REJOINDERS TO SYMPOSIUM AUTHORS

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(Translation: Nigel Harle)

1. Introduction

Having been writing for thirty-five years on environmental valuation, growth and sustainable national income, after a five-year incubation prior to putting pen to paper, and having spent twenty-five years (1969-1994) setting up and elaborating a system of environmental statistics, the indispensable raw material for practical calculations, my main desire today is for agreement on theory, *i.e.* on method. Agreement among the authors working in this field, who since the seventies have come to refer to themselves as environmental or ecological economists, and also between them and the vast majority of economists, whose thinking about scarcity and welfare is enacted in an ecological vacuum. My own position lies somewhere in between, perhaps, for my work is regarded by many ecological economists as 'neo-classical' - and they use the term in a clearly pejorative sense. I certainly do not live in an ecological vacuum, though, for the environment is the pivotal focus of my work, because I consider it the principal economic problem facing mankind.

What concerns me most is the enormous differences in the results generated using various methods of environmental valuation and in the associated figures for a national income adjusted for environmental losses, referred to these days as 'green accounting'. Until such time as the reasons for these differences are explained, it will be impossible for serious people to take this work seriously. And until the work of environmental and ecological economists is

accepted by a significant proportion of colleagues whose main concern is not with the environment, that work will have only minimal impact in society and on political decision-making.

If the mountain will not come to Mohammed, Mohammed must go to the mountain. By 1974 several approaches to environmental valuation had already been published, based on notions of travel costs, willingness to pay and to accept, and so on, and in that year I repositioned these within a new theoretical framework, adding some elements of my own. My basic point of departure was the simple notion that without preferences and costs there can be no economic value; my framework was based on the concept of 'competing environmental functions' as economic goods. Although the English edition of that study was held up for six years because of obstruction (I refer the reader to the author's Introduction there and to Robert Goodland's paper in the present volume), the essentials of my work have been available to readers of the English language since 1971 or 1974, as can be seen from the list of publications appended to Goodland's paper (see, in particular, Hueting 1971/1974 and 1975). It is on this 1974 study that all my later work is grounded. It was only after 1974 or 1980 that most authors began publishing on environmental valuation and green accounting. Although it is accepted scientific form to either build on the work of one's predecessors or reject it on the basis of reasoned argument, this was a privilege I was not afforded. Given the crucial importance of agreement on method, in our contribution to this volume Bart de Boer and I have sought to identify the precise differences between our theoretical approach and the methods of other authors. In doing so we have examined the work a number of environmental and ecological as well as general economists, several of whom base themselves on so-called general growth theory. The comforting conclusion is that the differences in results can be traced back either to differences in assumptions (frequently undisclosed) or to anomalous usage of key terms, such as the standard notion of 'production', the sum of values added by labour, and 'environmental function', the term I introduced to denote a *possible* (potential) use of our physical surroundings.

In my rejoinders to those authors who were kind enough to contribute to this volume I arrive at the same optimistic conclusion. Although some consider their approach to differ in certain respects, below I hope to demonstrate that in most cases this conclusion is borne of misunderstanding. One paper (Pearce *et al.*) criticises my work for asserting the exact opposite of what I implicitly intended and, in at least five publications, have explicitly stated. The supposed difference of view thus soon evaporates. Several other contributors do not address my work explicitly. In one case though (Mäler), De Boer and I conclude below that here, too, the (pronounced) discrepancy in results is rooted in making different assumptions.

One final note. Some of the contributors took up the offer of receiving a courtesy copy of the list of references, articles or the 1974 study (*New Scarcity and Economic Growth*), whilst others declined. In my rejoinders to the latter I have felt free to quote from any or all of my publications, references being given according to the list of publications appended to Robert Goodland's contribution to the present volume. To facilitate reading, wherever feasible reference

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[1] I am grateful to Bart de Boer, Lucas Reijnders, Wouter Achterberg, Cees Maris and Leontien Tels de Jong (classicist) for their help with certain passages of the text.

has been made to the summary paper by Bart de Boer and myself (referred to as 'H&B' for convenience), which then points readers to my earlier work.

I am very much indebted to the editors for giving me the opportunity not only to present my work on environmental valuation and SNI but also to elaborate on specific points of interest in this series of rejoinders to the symposium authors.

2. Paul Ekins

Paul's paper perfectly reflects the essence of my work: competing environmental functions - possible uses of our physical surroundings - constitute new scarce goods (Hueting 1969a, 1974a) and sustainability boils down to maintenance of vital functions. From here, the rest of my work follows on logically. For this I am very glad, all the more so because on this point I have not been overindulged (see introduction). A few comments are nonetheless in order, for on several issues Paul in fact appears to have misinterpreted my work. These relate to: (1) adjustment of the national income for loss of environmental functions, and (2) sustainable use of the functions of non-renewable resources. In addition, he neglects to consider one key element: my statistical studies demonstrating that the bulk of growth is generated by the most environmentally damaging activities; this is discussed below under (1). Together, these factors have led Paul to draw some erroneous conclusions about my work. His paper also contains several minor misconceptions, but for the sake of brevity I shall not treat these here.

Re (1). There are a number of misconceptions regarding my position on the adjustment of national income.

(a) On p.27 (draft) Ekins states that (marginal) damage costs are the theoretically appropriate measure for correcting NNP, if considered as a measure of welfare. But, as he notes here and on p.24 (draft), citing my work, NNP or production is not a measure of welfare, welfare being a far more complex concept, and consequently national income can never be corrected on this basis. Apart from the fact that solely damage costs are not the appropriate measure for correcting NNP (see below), Paul has misunderstood me here. On many occasions I have written as follows on this point (*cf.* H&B, Section 3). Although not amenable to direct measurement, welfare is arguably dependent on a number of factors which *are* measurable. One of these is the trend in production volume, provided it is constituted in accordance with preferences. As long as environmental functions remain free goods, production represents the sum total of all scarce goods. When functions become scarce this is no longer the case, and a correction must be made in order to restore an important indicator of welfare (*cf.* H&B p.13, draft, incl. the quotation from Tinbergen & Hueting, 1991). This correction should not, then, be based exclusively on the damage costs arising, as Ekins posits, but on shadow prices given by the intersection between the marginal elimination cost curve (supply) and an assumed demand curve, which represents (*inter alia*) both actual and anticipated damage. These number as many as the (reasonable) assumptions that can be made about preferences (*cf.* H&B, Sections 5.1 and 5.2, and (b) below). Ekins' argument that correction of national income is impossible because production is not an (all-inclusive) measure of welfare is therefore incorrect.

(b) It is entirely erroneous to hold, as Ekins does on p.29 (draft), that Hueting *et al.* (1992) is based on the maintenance cost approach. H&B, Sections 4 and 5, elaborates why the approaches introduced subsequent to the Hueting 1974 study, which are based solely on costs or solely on preferences, are (already) constituent techniques of the methodology presented in that study: in the absence of costs or preferences, there is no economic value, and these two elements are, moreover, inextricably linked (*cf.* (a) above). In the 1974 study I do not employ the term 'maintenance costs', a far later and to my mind rather unhappy notion, but rather 'elimination costs', in which elimination is defined as at-source removal of the environmental burden such as to fully restore environmental functions.

(c) In the quotation in the first paragraph of p.29 (draft) the "burden that has mounted up" relates not to the method but to a particular case that may sometimes arise (Hueting *et al*. 1992, p.14 should be p.12).

(d) On pp. 27, 28 and 29 (draft) Ekins clearly intimates that I am unaware that internalisation leads to changes in prices and consequently to changes in the composition of economic output. Thus, on p.29 (draft) he asserts critically "(…) they are static cost calculations (…) they cannot simply be deducted from GNP". This is such a massive criticism that, if it were true, I would be better to pack my bags as an economist. To my relief I found several pertinent statements in earlier publications. In Hueting (1991f, pp.205-206) we read: "To be absolutely clear, it should be pointed out that this is a partial equilibrium and static approach. Effects on other sectors of the economy as a result of taking measures and reducing activities are not considered. Neither are future developments (…) because in the model to be used, a large number of assumptions would have to be incorporated (…) The problem of the environment is a problem of allocation". The last sentence is in fact almost a literal translation of the title of one of my articles from the years 1967-1970, published in the volume 'What is nature worth to us?' (Hueting 1970, in Dutch). There, I assert that if nature is really that valuable to us, current allocation is suboptimal; reallocation then leads, via internalisation, to a lower national income and greater welfare; this is indeed the subtitle of the 1974 study: 'More welfare through less production?' Remarks similar to that cited from Hueting (1991f) are to be found, *inter alia,* in Hueting (1992f, p.37) and Hueting (1991e, p.32). Hueting (1995a, p.223) refers to "(…) the intention to investigate whether the assumptions with respect to technical measures and shifts can be validated or improved with the aid of a comprehensive economic equilibrium model". This intention, for reasons elaborated under (e) below, was realised not at Statistics Netherlands but at the Institute for Environmental Studies (IES) (*cf.* Verbruggen *et al*., this volume).

(e) As part of his critique under (d), Ekins states (p.29, draft) that Hueting *et al*. (1992) believed that the values (or costs; *cf*. H&B, Section 5) they found can simply be deducted from the standard national income. That is correct, and I still suspect that this is defensible as a rough approximation of sustainable national income (SNI). But this is by no means to say that changes in prices or in the composition of economic output were ignored or that a general equilibrium model approach was rejected (*cf*. quotes under (d)). There was a mix of theoretical and practical reasons for such a rough proxy.

