmental economists have been so critical of the Stern Review.

As Aronson (1992) explains on the working of the human mind: if a smoker advises others not to smoke, then this has more convincing power than when a non-smoker does so. Apparently having no vested interest, increases impartiality. In the same way, environmental economists warning against the economics in the Stern Review will have more convincing power to the public and policy makers than those supporting it.

The Stern Review understated the environmental challenge by looking mainly at the issue of climate change and not the other issues that are caused by a world population possibly rising towards two-digit numbers and often aspiring at material increase. And now the state of disinformation is increased and complicated by these strong critiques.

The only way to clarify the situation is by considering the arguments. This paper has dutifully tried to do so. We have taken a position akin to Tinbergen & Hueting (1991), see also Colignatus (2008), have evaluated the various points and provided some criticisms to the Stern Review, some of its critics and some of the critics of the those, proceeding to the fourth level of critique. Then, while having kept an open and critical mind, we have returned to the original position: that Tinbergen & Hueting (1991) still provide the best approach, even though it is rather neglected in the economic literature.

A major point is that key authors in this debate mistook some techniques in econometrics for economics itself. This is a risk in economic research that Tinbergen has been warning about, see Jolink (2007), perhaps also out of personal experience. Historians may later judge that he helped to create a monster, namely a system of economic advice that puts higher value on technique than on content. Alternatively, they may find that such a system likely would have arisen anyway due to the Western cultural attitude to mathematical technique, but that Tinbergen managed to install some common sense – that is: if we follow his example.

Appendix A. Possibly entertaining people (continued)

Following the 'stick and carrot' philosophy, and having mentioned the scare above, it seems proper to allow for some entertainment too. Weitzman's remark on 'science fiction' is tempting as well. Let me admit that I write science fiction on occasion, see Acapulco Jones (2007), styled after Indiana Jones but with a better beach. Readers might also enjoy Terry Pratchett (2007) on the life of a Central Banker with a Phillips machine in the basement.

One point to observe is that, with Nordhaus partly in the right court, technology may hold wonderful surprises. On the other hand, the precautionary principle is that we cannot reasonably plan to have what we cannot reasonably expect to have yet. Past rates of growth are misleading since they are contaminated by wrong accounting for the environment.

But of course, our phantasy allows all kinds of escape routes.

An example is that our skin is modified to contain chlorophyll, cutting short the food chain. An example is a subsidy for small people, who indeed require less energy. Soon, the little green men are here.

Another idea is to consider the Mediterranean, concentrate sunlight by mirrors, catch and guide the humid air in pipes towards the Sahara, and let water condensate there.

Colignatus (2006) is a bit more developed and contains two extravagant ideas, just in case that the world is not interested in the Dutch problem of saving Amsterdam. One idea is to use a gigantic geothermal machine, or a myriad of small ones, to create dikes from ice (also freezing the soft underground), and also take the CO_2 out of the air that other nations put there. That might really CO_2L IT. Another is to rearrange the Rhine river, that in the BAU scenario will cause a lot of inland problems too. Perhaps these approaches can be made feasible – I am not an engineer – and perhaps there is a Coase Theorem applicable here.

All these possibilities are as serious as the Weitzman (2007a) explorative remark: “Such emergency measures are likely to be so extreme as to be defensible only for an even-more-extreme environmental catastrophe in the making – perhaps they might include painting all human-made structures on the planet reflective white and creating a “Pinatubo effect” by seeding the upper atmosphere with metallic dust or aerosols. (footnote)”. See Lomborg (2007) for similar suggestions.

Schuiling & Krijgsman (2006) are geophysical scientists who propose to grind olivine to sequester CO_2. This seems a more serious option than the statements above. Schuiling earlier suggested injecting underground limestone layers with acids to create gypsum, thereby raising the floor of Holland. The new label is “macro-engineering”.

It stands to reason, though, that all such measures represent costs. These are expenditures required to restore what we had before environmental scarcity set in. When future newspapers report that Holland has an amazing “economic growth”, due to the construction of dikes and similar projects, then this would derive from misguided national income accounting that masks that the Dutch are actually hurting a lot. In the work by TH we can find ways for proper national income accounting so that such costs do not contaminate our notion of “growth”.