The main theoretical reason was grounded in the insight that a shift towards more environmentally benign activities has a substantial negative effect on national income, because the bulk of production growth is generated by environmentally damaging activities; this is demonstrated by an analysis of the raw material of the Dutch National Accounts (*cf.* Hueting 1981b and 1992b, and H&B, p.2 and p.31, draft, in conjunction with Section 5, p.12 and p.13, draft). Most of the economists with whom I had dealings in the early '90s held this effect to be zero or thereabouts and therefore non-existent (see, for example, Eijgenraam 1992), so that mere deduction would lead to a substantial underestimate of the level of SNI (there were no fears of too small a correction). A simple example may serve to clarify matters. Assume that environmental measures must be taken for 1000 products, on the production and the consumption side, and that these measures cost $ 100 per product. According to my critics, if half the consumers are moved by the price rise resulting from internalisation to switch to environmentally benign alternatives, the negative impact is then not $ 100,000 but $ 50,000. If, as I hold, the effect of the shift is substantial, there is by no means necessarily an underestimation of SNI; the correction may then in fact even exceed the aggregate costs, certainly if direct shifts, forced by levies, have to be applied because technological measures prove insufficient for attaining the sustainability standard (*cf.* H&B, p.31, draft: the new price ratios better reflect a sustainable situation). The Tinbergen-Hueting rough estimate of a sustainable world income (*cf*. Hueting 1991d) was based partly on the 'shift effect'. Tinbergen shared the view that such an effect does indeed exist, because, he said, "It is based on statistical fact". In light of the provisional results of the IES modelling exercise, the figure found does not appear extreme: around 50% of world income (*cf.* H&B, p.2, draft, and Section 1, Consequences of the principles, 11, as well as Verbruggen *et al*., this volume). This renders it plausible that a shift effect indeed exists and that it is substantial. I refer the reader, finally, to several arguments in Hueting 1995a (p.217).

Another theoretical reason for accepting 'simple deduction' as a rough approximation is that modelling, too, involves major uncertainties, because attaining sustainability entails large changes and a shift in basic priority from production growth to environmental conservation (*cf.* (f) below). This I have stated on repeated occasions: in Hueting 1992d, for example. On p.18 of the WWF edition (p. 18-19, CBS edition) I write as follows: "The labour released by reduction (…) will be put to work for the production of alternatives (…) a description of future patterns ought to be based on a series of additional assumptions with regard to reactions of consumers (…) This will certainly exceed the predictive capacity of the existing econometric models, because a change of priority (…) means a trend break in economic behaviour (…) Moreover, a package of measures is involved, the introduction of which will require a long period (…)".

There are additional reasons why an SNI, however it be calculated, can provide no more than an approximate indication of the volume of a sustainable level of production and consumption (national income is an index of *volume* changes). One of these is the insolvable problem of a composite price index, already referred to in the Hueting 1974 study. As stated in the (extensive) footnote 8 on p.229-230 of Hueting 1995a, for this and other reasons, not only is computation of an SNI strictly speaking a disputable exercise; the very concept of national

income is also impossible, But, again as stated, I choose not to press the point as long as national income figures continue to be calculated and comparison thereof remains the yardstick of economic success, with all the attendant existential risks to future generations that that implies. The latter has to do with the motivation of my work, and that has certainly been well understood by Paul Ekins, witness his words on p.34 (draft).

Following on from here, the following should also be noted. Because preferences can be established only very partially, shadow prices remain unknown and, with them, the value of environmental functions. But this means that the true value of the goods produced and consumed at the expense of environmental functions is equally unknowable. We must therefore weigh two unknown values - and not a known against an unknown (see e.g. Hueting 1974a and the 'Three Myths' article, this volume). If information is to be provided, therefore, assumptions must inevitably be made regarding relative preferences for produced goods and environmental functions. It follows that not only are SNI as well as other 'green' national incomes based on assumptions and therefore hypothetical, but also that the volume of the *standard national income*, conceived as the contribution of production to welfare, *is likewise based on assumptions and is therefore hypothetical*. One such assumption is that preferences for sustainability are virtually zero: whether this is true cannot be proven one way or the other. There is, besides, another reason why market prices and the national income constructed from them are to be seen as representing probabilities rather than certainties. This reason is stated in my rejoinder to Herman Daly.

As I mentioned, there was also a practical reason for accepting a 'simple deduction' in order to obtain a rough proxy for an SNI, which I here briefly (and incompletely) recapitulate. The competence of Statistics Netherlands to undertake an SNI modelling exercise was seriously disputed. In Parliament the government was requested, for a second time, to provide a calculation of a national income adjusted for environmental losses. This led to a meeting between the ministers of Economic Affairs and Environment and the present author. The upshot of the meeting was a grant to IES to elaborate the SNI according to Hueting in a modelling study (see (d) above).

(f) Ekins cites, uncritically, the familiar Factor Four and Factor Ten, accompanied by a doubling of national income, or more (p.33, draft). Given (e) above, Ekins appears to have overlooked the following. To bring about a factor-two increase in the vast bulk of national income, average labour productivity must be doubled. To achieve this average increase, however, the labour productivity of the approximately 30% of productive activity (in terms of labour volume) that generates the bulk of production growth must far more than double. These are precisely the sectors that burden the environmental most. For this reason, for the approx. 30% of activity having greatest environmental impact, the reduction of environmental burden must be greater than a factor four or ten; how much greater depends on the degree of expansion of the environmentally burdening sectors; this need not be proportional to the increase in productivity, as argued in Hueting (1981b, 1992b): sectoral expansion is something entirely different from sectoral contribution to growth, because (an increase in) added value is not the same as (an increase in) volume; growth of national income refers to a change in

volume. To achieve a doubling of national income, moreover, the cost of applying the as yet non-operational technology must be much lower than in the current situation. Furthermore, these prognostications take no account of man's ongoing encroachment upon physical space (the principal cause of species extinction), nor of the fact that ongoing growth requires ever-greater efficiency to achieve the desired environmental result, which means that with new technologies, too, one ends up in the progressively rising portion of the elimination cost curve (*cf*. 'Three Myths' article, this volume). While sharing Ekins' optimism regarding technological progress, these kinds of 'Factor Four or Ten' recommendations and forecasts are extremely risky for nature and the environment. More in accordance with the precautionary principle of sustainability would seem to be the conclusions of Tinbergen in Hueting 1991d: "(…) accelerate development of new technologies, such as flow energy (…) permit no further production growth in rich countries (…) stabilize the global population as soon as possible (…)"; and H&B, Section 1, Consequences of the principles, 11 and 12: (…) Assuming a preference for sustainability, welfare will increase by pursuing the transition route to a sustainable income level ; this level amounts to about/circa 50% of current income.

Re (2). In order to clear up misconceptions regarding non-renewable resources, let me first briefly recapitulate the essentials of the Tinbergen-Hueting-De Boer-Bosch methodology, focusing on those aspects that tend to be misinterpreted. This methodology has been published on several occasions, including Hueting (1992d and 1995c) and is also summarised in H&B, Section 7. I shall then indicate the main misinterpretations made by Ekins.

As with the renewables, the core of the matter is to derive the appropriate sustainability standard and to estimate the cost of achieving that standard. And, again as with renewables, the sustainability standards for non-renewables represent constant function levels that can be maintained *ad infinitum*. As stated, 'function levels' refers to the availability of possible uses of the resource and its substitutes (i.e. flows), while 'possible' here means: possible under the constraint of the sustainability standard. One key difference with the renewables is the prognostic element consisting of the assumption that the relative rate of substitution, efficiency improvement and recycling (and thus of technological progress) found in practice will persist into the future. The inescapability of this 'methodological' difference has been argued (*cf.* Hueting 1992d, 1995c and H&B, Section 7). Because non-renewable resources must perforce ultimately be entirely replaced by substitutes, an additional adjustment has been incorporated if the relative rate of substitution found is too slow to satisfy this condition (*cf*. H&B p.38 *et seq*., draft); this raises the costs of achieving the standard; a method is provided for making an approximate calculation of these costs.

The sustainability standards for the use of non-renewables and their substitutes will generally be (substantially) lower than the current use levels. Moving to a lower level will always have a negative effect on national income, regardless of whether this is achieved by technological means or via shifts (*cf*. (1)e above). The discovery of new stocks leads to a higher (i.e. 'milder') standard, because the depletion horizon is extended. But there is still rigid adherence to the requirement that the resource's function(s) must ultimately be taken over by

substitutes, in order for the (constant) function level (the flow) determined by the standard to remain available *ad infinitum*. Fulfilment of this requirement is (again) based on the assumption that the relative rate of substitution - generally increased at additional cost - can be maintained in the future. As stated in the aforementioned publications, such assumptions make the standards less 'solid' than in the case of renewables, but, as also stated, rejection thereof has bizarre consequences. The standard for a particular resource in a given country is derived from the global standard, in proportion to the country's share in global consumption of the resource.

Ekins' principal misconceptions regarding our methodology are, now, the following.

(a) The method was not "developed for one particular economy" (Ekins p.20, draft), but for the countries of the world together.

(b) The flow of functions from stock and substitutes need not be maintained at the current level (Ekins p.19, draft). The level to be sustained may be equal to, higher than or lower than the current level, but is generally lower.

(c) Consumption of a particular resource "in a given period" will never fall to zero, and certainly not if there are "substantial discoveries" (Ekins p.19, draft). In our method, resource consumption gradually approximates zero.

(d) The Tinbergen-Hueting-De Boer-Bosch method cannot be construed as an application of the strong sustainability principle, for it rejects the distinction between strong and weak sustainability as untenable (*cf*. Hueting 1998b as well as H&B, Section 7 and Section 1, Consequences of the principles, 2). If this distinction is to be made at all, we are here concerned with weak rather than strong sustainability, as posited by Ekins (p.21, draft).

3. Herman Daly

In the debate on nature and the environment there are few people with whom I feel such affinity as Herman Daly. He too deems production growth, as measured in the national income, to be at the heart of the environmental problem. I had therefore expected my rejoinder to Herman to be easy. But Herman has a habit of making people pause for reflection and I indeed prove no exception. In his paper he poses a key question that is scarcely ever asked: are there such a thing as objectively good preferences? If the answer is 'yes', then according to Daly this means that alongside subjective preferences there are also objective preferences, since the subjects subordinate their preferences to what is objectively good (or ought to do so). This leads logically to the thesis that objective values exist, alongside subjective.

One of the artefacts used by Daly in defence of this thesis is my perpendicular demand curve, that is to say, perpendicular in the relevant range (*cf.* H&B, p.19, draft). In doing so, he misinterprets this curve. This must have been a conscious move, because in January 1999 we corresponded about the background of the curve, on which occasion I plainly stated my view on the matter. This view has been reported (and clarified relative to earlier formulations) in my publications of 1992 and thereafter: the perpendicular curve is based on an *assumption* regarding *subjective* individual preferences, *viz.* that subjects have an absolute preference for attaining standards of sustainability for vital environmental functions; the absolute character of these preferences is bound up with the nature of a standard; the assumption is inevitable

because preferences can be deduced only partially and is legitimate because of the existence of what I have termed 'blockages' (*cf.* H&B, p.18, draft). Daly posits that my perpendicular 'demand curve' is intended to be an expression of objective value, not individual preferences. This is then compatible with Daly's own perspective, in which the attainment of sustainability is an end that is objectively good and one to which the subjects conform their individual ends or wants and thus their preferences (reflecting the relative weights of those wants); these individual preferences are thus objective, for they are derived from an objectively good, generally valid end. This, at least, is how I understand Daly after a close reading of his paper.

With his interpretation, Daly apparently wished to initiate a debate on preferences and the underlying ends or wants (two words for the same notion), and not only those of human beings but also of other organisms. For this I am grateful, although I myself have a different outlook. For the same holds true for perspectives as for assumptions: rendering them explicit improves the quality of the information, because readers then know what they are up against.

Of somewhat less apparent interest are Herman's remarks about a perpendicular supply curve for environmental functions. At the end of the present section I hope to demonstrate that such a curve is an impossibility.

In treating Herman's paper, I shall first expound my view on economics, which I shall then confront with his theses. However, these theses in fact imply an outlook on the world that co-determines Daly's perspective on economics: there is thus an underlying perspective or 'vision'. To this I shall turn after discussing point (1) below. Those who dispute the scientific nature of the portion of my rejoinder ensuing from 'vision' and thus from personal experience and beliefs in the widest sense I shall not counter.

My view on economics, by no means unique, can be summarised as follows. Economics rests on assumptions. The basic assumption is that in their dealings with scarce means human beings endeavour to achieve maximum satisfaction of their wants; 'endeavour', because mistakes are possible, and may sometimes be serious. This is a tautology, because the opposite is nonsensical, but it is nonetheless of the utmost importance and all too frequently forgotten. Wants or ends are meta-economic and are not for economists to judge. Ultimately, the economic problem boils down to the problem of choice arising from scarce means and conflicting ends (or 'purposes' in Daly's terminology). The bottom line - the satisfaction of wants, i.e. welfare - is not amenable to direct measurement. And so we make do with measurable factors which are assumed, by reasoned argument, to have an influence on welfare (*cf.* H&B, Section 3, with background literature).

From this it follows that an assumption regarding wants must *always* be made. The assumption must be reasoned through, it must be plausible. In my work and in the summary thereof in H&B, the plausibility and legitimacy of the assumption of preferences for sustainability are defended. In responding to Paul Ekins, I repeat once more that it is not only the prices of environmental functions that are unknown because preferences are unknown; the correct market prices are likewise unknown, and from that I draw the conclusion that the standard national income is also based on assumptions and is therefore just as hypothetical as the

sustainable national income (SNI). To this I would here add another argument, one that is scarcely ever mentioned in discussions, if at all. To hold market prices and the national income based on these prices for certainties is to assume implicitly that there is absolute consistency between wants and actions. This consistency has been disputed, *inter alia* by Marcuse (1964), who posits that we are caught up in the existing structure of production and consumption and in our actions are consequently hampered in satisfying our true wants. Others have expressed similar views (*cf.* footnote on p.88 of Hueting, 1980a). There is no way to decide this issue one way or the other: it is a question of plausibility, and I myself hold the opposite of Marcuse's thesis to be the more plausible. But I certainly do not reject the thesis as nonsense, when, for example, I see parents, *nolens volens*, driving their children to school or the playground because of the dangers of traffic (see, for example, Hueting 1974a, 1980a). Kuznets would probably characterise these expenditures as intermediate and thus as costs (*cf*. Hueting 1974a). Summarising: (1) people buy cars, meat and holiday flights because they like them; and (2) the assumption that the positive value of these products is higher than the negative value of the inherent loss of vital functions, which is implicitly made when constructing and using national income figures, is disputable; other assumptions are defensible.

Proceeding from this view, I shall now turn to what I consider to be the principal issues raised in Herman's article.

(1) For Daly the only valid definition of (national) income is that of Hicks' *Value and Capital* and it is this that should, he holds, be my basic point of reference. Hicks' concept of national income, says Daly, is "built around a prudential purpose" and consequently not based on individual preferences. That purpose can be formulated in two ways. The first is: net consumption of capital is to be avoided. If capital is consumed this counts as negative income, for which an appropriate correction should be introduced to arrive at the correct figure. National income, in Daly's view, is therefore by definition sustainable and the term SNI a pleonasm.

But Tinbergen, in the thirties a chief architect of the system of National Accounts and the concept of national income, and implicitly also Kuznets, for whom the same applies, albeit to a lesser degree, did proceed from individual preferences, with Tinbergen making clearly explicit the assumptions he thereby made (*cf.* Hueting 1974a, 1991d). Given that official national incomes are based on this concept and that their growth enjoys top priority in economic politics the world over, to the detriment of the environment, there is an eminently practical reason for conforming to this concept in correcting for environmental loss: comparability. But there is also a theoretical reason for following Tinbergen rather than Hicks, and it is equally important. From the view on economics outlined above, Hicks' income concept rests on an assumption vis-à-vis individual preferences: that subjects are prepared to sacrifice a portion of their present consumption, *viz.* up to the time-dependent and therefore variable level required to keep constant the capacity for producing consumption goods, expressed as the aggregate stock of man-made and natural capital. That level (the Hicksian income according to this first formulation) may thus be lower than the level actually realised, even at the start of the year. In Hicks' day the implicit assumption regarding individual preferences was anything but robust:

structural, long-term production growth was (and for many still is) an eternal given and the environment was still conspicuously absent in the economic literature (*cf.* Tinbergen, in Hueting 1974a, 1991d). Applied to the situation post-1970, however, the assumption is certainly robust.

This can best be clarified with reference to a second, oft-used formulation of Hicksian income: income is what one can spend (consume) in the course of a year without being worse off at year's end. But whether one is better or worse off depends on one's preferences for current consumption relative to future consumption options. If the latter weigh more heavily in a person's preference pattern, then in pursuing Hicks' 'prudential purpose' (Daly's phrase) he or she will be better off, but at a substantially lower level of consumption. If current consumption weighs more heavily, they will be worse off.

Stringing individual years into a long term time series, it is only the second formulation that yields a constant national income. If preferences for sustainability are moreover assumed to predominate, this yields a national income that is the maximum achievable while securing sustainability; this is then the SNI according to Hueting. (For the reasoning, see my response to Sylvia Faucheux and Martin O'Connor and also H&B, p.32, draft, for the time horizon taken.) But, in contrast to the Hicksian income, this SNI does rest on a robust assumption regarding preferences: that subjects are 'better off' if, and only if, vital environmental functions remain available *ad infinitum*. Contrary to the situation in Hicks' day, those preferences involve a willingness to accept the sacrifice of a substantial reduction in consumption and a drastically changed consumption pattern. Such willingness and acceptance can be neither proven nor falsified and can therefore only be assumed. To dispute that securing sustainability requires sacrifice is to dispute the existence of the environmental problem.

In addition, the magnitude of this sacrifice, and with it the level of the SNI, can in practice only be estimated with the aid of quantitative sustainability standards established with the greatest scientific rigour (*cf.* Hueting 1988b and H&B, Section 7, draft). Again, this is not something to which Hicks attended, because in his day the environment was not an issue. Neither, certainly, did Hicks foresee a substantial sacrifice of consumption. In designing the concept of 'national income' Tinbergen likewise ignored the environment, because it was not then a factor of significance; as from the early seventies, however, he was a firm advocate of correcting national income for environmental loss (*cf.* Hueting 1974a, 1991d). Whether Hicks, too, adopted a similar position in his later life we do not know. Daly as well as Salah El Sarafy, whom he cites, ignore both the assumption regarding preferences and the practical necessity of estimating physical sustainability standards. My conclusion: in the novel situation of a new fundamental category of scarce goods, the established concepts of the economic literature cannot be applied gratuitously (*cf.* H&B, p.11, draft).

(2) The second point, more than the first, is concerned with the deep, philosophical questions raised by Herman in our correspondence, which I have interpreted above as his underlying perspective or 'vision'. Herman makes it especially difficult for me here, for when he uses the word 'purpose' he appears to have two entirely different senses in mind, which he does not disentangle: (a) '(human) ends', in the established economic sense of the term, i.e. meaning

wants or needs (fulfilling wants is the same as attaining ends) and (b) the general philosophical or religious meaning of 'object for which a thing exists, final cause' (Oxford Dictionary). What is concerned in the latter case, as I understand it, are such questions as: Is there a purpose to life, to existence, or is it without purpose? What is the meaning or purpose of existence? This impression is reinforced by Daly's use of the term to encompass all other organisms on the planet, too.

Daly answers the question whether human beings and other organisms have a purpose unshakingly in the affirmative. As an agnostic, my own answer must be: I do not know. On the sole basis of Daly's text I cannot deduce what exactly he holds to be the purpose, but my interpretation is that conservation of nature and the environment, and thus sustainability (or, more religiously coloured, 'stewardship'), are certainly part of it.

I use this interpretation to bridge the gap to the strictly economic sense of the term 'purpose'. Daly addresses my concerned question: If individual preferences are rejected, by whom or by what are our preferences determined? Not, evidently, by government. Neither is that indeed feasible, of course, certainly not in the case of sustainability. Although many governments have rallied behind 'sustainability', following the Brundtland Report, they still continue to give top priority to the conflicting goal of 'production growth': governments build roads, fill in estuaries to construct harbours, open up natural areas to development and neglect to make the market operate in a legislative framework that might lead to sustainability (internalisation). The Brundtland Report is itself a matter of conflicting goals, it should be added (*cf.* Hueting 1990c). Daly's answer to my question, now, is the following: 'purpose' is an objective value, a moral compass ("magnetic north"), a generally valid goal which is objectively good and to which both individual subjects and governments (should) subordinate their other objectives.

But this answer begs new questions, such as: (1) How do such 'objectively good' goals arise, and in particular that of 'sustainability'? (2) Alternatively, do they not arise but simply exist? (3) In that case must they be 'eternal' and therefore immutable? (4) How do individuals recognise the objectively good goals to which they (should) conform? From a close reading of Daly's text I distil the following answers to these questions. These objectively good goals are simply there. They are immutable over time. The subjects are well aware what these goals involve. They also know they should (really) show the courage or willingness to subordinate their other wants or goals accordingly - in the realm of sustainability, the goal of additional consumption or unprotected sex, for example. They should also refuse to allow themselves to be manipulated by governments that give priority to goals that conflict with the objectively good. Sustainability is, for sure, an objectively good goal.