Appendix B. Nordhaus and sustainability (continued)

While many economists neglected the environment, Nordhaus stands out as one who took it serious, but his serious endeavour apparently obscures the fact that he did not take it serious *enough*. This appendix clarifies this, starting with Nordhaus & Tobin (1971) and following the history of this line of research. It will be useful to itemize the comments.

(1) On Nordhaus & Tobin (1971): (a) On catastrophes, they state:

> As for the danger of global ecological catastrophes, there is probably very little that economics alone can say. Maybe we are pouring pollutants into the atmosphere at such a rate that we will melt the polar icecaps and flood all the world's seaports. Unfortunately, there seems to be great uncertainty about the causes and the likelihood of such occurrences. These catastrophic global disturbances warrant a higher priority for research than the local disturbances to which so much attention has been given.

Clearly, ecologists in 1971 had different opinions. Thus, "economics alone" is not the proper reference.

(b) On the index of welfare: the inclusion of other items such as leisure is OK when the goal is to measure welfare from luxuries but the ecological discussion is about survival and no amount of leisure can substitute for a catastrophe.

(c) A more minor point, but still serious enough, is that Hueting (1974, 1980:183-184) criticizes the approach on urbanization.

(2) Nordhaus (1976) concludes to a "little change" scenario with a horizon of 20-40 years:

> In summary, an efficient program for meeting reasonable carbon dioxide standards appears feasible and, moreover, requires little change in the energy allocation for 20 to 40 years. Subject

But later the DICE model causes a similar conclusion – a drift of the horizon. This drift is perhaps due to the discounting, and perhaps this is like the temporal or dynamic inconsistency that may also affect the credibility of a Central Bank. Something to look into.

(3) Nordhaus (1995) gives a laudable extension of the calculus of variations, referring also to apparently a similar paper by Karl-Gustaf Löfgren 1992. PM. As far as I have been able to see, Nordhaus (1994) contains a similar argument and was published outside of the Cowles Foundation; in contrast the Nordhaus (1995) paper apparently has remained a CF mimeo.

(4) The Nordhaus paper actually agrees with the Hueting approach, later adopted by Tinbergen & Hueting (1991), that both the standard measure for national income and the Weitzman (1976) sustainable national income for markets are inadequate when there are relevant non-market resources. Statistical offices around the world must be shocked to realize this. That is, they will know, conceptually, that such definitions exist, but to actually implement them, and to accept that a shortcut made in the 1930s is no longer sufficient for our times, is another story. Statistical offices that associate 'facts' with 'the past' and not with 'reality' that includes a future, have to face that paradigm switch.

(5) The various authors in Ahmad et al. (1989), and in particular Hueting (1989) and Tinbergen & Hueting (1991), are concerned with environmental sustainability, while Nordhaus (1995) generalizes and in fact uses "knowledge" rather than "nature" as his prime example. This generalization is alright as a mathematical exercise but the economic problem was ecological survival and the required adaptation of economic accounting procedures to facilitate survival. Nordhaus's neglect of the basic problem causes all kinds of irrelevant criticisms. For example, he criticizes various authors for not including expected growth of knowledge in their notions of sustainable income. Yet knowledge is not the problem under discussion. In the literature at that time (e.g. Ahmad et al. (1989)), "sustainable income" meant "environmentally sustainable income". It is only because of this misunderstanding of the term "sustainability" that now the prefix "environmentally" has had to be added (turning SNI into eSNI).

(6) Nordhaus (1995) distinguishes different concepts of "income". Here he follows Hicks (1939) in *Value and Capital*. For unclear reasons Hicks's Definition 1 is labeled the "Hicksian definition" (production for a limited period, maintaining capital, that however is defined on prospective returns) and Hicks's Definition 3 is labeled "Fisher's definition" (wealth based, condition on future income). Only the latter would be "sustainable income". This causes a curious criticism that authors who work on sustainable income and who say that they adopt Hicks's notion on income, would be inconsistent. Nordhaus also refers to the UN SNA that uses "Hicksian income" and that would become inconsistent if it would try to implement sustainability "in that manner". This is a very curious way of putting things. Rather, I find the TH position more tractable that the notion of "national income" (NI) is based upon Hicks's definition 1 and that the notion of "(environmentally) sustainable national income" (eSNI) differs

from NI by corrections for environmental sustainability. Again, only the environment, for ecological survival, and not the luxuries.