Here we have a view on economics derived from a particular outlook on life, on human existence. Herman Daly is a believer. As an agnostic, I lack such faith. But before the reader rejects as eccentric the notion of a person's faith influencing his or her view on economics (including sustainability), let me recall Adam Smith, often cited as the founding father of economics. For was it not Smith who said: the baker bakes his bread as well he can, not to serve his fellow man but in his own interest? Herein, too, lies embodied an outlook on the human species that determines the perspective from which economics is practised.

Above, I have stated my own view on economics. In consciously misinterpreting my perpendicular demand curve, Herman is in fact implicitly asking me: What is your underlying view or 'vision'? Challenged thus, and for this reason alone, I shall answer that question. Compared with fifty to sixty-five years ago, when I held a firm belief, there remains today but one element: a belief in information. I proceed from the assumption that if individual subjects are in possession of appropriate factual information, they will make fewer and less serious errors in endeavouring to attain their goals or, in other words, maximise their welfare (see above). I make no pronouncement on the virtue of those goals (*cf. New Scarcity* and H&B, Section 3, draft). In religious terms: it is God (rather than man) who is the judge of motives. I thus assume that when subjects have made their choices on the basis of correct information the outcome thereof is 'good' in the sense just mentioned.

The hardest problems arise in the case of collective decisions concerning collective goods such as the environment (sustainability), democracy, and war and peace. As history shows, it is in these realms, above all, that agreement on the validity of information is often lacking, that information is often manipulated or, worse, supplanted by disinformation. History also shows that subjects experience bitter remorse, after the event, if they have acquiesced unprotestingly, due to ignorance, indifference, impotence (real or imagined) or lack of courage, to decisions which for them have proved disastrous and which were taken by individual subjects constituting government, even if those subjects were elected democratically.

Examples are rife, even restricting myself to what I myself have witnessed (sometimes at close hand). In the democratic elections of 1930, Hitler's NSDAP became Germany's second largest party; in 1933 he was elected Chancellor, by way of the democratic procecs. A small number of individual subjects informed the world about the lies on which that victory rested and about subsequent turns of event. I was seven when I first heard that information. With my own eyes I saw how the majority chose not to believe that information ('the easy way'?) and how already then, four years prior to the outbreak of war, resistance to an inevitable occupation was therefore mounted by a small group of individual subjects. Today, that information is universally believed, and remorse is bitter. Shortly after the war, the democratically elected individual subjects comprising the Dutch government chose to undertake two 'police actions' in Indonesia. The vast majority of the population acquiesced. From Dutch citizens in Indonesia information was available to the effect that those actions constituted a colonial war against freedom fighters. There were but few who gave credence, however. The vast majority believed the aim was to restore law and order, which had been disturbed by a *small* group of 'terrorists'. That majority also included the *small* group of individuals from whom I had heard what was going on in Germany ten years prior and who had mounted resistance, regardless of personal consequence. Today, remorse is the predominant sentiment.

In India, marked on the constitutional atlas as a democracy, I have seen how (widespread) child labour is accepted by much of the population; in some of its manifestations the term slave labour is closer to the truth. This acceptance is likely to decline, however, when information on the consequences for these children sinks in to people and the size of families decreases. In my experience, the latter will only be the case if the status of women is improved.

For thousands of years slavery and the denial of women's rights, including passive and active voting rights, were accepted facts. In ancient Greece, where the word and notion of democracy were born, the very concept of suffrage for women was not even raised, even by women; slavery was an accepted fact of life. The same held true in classical Rome. Indeed, denial of women's suffrage was generally accepted in both Europe and America until the twentieth century. In those countries, slavery was long accepted by the vast majority of the population and their churches. Today, shame predominates. At any particular time, some groups of individuals consider themselves *genuinely* superior. Herein lies part of the cause of these historical events, and of what is still happening today. But human perspectives and the preferences based upon them are continuously evolving.

All these phenomena, from personal experience or from literature, have led me to a different outlook from that of Herman Daly on a number of issues. Let me mention the principal of these. (1) My faith in such abstractions as 'government objectives' has declined. (2) Democracy, contrary to what Herman holds, certainly does find itself subjected to a popular vote - every day anew. (3) Individuals having different preferences which, contrary to what Herman holds, are not *a priori* either good or bad, may, again contrary to what Herman holds, most surely engage in dialogue. They may exchange information, validate that information and discuss interpretation thereof. Indeed, democracy requires that they do so as an intensive and ongoing process. (4) Perspectives on good and evil evolve over time and differ from culture to culture, often markedly so. But on one point we can wholeheartedly agree: individuals should not allow themselves to be manipulated by other individuals, whether embodying government or otherwise.

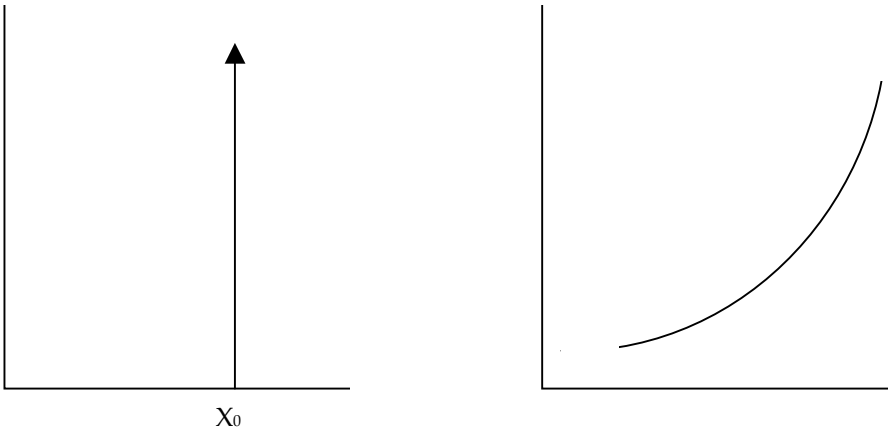
Above, most of what there is to say about preferences for sustainability has, *mutatis mutandis*, already been said. Individual subjects, be they citizens or politicians, are confounded in the extreme by the economic information with which they are confronted. Largely through the media and other coverage of economic affairs, there has evolved an almost universal belief in a conflict between environment and employment and in production growth being able to go hand in hand with environmental conservation - and, indeed, even in the necessity of such growth for conservation, whereas the opposite is true: environmental protection creates jobs and conflicts with production growth (as set out in *Three Myths*, this volume). There is absolutely no justification for proceeding from individual behaviour, because of the existence of what I have termed blockages (*cf.* H&B, p.18). Scarcely, if ever, can subjects form an conception of the *physical* benefits of extremely complex processes, the physical costs (as negative benefits) of disturbance thereof and the technological (im)possibility of intervention in such disturbance. This is also one of the insurmountable problems of contingent valuation (*cf.* Hueting 1989b, 1992a, 1995b). Although on the scientific side our information is better and more consistent, here too there are still major uncertainties. Let me mention the two examples that to me appear the most important: (1) the forced greenhouse effect and (2) the macro-effect of species extinction. The uncertainty regarding the first I presume to be universally familiar. The latter uncertainty is discussed in Hueting (1974, p.145 and 146, English edition), based on the work of Eugene Odum and also personally validated by him. In short, the continuing burden on the environment

may lead to an ecological crisis in which man's continued existence is at stake, but there is no way of indicating at what level of burden such a crisis will occur. In the estimation of SNI according to Hueting, species conservation is nonetheless taken as a sustainability standard. That norm is based on the precautionary principle and on the fact that the rate at which species are becoming extinct today is at least a factor 10,000 higher than the rate at which new species are evolving (*cf.* H&B, p.39, draft).

My conclusion: there is considerable scope for improving and debating information vis-à-vis sustainability, as well as for debating what preferences for sustainability precisely are, before judgement can be made about those preferences. I conclude, and here I differ with Herman, that an assumption about preferences is inevitable as well as legitimate.

Herman posits that estimation of an SNI is a different matter from designing a policy for attaining sustainability in a market economy. But the opposite is true: estimates of SNI are based precisely on simulated internalisation of sustainability costs in market prices (*cf.* H&B and Verbruggen *et al.*, this volume). He also posits that introduction of tradable permits leads to a perpendicular supply curve (and thus to an objective value). However, supply curves for environmental functions are made up of increasing costs per unit reduction of environmental burden, and thus per unit function restored or supplied. They are empirically grounded and have been estimated since the early seventies (*cf.* Hueting 1974a for several examples). Obviously, introduction of tradable permits does not change the characteristic, progressively rising shape of the curves. The sole objective is to achieve a certain level of environmental burden (e.g. emissions) as efficiently as possible. Any standard of environmental burden, sustainable or otherwise, can be represented as a limit and therefore as a perpendicular. Subsequently preferences, and therefore a *demand*, can be assumed which correspond to the (sustainability) standard (*cf.* H&B, p.19, draft, for the ins and outs of verticality). The degree of availability of environmental functions is determined by the standard (and thus by demand). The cost of supply depends on the shape of the specific elimination-cost or supply curve determining the point of intersection with the standard or demand curve. This is shown, *inter alia*, in Figure 7 of H&B for a sustainability standard D.

A second objection is that there is no such thing as a perpendicular supply curve for environmental functions. Herman here appears to be making the same error as Constanza *et al*. (1997). A perpendicular supply curve intersects the abscissa. This means that the function (and its services) can be supplied at zero cost up to the perpendicular, which boils down to the statement that functions (including the vital functions of the world's ecosystems) are free goods, because they are 'supplied' free of charge. This is an inappropriate representation of the opportunity costs associated with provision of a typical environmental function. Consider the graphs below.



Graph A Graph B

The horizontal axis is some index of quantity and quality (that is: availability) of an environmental function, and the vertical axis is money units. In graph A, a certain amount X_o of the function is available as a free good, but the cost of increasing the function's availability beyond this level is infinite. This corresponds to the 'indestructible land' of the classical economists, such as John Locke, Jean Baptiste Say and David Ricardo. But it is certainly *not* the typical situation when it comes to environmental functions, including those of the planet's forests, marine ecosystems, wetlands or arable lands, all of which are clearly *destructible* (more or less irreversibly). Now consider the question of how to value this indestructible asset. Say that it is productive land and that the product (say corn) has a money price. If, after every man has taken as much land as he wants, there is still some to spare, then the resource will have a zero price (free good). If, on the contrary, the amount is less than what people would utilise if it were freely available, the land will command a 'rent' as a factor of production, in accordance with established economic theory. However, this rent does *not* measure the cost of producing land, nor is it an opportunity cost in the sense of production having to be sacrificed to obtain more land. There is no question, in this situation, of loss of functions due to depletion of land, nor of marginal cost of an additional unit of land: its supply is fixed. So 'rent' is a value category that arises through imputation back to land as a factor of production, reflecting the usefulness of the land as a source of income through the market value of the goods produced with that land.

Consider by contrast environmental functions from, say, aquatic of forest ecosystems. These are not indestructible, neither is their quantity fixed. The amount of environmental functions available in any period of time becomes an endogenous variable. Today, the functions of ecosystems are no longer free goods (see e.g. H&B) and their supply curves are typically of the form shown in graph B, as demonstrated by research (see e.g. Hueting 1974a, De Boer 1996). Measures must - and can - be taken in order to restore and safeguard the functions that one wishes to maintain, to the extent that irreversible losses have not yet occurred. These measures form (opportunity) costs. So sacrifices in terms of consumption (or family planning: lower national income, more environment) are evidently unavoidable. The costs of the measures form the basis for constructing a supply curve in the normal economic sense of the concept: the

supply of a desired economic good through human activity involving the sacrifice of an alternative. The necessary sacrifices are obviously not reflected in the perpendicular that Daly and Costanza *et al*. (1997) have in mind. Therefore this curve is not a supply curve in the economic sense and cannot consequently be used for economic valuation of environmental functions and their services: all it tells us is that the cost of any quantity of function exceeding that indicated by the perpendicular would approach infinity.

Ecosystems as we find them, the result of hundreds of millions of years of evolution, cannot be quantitatively extended by human action. But they certainly can be - and in fact are - cut back by human (productive and consumptive) action. At the same time, however, these systems can be maintained and restored by human action. It looks as if what Daly and Costanza *et al.* (1997) have in mind is that irreplaceable goods cannot be supplied. However, it is possible to formulate measures (what has to be done and, above all, from which activities must people refrain?) in order to safeguard (the functions of) irreplaceable ecosystems, and to estimate the (opportunity) costs thereof. This yields an elimination or supply curve: the less risks one wishes to take, the higher the costs (*cf.* Hueting 1998a, 1999d).

4. Richard B. Norgaard, Astrid Scholz and Sarah Fleischer Trainor

Much of what Richard *et al*. bring forward in their contribution I can wholeheartedly endorse. In particular, I sincerely support their recommendation to stimulate opinion-forming on collective goods such as environmental functions in “discursive, democratic processes”, thus to arrive at better grounded preferences. Their paper does, nonetheless, raise several questions and prompts me to make a few remarks.

(1) I cannot comprehend their philippic against utilitarianism. In standard economic theory, utility has a far wider semantic range than in everyday language. Utility derives from anything that can satisfy human wants or - an equivalent statement - can help achieve human ends, to the extent that these depend on our dealings with scarce means. These wants or ends are not for economists to judge. Utility, or welfare, or satisfaction of wants is a psychic category that is not amenable to measurement. Information on the level thereof can be obtained only by introducing (plausible) assumptions. I refer the reader to H&B, Section 3, and my response to Herman Daly. If, for example, a subject deems biodiversity to yield *utility* (satisfaction of wants) in his or her own judgement and conservation thereof involves opportunity costs, in whatever form, then we have a problem of economic choice. In the sense used in standard economic theory, it is entirely irrelevant whether that utility consists in acquiring medicinal drugs and agricultural crops, in the agreeable knowledge that species are being conserved for their ‘intrinsic value’, or ditto that future generations are not existentially threatened by disruption of life-support systems, including ecosystems, leading to loss of vital environmental functions.

Richard *et al*. have distinct views on individual preferences, on the dependence of these on culture and historical juncture, and on ethical criteria and normative judgements vis-à-vis valuation of vital, indispensable environmental functions, which are collective goods. For my own views on these issues I refer the reader to my rejoinder to Herman Daly.

(2) Ultimately, valuation is a question of preferences. Given the existence of scarcity, the bottom line of economics, (individual) preferences necessarily arise out of a process of choice. Which want or need is the more pressing? What alternative is to be sacrificed (opportunity costs)? Because preferences can be identified only very partially (*cf.* H&B, Section 5.2), in environmental or ecological economics there are just two 'leverage points': the description of the utility that environmental functions can provide, and the estimated opportunity costs associated with restoration and maintenance thereof, in the form of estimated cost-effectiveness: what reduction in environmental load and thus what increase in function restoration and maintenance is obtained at a certain cost level (*cf*. Hueting 1974a, p.185 English edition)? The latter is a *conditio sine qua non*, for without opportunity costs there is no *economic* value (free goods). I do not therefore understand why even the word 'costs' is absent in Richard *et al*.'s contribution. When I asked Astrid and Sarah about this after their presentation at the Amsterdam symposium, they responded that they were unaware that such calculations could be made. But such cost-effectiveness estimates have been undertaken since the early 1970s (for some examples, see Hueting 1974a). In all friendship, I can only conclude that the valuation processes advocated by Richard *et al*. remain counterproductive in the absence of such essential information as cost-effectiveness data, micro or macro (impact on national income). At the risk of pedantry, let me add one further remark. Contrary to what is commonly held, with opportunity costs money is not, in essence, the point at issue. Although cycling may indeed be cheaper than driving a car, for example, a shift towards cycling in the interests of sustainability does require a sacrifice and will have a negative effect on national income *volume* (*cf.* H&B, p.31 draft, my response to Paul Ekins and *Three Myths*, present volume).

(3) Contrary to Richard *et al*. I hold that objective, scientific descriptions of a given state of the environment and of sustainability standards are to be strictly distinguished from subjective preferences for such a state, for sustainability, say (*cf.* Hueting 1998b). If knowledge is lacking, the precautionary principle should be applied and research encouraged. If Richard *et al*. merely mean that availability of additional information influences preferences, then I am in full agreement with them.

(4) On p.16 (draft) the authors hold that I deem it immoral to destroy the vehicles of vital environmental functions, thus depriving future generations of use of those functions. No reference is given, and logically so, for I have never passed judgement on the morality of others. Why I would never consider doing such follows from my response to Herman Daly and, in part, from the above rejoinder.

5. Wilfred Beckerman

According to Wilfred Beckerman, environmental considerations provide no grounds whatsoever for concern about the physical conditions under which future generations will have to live. In his opinion, a Sustainable National Income (SNI) study designed to estimate the distance between

actual and sustainable levels of production and consumption is therefore entirely superfluous. The value of such a study rests, moreover, so Beckerman holds, on the erroneous notion that rights can be conferred upon future generations. Any undertaking on behalf of such generations can at best be based on “imperfect obligations” borne of moral considerations.

Let me start with the second point. Conferring rights has nothing whatsoever to do with the SNI study, nor with any other topic on which I have ever published. The SNI according to Hueting is not based on the rights of future generations, nor on intergenerational equity, but on the *preferences* of the present generation for handing down the vital functions of our physical surroundings (the environment) intact to generations to come. There are two grounds for assuming such preferences. First, the existence of ‘blockages’ preventing these preferences from being expressed (*cf.* H&B, p.18, draft). Second, the postulate that “man derives part of the meaning of existence from the company of others. These others include in any case his children and grandchildren. The prospect of a safer future is therefore a normal human need, and dimming of this prospect has a negative effect on welfare.” (Hueting, 1987d).

Regardless of these considerations, though, why should one not speak of the rights of future generations? Unlike Wilfred, I am a complete layman in the realm of philosophy (of law). Nonetheless, I do not see how his reasoning can stand up to scrutiny. Wilfred is obviously right in stating that if something no longer exists (an animal species, for example) there is no sense in speaking of rights and obligations. But why should we not be able to confer upon future generations the right to dispose over vital environmental functions that *do* still exist, among them the functions of life support systems, of which natural ecosystems (biodiversity) form an important part? If we assign such rights, surely these would be balanced by our obligation not to destroy those systems? Then surely Beckerman’s logical condition is satisfied that rights should always have their counterpart in obligations? Surely the indeed unsatisfiable condition that future generations be able to exercise or waive such rights is not a *conditio sine qua non*?

I put these questions to C.W. Maris, specialised in philosophy of law, and W. Achterberg, specialised in environmental, moral and political philosophy, both at the University of Amsterdam. Their answer: “yes, indeed”. They had more to say, though.

One’s answer depends on how ‘rights’ are defined. It is, in short, a question of semantics. If rights are defined such that the bearer thereof is to be able to choose whether or not to exercise them - if, in other words, they are defined in terms of the protection of individual autonomy - then there is an identity problem. ‘Future persons’ cannot, after all, be individually identified. No contract can therefore be concluded with them entailing specific rights and obligations. Beckerman’s discourse proceeds from this definition of rights. However, if rights are defined in terms of the protection of interests, there is no problem of identity. Interests are often diffuse and not specifically attached to discrete persons but to groups of persons, such as future generations, for example. In the latter case we can proceed from the reasonable assumption that there will be people in the future and that these ‘future persons’ will have an interest in being able to dispose over those environmental functions that are indispensable for human life (see, for example, Feinberg, 1974). It is, therefore, surely possible to confer rights of the disposal over

those vital functions upon future generations, balancing them with an obligation to hand down these functions undamaged.

Beckerman argues the first point - that there is no need for concern about future generations - solely by extrapolating observed trends vis-à-vis technological progress. Two points of criticism can be raised here. (1) His extrapolation is only partial. (2) It is unduly simple.

(1) On the first point, Beckerman asserts (p.1, draft) that my exclusive focus is on stocks of natural resources and in his contesting the need for concern he refers solely to stocks of non-renewables, albeit that renewable energy sources are mentioned in a discussion of fossil fuels. Non-renewables formed in slow geological processes are clearly important natural resources. Wilfred's treatment of these resources is inconsistent and takes inadequate account of the many different functions of the resources. Thus he argues that "we will never run out of any resource or even suffer seriously from any sudden reduction in its supply" (p.11, draft). Further on, though, he approvingly quotes authors who hold that fossil carbon resources will run out in several hundred or thousand years (p.13, draft).