(7) In the TH work we also find that the definition of NI may well have been adequate at the time of Hicks, i.e. eSNI ≈ 100% NI, but that now eSNI ≈ 50% NI. According to Tinbergen & Hueting it is not a relevant discussion what to choose, either NI or eSNI, and economists should use both figures. The idea is to provide people with information about the state of the economy, and not to impose, as a caste of know-all economists, what kind of "income" people have to use.

(8) Note that Nordhaus (1995) confirms that when the economy becomes sustainable (in his formulas $A(t)$ constant), then NI = eSNI, which is also the TH position.

(9) TH basically calculate only one income value of a base year $Y(b)$, based upon a trajectory of $y(t \mid b)$, note the difference between Y and y, and they assume constant technology given that base year, and thus limited production growth. Their model thus is a specific application of the Nordhaus (1995) model (and not its solution). Conceivably, apart from this statistical approach, a planning agency might make projections of such $Y(t)$ with added expectations on technology. The relation between this $Y(t)$ and the various $y(t \mid b)$ is a bit complex, see Hueting & De Boer (2001).

(10) Nordhaus (1995) contains a very curious example of a "wayward spaceship" that contains a fixed amount of food without possibility of producing more. It does not help the discussion where he suggests that this might be the position argued by environmentalists.

(11) Nordhaus (1995) on risk: "A second point concerns the claim in some environmental writings that the capital-intact definition should apply specifically to "natural capital". (…) Natural capital has a claim to be maintained intact, they claim, because of risks, uncertainties, and irreversibilities in their use. These are more questions of religion than science. The fact that natural capital is misallocated means that we should use the appropriate shadow prices but surely does not imply that the appropriate policy is an absolute prohibition on declining natural stocks. Furthermore, risks, uncertainties, and irreversibilities are hardly unique to natural capital. (…) Natural

capital has no natural monopoly on risk and irreversibility." This neglects that natural capital has a natural monopoly on survival, which is the relevant notion since we are discussing the ecological base of human existence. Thus: (a) "some writings" is unspecified while TH give a well balanced exposition, (b) when the shadow price is infinite then prohibition ensues, (c) the risk that the Greenland ice melts is serious and for a long while irreversible, which is well-documented, (d) the fact that an investor can lose money is not relevant when we are discussing ecological survival. Clearly, Nordhaus (1995) did not fully understand the ecological challenge and it is striking that his references are not to ecologists directly but to economists who discuss the ecology – who need not understand the issue well either, see Hueting (2001) who did take the ecological question seriously.

(12) The TH approach is to impose sustainability by standards and let the economy develop under those constraints. TH subsequently discount with a zero rate to the base year. This is not quite the same as the problem of discounting in the calculus of variations. Nordhaus (1995) does not take account of this and as a result, we find various statements on discounting that are confusing with respect to the notion of sustainability according to the definition of Hueting as used by TH.

(13) Nordhaus (1995) states: "Third, sustainability is an insufficient criterion for judging the wisdom of a particular economic trajectory. (…) Hence there is no normative content in the designation of a path as "sustainable"." (a) This would be a welcome support for the TH approach, since that approach is frequently labeled as a political preference for sustainability, while TH hold that sustainability is an objective notion. (b) However, there is a subtle difference. In the TH approach, both NI and eSNI are conditional notions, based upon assumptions about the preferences of the economic agents. What the true preferences are is unknown and thus what the chosen path means is a bit unclear, except that NI is measured by the statistical office and eSNI is calculated as a model-based correction upon that (what best should be done by that statistical office too). When these figures become available as information to the economic agents, they might adjust their behaviour, with the subsequent year a new set of NI and eSNI. This is clearly a different kind of process than what Nordhaus has in mind, even though the statement still remains valid. (c) There is the notion of a Meta-SWF (social welfare function) that can bring about a regime switch, see Colignatus

(2000). (d) TH recognize that the Brundtland Report choice for sustainability would imply a preference. There is a difference between understanding this preference and its denial.