He asserts, furthermore, that "if, for example, coal were ever to become a very scarce commodity its price would rise to the point where, like other minerals, such as diamonds, it would be used only for jewellery or certain very special industrial uses" (p.11, draft). In that case, however, its function as a major source of energy would clearly be lost. Because natural resources formed in geological time should for practical purposes be considered finite and because prices may be expected to soar as resources peter out, ongoing dissipation of these resources will lead ultimately to loss of most, if not all their current functions. With ongoing dissipation, the question is not whether functions will be lost, but when.

Beckerman moreover ignores the fact that burning all fossil carbon stocks would lead to dramatic climatic changes (Graedel and Crutzen, 1993).

Non-renewables are not the only resources on which mankind depends, however. Contrary to Wilfred's assertion, in my work I have always stressed the importance of the life support systems of our planet (for a brief description: H&B, note 2, p.51, draft). These constitute a key element in the estimation of sustainable national income. Life support systems are not stocks but *processes.* These processes are irreplaceable and non-substitutable; and they are threatened by the ongoing growth of production and consumption by ever more people. Their functioning depends on conditions far up into the atmosphere and probably even beyond. Their equilibrium is vulnerable, extremely complex and as yet only very imperfectly understood. There are already instances in which meddling with those processes has had major deleterious consequences. Cases in point include the recent collapse of a number of fish populations in the North Atlantic and several other important fishing grounds (Tolba and El-Kholy, 1992; FAO, 1990-1998) and, in the more distant past, the collapse of agriculture and a highly evolved culture on Easter Island (due to erosion and the clearcutting of forests) and in what is now Southern Iraq (due to salinisation) (Reijnders, 1996).

Natural ecosystems are part of the life support systems on which mankind depends. The threat to such ecosystems goes far beyond the extinction of Beckerman's Dodo (Odum, 1971;

Goodland, 1995; Lovelock, 1979). Species are currently being lost at a rate exceeding new species formation by several orders of magnitude (*cf*. H&B, p.37, draft, and Hueting, 1998b) and the area of land covered by natural ecosystems is dwindling rapidly. This is fast reducing the availability of resources for plant breeding and pharmaceuticals production and altering the composition of the atmosphere (Lovelock, 1979; Graedel and Crutzen, 1993; Goosland, 1995; Tolba and El-Kholy, 1992).

These processes, absolutely vital for future generations, have no part in Wilfred's extrapolations. They are not even mentioned, in fact.

(2) My second point of criticism is that Beckerman merely extrapolates historical trends. Attempts to shed light on future economic developments are unfortunately not that straightforward, however. I might well do the same as Wilfred. Consider the following: In estimating the distance between the standard world income and its sustainable counterpart Tinbergen and Hueting arrive at a rough figure of 50% of current world income (*cf.* Hueting, 1991d). The provisional results of a modelling exercise show that this estimate is by no means extreme (Verbruggen *et al.*, this volume). This study also demonstrates that the present 'sustainability gap' cannot be bridged with the technology currently available, implying an additional need for changes in consumption patterns. Not that long ago, on the relevant geological time scale, this gap was virtually non-existent, for environmental functions were then free goods (*cf.* H&B., p.8, 12 and 13, draft.). Despite gigantic technological strides being made, then, with regard to those functions of our physical surroundings that permit our very survival there is an empirical trend of growing scarcity. Entirely in line with Wilfred's method, I might then extrapolate this trend and conclude: if we continue down the present path, mankind as a species is doomed to extinction, regardless of technological progress. Obviously more information can be gained from tracing the development of the gap between SNI and actual national income than from extending lines.

I share Wilfred's optimism about technological progress, although not *in extremo,* particularly when it comes to renewable energy resources to substitute for fossil fuels. However, I would point out a number of errors and omissions that render his extrapolations unduly simple.

(a) In Beckerman's view, scientific and technological progress are the fundamental driving force determining production growth (p.8, draft). I agree. However, intellect and our physical surroundings are entirely complementary. All produced goods, including human-made capital goods, are a combination of intellect and elements of our physical surroundings. Without the latter, we humans are left empty-handed (*cf.* H&B, p.8, draft and 'Three Myths', this volume). The issue is therefore whether the functions of our physical environment remain intact. A number of elements of our environment are irreplaceable and non-substitutable. These Beckerman ignores, as I have already noted. Exhaustible resources must be substitutable (in time) by other elements of our physical surroundings that can provide the same services. Wilfred's assertion that this will remain possible *ad infinitum*, regardless of the levels to which production and consumption soar, is pure speculation and thus at odds with the wisdom of the precautionary principle. He argues this position with almost exclusive reference to energy. But what about the resources required for food production and the world's finite stocks of

freshwater? There is no agreement about the adequacy of natural resources to feed an ever-growing world population, particularly given the finite nature of freshwater resources and the ongoing deterioration of land suitable for agriculture due to soil compaction, erosion, salinisation and other processes (Tolba and El-Kholi, 1992; Kendall and Pimentel, 1994). For Wilfred, however, all this is absolutely no cause for concern, for his extrapolations assume ever-rising per capita income, *ad infinitum*.

(b) On p.11 (draft) Beckerman asserts that the price mechanism will ensure there are no disastrous shocks. There have been serious famines in the recent past, however, in some cases compounded by shortages of drinking water, and new famines are anticipated in the near future (Tolba and El-Kholi, 1992; Kendall and Pimentel, 1994).

(c) Beckerman's extrapolations are unduly simple, for he overlooks at least six factors of relevance. First, he makes no allowance for the fact that any price rise in real terms means a decline in the volume of national income and therefore a check on production growth (*cf.* H&B, p.12-13, draft). *Second, Wilfred overlooks the fact that, for a given technology, product costs will rise progressively as the yield (effect) of environmental measures is increased. Technological progress leads to higher yields, of course. As production increases further, however, so too must the yield of the measures in order to maintain the same state of the environment, while the fact of progressively rising costs with rising yield remains unaltered. There is thus a 'race' between environmental technology and production growth, the outcome of which cannot be predicted.* Third, Wilfred neglects the fact that the vast bulk of production growth is generated by the approximately 30% of industries that cause most pollution, utilise the greatest amount of land and deplete natural resources most rapidly (*cf.* Hueting, 1981b and 1992b, my response to Ekins and H&B, p.31, draft). A shift towards environmentally more benign activities therefore implies a substantial check on production growth. Such a shift is already unavoidable if sustainability is to be achieved; see the first paragraph of (2) above. Fourth, Wilfred neglects to consider the impact of increasing land use on, *inter alia,* species extinction, because he ignores the importance of life support systems for human survival.

Fifth, Wilfred does not take into account that in a long historical time series energy consumption (and CO_2 emissions) is found to run parallel to production and that there is negligible substitution by renewable energy sources, *i.e.* energy derived from the sun (*cf.* Hueting, 1991d). This trend he does *not* extrapolate. Although In the recent past there have been brief periods in which production growth, as measured in national income, was not accompanied by rising energy use, overall in the last fifty years this has been the case (Adriaanse *et al.*, 1997; De Bruyn *et al.*, 1998; Schipper & Meyers, 1992). As a rule, moreover, renewable energy is currently much more expensive than energy generated using fossil fuels. In the case of photovoltaic power, the price may even be far higher than electricity from a coal-fired plant (Johansson *et al*., 1993). And although I share Wilfred's conviction that the cost of renewable energy will fall substantially in the future, implementation costs will do so far less. The latter will be extremely high, in my estimate, which will check production growth. For this check to be removed and continued production growth achieved, the renewables will have to become

cheaper than current fossil energy. Whether this will ever be the case we cannot predict. The same holds for substitutes for any other resource.

Sixth, Wilfred makes no allowance for the fact that, for there to be growth of the total mass of production, the productivity of the approximately 30% most environmentally burdening sectors that generate most of that growth would have to rise by far more than the overall average. Consequently, the yield of environmental measures in these sectors would likewise have to increase by far more than the average across the national income in order to maintain the same state of the environment - possibly a factor two or three more, with progressively rising elimination costs and a severe check on production growth as a result (see above). A correct estimate of this factor requires more than a back of the envelope calculation, however (*cf.* my response to Ekins).

If due allowance is made for all these factors (and their interrelationships), the growth and other figures predicted by Wilfred may well prove to be a factor ten to twenty too high. But that requires a far more extensive analysis than Wilfred has presented and, as I have said, I have major doubts about the usefulness of such prognostications.

6. Salah El Serafy

Salah El Sarafy shares my concern that the universally accepted compass of economic policy - changes in national income - is giving us the wrong signals about economic success. Despite irrefutable, or as yet unrefuted, information on the defectiveness of this compass, as detailed by numerous authors for several decades now, there is not one country in the world that has ever published in its official statistics a national income figure amended for natural resource depletion and damage to irreplaceable ecosystems. Economic activity thus continues to be steered by the wrong compass despite steps in the right direction, such as putting physical environmental data into satellite accounts. When it comes to our understanding of economics, too, Salah and I are in basic agreement. With reference to Robbins, Salah states quite rightly that economics is a positivist (descriptive) rather than normative theory. Salah's conjecture, in Section 5, that my introducing competing environmental functions as scarce, and therefore economic, goods is based on Robbins' work is thus correct. I received my training under Hennipman, whose masterful oeuvre builds on that of Rosenstein and Robbins; see H&B, Section 3, and elsewhere. Finally, Salah states, as I do in the introduction to these Rejoinders, that ecological economists are often unduly dismissive of what they take to be neo-classical economics, and have unnecessarily limited influence as a result.

In Section 6 of his paper Salah says he will try and highlight our differences in views, as one reviewer had asked. It is an endeavour in which he has scarcely succeeded, I feel, and given my last paragraph it could hardly be otherwise. To my mind, Salah and I are in agreement on the following essential points.

(1) Salah considers my chief concern to be with welfare, giving my work a broader perspective than his own, which he regards as being concerned solely with correct accounting practice. This cannot be the case, for several reasons. From the writings of Rosenstein-Rodan, Robbins and Hennipman and others it follows directly that all economic activity is directed

towards the satisfaction of wants and therefore, by definition, towards maximising welfare (H&B, Section 3). Indeed, what other purpose could it possibly have? The opposite is nonsensical. Data that tell us nothing about welfare, whether directly or indirectly, are therefore devoid of meaning as economic information. Only if welfare increases can there be economic success. Time series of the volume of national income were intended by Tinbergen, Kuznets and the other theoreticians of the SNA to serve as just one indicator of how well a society is succeeding in satisfying its wants, *i.e.* achieving welfare, and specifically the indicator of the influence on welfare of scarce goods, under a number of assumptions (Hueting, 1991d; H&B, Section 3). And this is how these time series are still conceived, positive changes prompting 'flags out' on government buildings and at newspaper offices, and negative changes mourning costumes.

This was perfectly justified as long as all scarce goods were being registered in the national accounts, but became untenable with the widespread emergence of a new category of fundamental, scarce goods - environmental functions. This view was shared by a growing number of authors, including Tinbergen (*cf.* Hueting 1991d) and almost certainly Kuznets (*cf.* my rejoinder to Daly). Others, meanwhile, see no reason why the flag-waving and mourning should not continue unabated, the combination of budget and market mechanisms in a democracy being deemed as firm a guarantee as ever of a package of goods - produced goods *and* environmental functions - in perfect conformity with the preferences of the economic subjects (individuals and institutions).

In my work I have argued extensively (in H&B and elsewhere) that this is an *assumption*, an assumption implying that the economy is on an optimum path. That is a legitimate assumption. But no more so than the assumption that there exist overriding preferences for sustainability that are prevented from being expressed by the existence of blockages, implying that the (far) lower-level path on which the SNI lies is optimum (*cf.* H&B, Section 5.2). If they are to be providers of impartial economic information, statistical offices therefore have a scientific and moral obligation to publish several national income figures based on different assumptions concerning preferences. Salah can only belong to the former group of authors (preferences not properly reflected, current path sub-optimal). Being familiar with his work and his interventions in discussions, neither can I imagine that he regards national income as a mere registration of transactions unrelated to the satisfaction of human needs (welfare), including those of populations of developing countries. Salah's assumptions may be a little more cautious than mine, but the conclusion can never be that my work is broader in perspective.

(2) Following Hicks, Salah holds that correct accounting practice involves writing off both human-made capital and non-renewable and renewable resources such as forests at market prices in the current accounting year. (National) income is what remains after depreciation. However, as argued more extensively in my response to Daly, this entails an assumption about preferences which in Hicks' day scarcely merited that name, but in the year 2000 is entirely robust, *viz.* that subjects are prepared to sacrifice a substantial portion of their consumption in order to preserve (the functions of) natural resources. My conclusion once more: Salah and I merely entertain different assumptions.

(3) Salah writes that I am seeking consensus on the 'right' approach (p.9, draft). That is not entirely true, however. What I do see as a minimum requirement is that an explanation be provided for the enormous differences in the results of the "bewildering variety of approaches and estimates", as Salah puts it on p.8 (draft), and I feel that this should be the concern of us all (*cf.* my Introduction, above, and Hueting, 2000a, quoted by Salah. For most authors, including Salah, I feel I have pinpointed the cause of the problems: differences in assumptions that often remain implicit - assumptions about preferences, for example, and about whether or not the economy is on an optimum path. Here, too, we are undoubtedly in agreement.

On the following points, to my mind less crucial, I do see a difference of view.

(i) To my mind no distinction can be made between weak and strong sustainability (Hueting, 1998b; H&B, Section 7). When it comes to environmental sustainability, only one thing matters: preservation of vital environmental functions (i.e. possible uses of the environment). These functions depend on the state of our physical surroundings (including physical processes), the result of a vast historical process going back many thousands or hundreds of thousands of years. By the same measure, environmental sustainability must also be a very long-term issue. It follows that the only behaviour to be deemed sustainable is that guaranteeing long-term preservation of vital environmental functions. That is why I hold it to be simply unsustainable to expend the funds set aside under 'resource depreciation' on investments in human-made capital (p.4, draft), unless the explicit aim is to generate substitutes, which is sometimes unfeasible and sometimes essential for attaining sustainability (Hueting, 1998b; H&B, Section 7). Although it may be advantageous for a particular country to invest these funds in the consumer goods industry, if every country does so it will be of no advantage at all. After all, the chief environmental problems - resource depletion, loss of biodiversity, global warming and so on - are global issues. I would like to add that in 'the SNI according to Hueting' the share of a given country in elimination costs is taken proportional to that country's contribution to the exhaustion of renewable and non-renewable resources, thus leaving income ratios unaffected; national contributions to resource depletion are reckoned on the basis of national *consumption* (*cf*. Hueting, 1992d, and elsewhere).

(ii) There is absolutely nothing to guarantee that the depreciations-at-market-prices (sums set aside), mentioned in point 2, above, are equal to the sums required to preserve intact the functions of the resources in question - the crucial condition of 'sustainability'. The same applies to the interest rate. Salah's Hicksian income depends on the choice of interest rate. How does one know that the interest figures used are adequate for the measures required to meet sustainability standards?

(iii) Salah says his sole intention is to improve on the registration methods currently being used by national accountants. The System of National Accounts (SNA) records only *transactions*, however, as per international agreement. The corrections that Salah (and many others with him, including this author) would like to see implemented are not based on transactions and - contrary to what he himself suggests - Salah's work is not therefore in accordance with the SNA guidelines. Once more, we are up against underlying assumptions. In

the SNA it is assumed - *implicitly* - that the economy is on an optimum path. There would already be major gains in terms of impartiality of economic information if this assumption were rendered explicit. This would automatically mean having to publish more than one national income figure (*cf.* point 1, above).

(iv) Salah states (p.10, draft) that adjusting the national income in deference to strong sustainability "would lead to meaningless estimates". This he illustrates with reference to the hypothetical example of a country whose national income consists entirely of the sales revenues of a particular resource extracted at zero cost. According to Salah the required correction then equals the market price of the quantity of the resource extracted and sold in the current accounting period. This is then equal to the national income, which therefore works out at zero. Several comments are in order.

Salah's example is not an illustration but an impossibility. If there is no human-made capital, as Salah assumes, the resource will have to be exploited using bare hands. This labour is value added to the resource. In the SNI it holds that the sum total of values added (to our physical surroundings; see H&B, Section 4) equals the sum total of revenues equals the sum total of costs. In Salah's example the costs are zero, so there is no value added and consequently no labour. Without labour, *i.e.* without added value, it is impossible to extract the quantity of the resource in question. The resource then remains intact and cannot be marketed and no correction for depletion can be made. National income (the sum of values added) can therefore never consist solely of rent, as Salah implicitly seems to assume.

Even if we elaborate Salah's example into something physically conceivable, *viz.* bare-handed extraction, his criticism is unjustified. Disregarding any rent, the market value now consists of the value added in manual extraction (equalling the labour costs). In Salah's approach, this value would have to be corrected by the same figure, *viz.* depletion costed at market value (equalling the labour costs). National income once again tallies to zero.

However, the point is that the value added to the resource by labour in the process of extraction (and processing) is precisely *not* the value of the resource's functions (*cf.* Hueting 1998a and H&B, p.8 and 9, draft). The value of environmental functions and the costs associated with their loss are expressed only imperfectly, if at all, via the market mechanism - and then never as added value. Neither does the value added determine the value of the resource itself, for the latter derives from the value of the functions, or flows, of the resource (the stock), in the same way that human-made capital goods are valued with reference to the value of the consumer goods (the flows) they generate. 'Sustainability' presumes preferences for the future availability of functions (flows) of the resource. In the final count, such can only be guaranteed by replacing the resource by other elements of our physical surroundings (substitution) or, if that is not feasible, by eliminating the environmental burden at source. In either case the associated costs will show up on the elimination cost curve, which simultaneously determines the value, *viz.* at the intersection with the demand curve (*cf.* H&B, Section 5.1). These costs have nothing to do with the revenues Salah considers in his example.

Perhaps unnecessarily, I would recall that growth of national income involves a change of *volume*, not of added value (*cf.* Hueting, 1992d and H&B, p.12, 13 and 14, draft).

My overall conclusion: Salah and I are essentially in agreement. We merely differ in the assumptions we make, with mine going one step further.

7. David Pearce, Kirk Hamilton and Giles Atkinson

As already noted in Section 1 of these rejoinders, David Pearce et al. are in agreement with me on more issues than their contribution suggests. As was the case with Herman Daly, their paper was completed after correspondence in which I clearly stated my position on (individual) preferences. Both Daly and Pearce et al. have ignored this information and in doing so they consciously misrepresent my views on preferences. In Herman's case, I strongly suspect that he was keen to provoke a discussion on crucial issues (cf. Section 3). Although David et al. undoubtedly also had a reason, I cannot surmise what it might be. Unfortunately, then, I see myself obliged merely to quote, once again, from my work to demonstrate that we are in fact in agreement with regard to basing environment valuation on individual preferences. Subsequently, I shall summarise the points on which we do differ in our understanding, endeavouring to place these in an appropriate perspective; for the good reasons set out in Section 1, I am keener to seek concordance than discord.

David *et al*. write: "(…) the approach assumes that sustainability targets can be established in a fairly robust manner. Roefie's view has been that governments represent a channel of 'revealed preference' so that targets set by government act like shadow prices (…). Those costs must be based on individuals' preferences. (…) Whatever a government decides is a target is the socially desirable and optimal target. (…) The reasoning is entirely circular. (…) We therefore find the 'distance to targets' approach suspect."

It is painful to be criticised for advocating a method of (environmental) valuation that I have for 35 years attacked so vociferously in countless publications. In his contribution to the present volume Robert Goodland quotes as follows from one: "He criticized the official CBA of the construction of a polder in (…) an internationally important estuary. (…) Hueting's main objection was that the official CBA-assigned value of one hectare Waddensea estuary was set at the same value as one hectare marginal agricultural land (…) That was the value officially set for nature areas by the Netherlands Government. In his review of the official CBA, Hueting observed: 'This is not valuing at all, because it gives the Government the value which the same Government had already decided upon.' (…)" The quote is from 1978 and voices precisely the criticism formulated by David *et al*.: if the Government perfectly reflected subjects' individual preferences, *quod non*, valuation would be superfluous; economists providing such 'valuation' results make an easy living; it is circular reasoning.

Only recently, in *Economisch-Statistische Berichten*, 85, no. 4244, I published an article entitled 'Price the environment, not the policy' in which I criticise derivation of 'shadow prices' for environmental functions from government environmental standards (which had been done in the framework of a transport CBA). After all, in setting those standards the government is implicitly setting the price of the functions in question (the article is being translated and is available on request).