(14) Nordhaus (1995): "Fourth, some readers have complained that our treatment of natural capital is incomplete and inadequate. We have considered cases where natural capital is a perfect substitute for knowledge, which is clearly unlikely and may be grossly misleading. (…) In addition, we have omitted depletion of natural capital and corrections for externalities (…) We have done this because there are no reliable measures of depletion of natural capital (…) However, the results should not be interpreted as a Panglossian brief for profligacy or neglect. The estimates provided here may be off base if there are sudden or unpredictable declines in economic activity because of malfunctioning markets or unforeseen events. But the best remedy for avoiding disasters is good science not bad economics." This statement is a bit curious. (a) The Hueting, Bosch and De Boer (1992) publication at CBS Statistics Netherlands gave a decent methodology for the calculation of environmentally sustainable national income, which methodology might also be applied to the USA. Thus there was a measure. (b) Precisely because of the unpredictable events, economic science uses the precautionary principle. Thus, based upon this principle, one makes a best estimate, or provides various scenarios, rather than fully neglecting the issue. Note that Nordhaus (1995) predates the Weitzman (2007a) analysis on uncertainty, but has the same "good science versus bad economics" attitude against the use of certainty equivalence. (c) This episode may be a case where lack of knowledge in 1995-2008 actually is a substitute for depletion of natural resources and environmental deterioration in 1995-X.

(15) In the conclusions section, Nordhaus (1995) provides support to TH that the UN SNA are defective with respect to "sustainable income". However, as said, his concept of "sustainable" contains a factor "knowlegde" based upon total factor productivity, that still neglects the environment. This is curious since the subject under discussion is environmental sustainability.

In sum, the Nordhaus (1995) paper is enlightening for the concepts and mathematics involved but at the same time very confusing for the issue of environmental

sustainability, i.e. what the Brundtland Commission (1987), Ahmad et al. (1989), Hueting (1989) and Tinbergen & Hueting (1991) are concerned about.

(16) For the apparent precursor Nordhaus (1994) we find an interesting conclusion:

> "The shame of the current generation in America is, contrary to much popular opinion, that it has probably overinvested in seductive areas like pollution control, farmland protection, and military R&D while underinvesting in dull areas like training, equipment, and applied research. This investment strategy is long in plants and mortars and short in plant and brainpower."

This conclusion is interesting in that some aspects convince by common sense (yes, better education) while other aspects are curious (worse pollution control ?). The article contains the same confusions as Nordhaus (1995) and the same lack of substantial research in the environment.

Appendix C. Nordhaus (2007b) on the role of federal statistical agencies

Key points, selected by me, in the Nordhaus Key points (i), are the following. Note by the way that most of this selection can already be found in the earlier writings of Tinbergen and Hueting.

(1) "The issues involved in understanding global warming and taking policies to slow its harmful impacts are the major environmental challenge of the modern era. These issues pose a unique mix of problems that arise from the fact that global warming is a global public good, is likely to be costly to slow or prevent, has daunting scientific and economic uncertainties, and casts a shadow over the globe for centuries to come. It is also likely to be a major public-policy challenge for the indefinite future, and therefore will require concerted efforts among natural and social scientists to understand its genesis, potential future paths, impacts, and potential strategies to slow or mitigate its impacts."

(2) "The challenge of coping with global warming is particularly difficult because it spans many disciplines and sectors of society and the natural world. Understanding the full ramifications involves areas of geosciences, ecology, economics, political science,

domestic and international law. Each of these disciplines has a well-established group of researchers who are studying the implications and effects of global warming."

(8) "Second, understanding and modeling the "downstream" ecological, environmental, and economic effects of global warming are completely dependent on the results of the "upstream" geophysical sciences. In a sense, economists are sitting by the river retrieving the pearls or flotsam, as the case may be, of results from the upstream geoscientists and their modeling. If upstream modelers do not provide high-quality scenarios for abrupt climate change or sea-level rise or river runoff, then downstream economists and policy analysts cannot incorporate high-quality results into their models. In this respect, one respondent noted, "one of the greatest data/measurement needs for better economic analysis is for more refined (i.e. geographically specific) biophysical impact estimates from the natural sciences. For example, in the case of the United States, perhaps one of the greatest economic impacts of climate change will be in terms of snow pack and hence the flow of the Colorado River, on which much of western irrigated agriculture depends." The economic analyses can get nowhere without reliable "upstream" geophysical analyses."

(9) "(…) The IPCC working group charged with assessing the underlying science has, in the Working Group 1 report of the Fourth Assessment, apparently decided to avoid any probabilistic interpretations of emissions or climatic trajectories. As one researcher commented on this approach, without temperature ranges and associated probabilities, we "cannot do risk analysis of impacts, cannot show that there are near term risks, and cannot evaluate commitment to various levels of abrupt change." Who knows what lurks in the tails of the distributions?"

(11) "Most of the researchers who responded to my inquiry about priorities for data mentioned prominently the abysmal state of our knowledge about the impacts of climate change. We can divide the terrain into market impacts and non-market impacts. In general, it is the non-market impacts that pose the major uncertainties. Within this category, we can subdivide those into managed and unmanaged systems. Human health and gardening are managed non-market activities, while ecological systems would be largely unmanaged. It seems likely that unmanaged systems are the major uncertainty. Researchers identify ecological "hot spots" as particularly vulnerable targets of climate

change, particularly abrupt climate change. (footnote) Several respondents mentioned that we need, in one respondent's words, "to accelerate our measurement of the use and impact of ecosystems and ecosystems services of value to people over the short and long term."

(14) "Turning to the U.S. Federal statistical system, there is one glaring weakness – I would even say one catastrophic error of omission. This is the absence of an independent statistical agency that is dedicated to the design and collection of environmental and ecological data. Indeed, when I did a search of environmental statistics for the United States, I obtained an EPA web site that said, "The Environmental Quality Homepage is no longer available." (…) It is hard to see how the U.S. can undertake serious research on environmental and ecological impacts without an independent statistical agency devoted to this task. The major recommendation in this area is that the Federal government move to develop an independent statistical agency that is devoted to design and collection of ecological and environmental data."

(21) "(…) I strongly urge the Energy Information Agency to take stock of its mission in providing and supporting timely domestic and international data and long-term integrated energy and economic models relating to energy and emissions trends in global warming. The EIA could take a lead role in ensuring that energy and emissions data models used in global-warming studies are comprehensive and reliable."

(22) "I mentioned above that the major gap in our understanding of the economics of global warming concerns impacts, particularly involving non-market sectors. A critical component is collecting better (or at least minimal) data on various non-market processes, particularly involving ecosystems and the environment. A second component is valuation. The third missing component is an organizing framework in which to place the quantities and values. This is the area of non-market accounts."

(23) "There are several areas where a set of non-market accounts would be useful in developing impacts studies. One respondent noted that "the environmental accounting approach, and expanding that approach to non-market activities of the household, is a useful way to frame data." Such activities as "outdoor activities, exposure, time spent in different activities – everything from caring for illness that may be environmentally

related to time spent commuting – could help resolve why people make the choices they do, and how they would value having to change those choices.""

(27) "Issues of non-market and environmental accounting have occupied the Bureau of Economic Analysis (BEA) and CNStat for more than a decade. The BEA produced an early version of its environmental accounts in 1994. This report contained an early draft national balance sheet that contained estimates for non-produced assets. However, shortly after this, Congress issued a stop-work order. CNStat sponsored two reports that strongly endorsed non-market and environmental accounting, but BEA has undertaken only limited work in these areas."

(28) "The recommendation here is that the U.S. should move expeditiously to complete the work plan laid out by the BEA in 1994 and endorsed by two reports from the National Research Council on environmental and non-market accounting. To reiterate a recommendation from the NRC Report on this: "Extending the U.S. national income and product accounts to include assets and production activities associated with natural resources and the environment is an important goal. Environmental and natural-resource accounts would provide useful data on resource trends and help governments, businesses, and individuals better plan their economic activities and investments.""

(33) "The summary recommendation here is that U.S. federal statistical agencies need to become even more active in the international statistical system if we are to improve international socioeconomic data for research in global warming. The quality of our models with a global public good like global warming is in a deep statistical sense a "weakest-link" technology. Obviously, the U.S. should not neglect its own data needs or improvements in its own system. However, in the global warming area, there would be a large payoff if the major federal statistical agencies could share their expertise to help countries with limited expertise and resources to improve methodologies and data systems."

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