The logic of Pearce *et al.*'s critique also rather escapes me, since most of the environmental standards set by governments around the world are very far removed from 'sustainability standards'. The standards employed in estimating Sustainable National Income (SNI) can therefore never be derived from government targets, as David *et al*. state. In my *Three Myths* article (this volume), under Myth 1: 'Environment conflicts with employment', I write: "The environmental measures taken to date are marginal in relation to what has to be done to arrive at a sustainable use of the environment. Most measures only slow down the rate of deterioration, owing to the persistent character of the burden. No government accepts the unavoidable truth that, given the available technology, more environment means less production (and vice versa); so nowhere is reducing the wage rate taken into consideration." Obviously, governments may voice their intentions. The voicing of such intentions vis-à-vis sustainability worldwide certainly implies that our SNI research enjoys a measure of support in society. At Statistics Netherlands, this is a *conditio sine qua non* for setting up research, and rightly so. However, expressing an intention to act (and a willingness to pay for such action) is of course not the same as actually doing something. Intentions are obviously not revealed preferences.

In Hueting (1994, p.45) I write: "(…) the *assumption* is made that the standards reflect the intensity of individual preferences for environmental functions" and (p.46): "as stated, the calculation is based on the *assumption* that the perpendicular on point D reflects the intensity of individual preferences for environmental functions". (The italics, like those below, are original. I might add that David was one of the editors of the volume in question.) Hueting (1995a, p.209) puts is thus: "(…) any monetary valuation of environmental functions and their loss is based on an assumption about demand. (…) assuming that individual economic subjects have a desire to use the environment (including natural resources) sustainably" and (p.226): "A demand curve (…) on the basis of (…) assumptions (…) that the standard for sustainable use of the environment is a good measure of individual preferences for environmental functions". Hueting (1995c, p.4) reads: "The calculation is therefore based on the *assumption* that the perpendicular in Figure 1 reflects the intensity of individual preferences for environmental functions". Likewise, Figure 1 of Hueting (1995b) is introduced with the words: "This comes to making the *assumption* that individuals want to use environmental functions in a sustainable way." On p.32 of Hueting (1998a), finally, I write: "Consequently, for finding shadow prices for functions, it is necessary to make assumptions about (individual and social) preferences, in CBA's and green accounting."

In my rejoinder to Daly (this volume) I have noted my reasons for proceeding from individual preferences. There, I also note that even in a country with high standards of democracy such as The Netherlands, a lack of information can result in fatal decisions being made. Without economic information on the very crux of the environmental problem - the conflict between the environment and production growth as measured in national income (population times volume of economic activity) - well-founded decisions on environmental conservation cannot be made. Besides opportunity cost data, data on (individual) preferences are indispensable for such information. If one takes as established fact that the latter data can be deduced only very partially from market behaviour or surveys, then making assumptions vis-à-vis preferences is inevitable (e.g. H&B, Section 5.2).

Government decisions do not reflect individual preferences in any *a priori* sense. On this we are in agreement. Where my difference with David, Kirk and Giles lies is the inescapability of introducing assumptions on preferences as well as on several other points.

(1) David *et al*. believe that, ultimately, WTP will prove an adequate tool for measuring preferences, including preferences for the functions of life support systems of existential importance to future generations. It is merely a question of time, they tell us. In Hueting (1989b, 1992a, 1995b) I have argued why such a quest is very unlikely to succeed. The arguments in these publications overlap one another only partly and are by no means exhaustive.

(2) According to David *et al*., estimation of an SNI is an impossible undertaking. In defence of this position they offer the following arguments:

(a) In their view, the sustainability standards used in the SNI research are based on government targets or standards, so that the SNI estimate is based on circular reasoning. As already stated, this is a misrepresentation of my work. The sustainability standards used in the SNI are based on independent scientific research (see, for example, Hueting 1992b and 1998b, and H&B, Sections 6.6 and 7).

(b) The point of estimating the SNI is to attach a value to major changes in the availability of environmental functions, while Pearce *at al*. maintain that minor changes are all that can be valued. Under the assumption of preferences for sustainability, the marginal costs of environmental measures rise exponentially and it is to be questioned, they hold, whether these costs are worth the marginal benefits obtained. These doubts are obviously legitimate.

Given preferences for sustainability, however, the benefits consist of the positive effect on welfare of avoiding existential risks to future generations; the costs consist of the negative impact on welfare of (1) abandoning goods that place a major burden on the environment in both the production and consumption phase in favour of environmentally benign alternatives and (2) moving towards smaller families (this may in fact have a positive effect on welfare, especially for women in developing countries). The environmental problem is, after all, an allocation problem. With opportunity costs we are not essentially concerned with money (which, of itself, has no meaning), but with a change in consumption pattern. Whether the re-allocation required to safeguard the environment is effectuated by internalising the costs of elimination measures or by switching directly to ecologically superior products (less meat and more beans, for example) is evidently irrelevant. Once one realises that this is what is at stake, doubts as to whether the benefits of consuming (ever more) environmentally damaging goods are worth the sacrifices embodied in real and existential risks for the future are equally legitimate (bearing in mind that benefits can be formulated as a reduction of costs and opportunity costs as a sacrifice of benefits). Doubts - either way - are inherent in assumptions. Let me refer once more to *Three Myths* (this volume) and in particular Myth 3: Environmental conservation is not too expensive, for a bicycle is cheaper than a car and raising two children is cheaper than raising eight; naturally, there *is* a sacrifice involved and a negative effect on the volume of national income (*cf*. H&B, p.30-31, draft).

As David *et al*. have well understood, I still stand by the conclusion of Hueting, 1974: shadow prices cannot be constructed on the basis of revealed preferences. However, it has apparently escaped them that in my work post-1985, to be able to provide information on the conflict between production growth and environmental preservation I have introduced assumptions regarding preferences and have since then worked with assumed shadow prices. H&B, Section 5.2, explains that the optimum described in Section 5.1 (which rests on the assumption of fully revealed preferences) remains valid as an assumed optimum (and then rests on a different assumption about preferences); *cf*. Figures 1 and 7. So I do not see how I can possibly be "detract[ing] attention from the theoretically correct (…) solution of estimating a shadow price based GNP", as David *et al.* write.

The aim of our SNI study is to obtain the best possible information, however crude, on the distance between the current growth path (which is optimum under the assumption that the current package of goods, including environmental functions, is a perfect reflection of preferences) and the sustainable growth path (which is optimum under the assumption of preferences for sustainability); *cf*. Figure 3 and accompanying text in H&B. This is not the same as estimating the 'total' value of the environment; in their text David *et al.* wrongly suggest that I am engaged in such a pointless exercise. The distance to sustainability appears to be large, so our work seems to indicate. A decade ago Tinbergen and Hueting gave a rough estimate of about 50% of world income (Hueting 1991d), by no means an extreme figure given the provisional results of our SNI study (Verbruggen *et al*., this volume). One can only conclude that *if* we do not want to jeopardise future generations, we must stop identifying changes in standard national income with economic growth and must indeed abandon the term. What we should then present to politicians and the general public are changes along the sustainable growth path (*cf*. H&B, Figure 3) as a partial indicator of economic success (i.e. an increase in welfare). This would encourage the move from the current growth path, via the transitional path (Figure 3), to the sustainable growth path. (I would remind readers of the subtitle of my 1974 work: 'More welfare through less production?'.) This implies a fundamental shift in economic priorities from production growth to environmental preservation.

I wrote: *if* we do not want to jeopardise the future, we should adopt a certain course of action. However, we can immediately start providing information by *assuming* that we do not wish to roll off serious risks onto future generations. I sincerely hope that David *et al*. see the point of such an exercise and are willing to cooperate on its realisation.

(3) I agree with Pearce *et al*. that the genuine savings approach can provide some kind of (weak) signal vis-à-vis sustainability, but only under additional conditions. The two approaches (SNI and genuine savings) can then complement one another, but only under the six conditions stipulated below, which are not presently satisfied.

As Pearce *et al*. rightly assert, welfare depends on total stocks of produced, natural and human assets. Produced capital, however, is a combination of labour (technology) and elements from our physical surroundings (the environment). In the final count, we are dependent upon but two factors: human and environmental assets (see *Three Myths*, this volume). The *sine qua non*

of *environmentally* sustainable development is a production level that guarantees preservation of vital environmental functions with the available technology (*cf.* H&B). From this there follow three conditions for calculation of the 'genuine savings' indicator.

(i) Any increases in human assets must be used exclusively for environmental protection and/or for growth of production that does not (further) damage the environment. This condition is hard to satisfy, because (a) being intermediate deliveries, expenditures on environmental protection check production growth (see, *inter alia*, Hueting 1974 and H&B) and (b) it is precisely the most environmentally damaging sectors of the economy that account for the bulk of production growth. On the latter point see Hueting (1981b and 1992d), paying due heed, in implementing this condition, to the essential difference, explained there, between an increase in the size of a sector (expansion) in terms of added value and that sector's contribution to an increase in production volume, as measured in national income; *cf.* Hueting (1974, p.170, footnote 2, English edition).

(ii) Likewise, increases in stocks of produced assets must be exclusively for the purpose of environmental protection or 'clean' growth. Again, it is a condition that is not easy to satisfy, for the reasons just given under i(a) and i(b). According to David *et al*., investments in infrastructure contribute positively to genuine savings. From the perspective of sustainability, however, their contribution is surely negative. The fragmentation of the landscape caused by roads and other infrastructure and the consequent loss of habitat and isolation of gene pools are substantially accelerating the rate at which plant and animal species are becoming extinct. Bringing this process to a halt is deemed by many people to be a major first step down the road to sustainability (*cf*. H&B). Certainly in the industrialised countries, infrastructure should be demolished rather than constructed if the goal of sustainability is to be realised.

(iii) Resource revenues must be invested in environmental protection or 'clean' growth; see i and ii.

The C in the genuine savings formula is most probably taken from standard national income statistics. So C contains most probably some asymmetric entries (see, *inter alia*, Hueting 1974 and H&B). This leads to the next condition.

(iv) From C in this formula must be deducted expenditures on elimination of and compensation for loss of environmental functions, to the extent that these are financed by government or private households, as the theory presented by the authors suggests.

Further:

(v) The condition $S_g \geq 0$ must hold for all t to warrant (weak) sustainability, i.e. for a long time series, not just for a single year or single accounting period, as in the formula presented by Pearce *et al*..

(vi) Only in the case of non-renewable resources may technology be substituted for nature, as argued in Hueting (1998b) and H&B, Section 7.

As long as these six conditions remain unsatisfied, the genuine savings method certainly cannot serve as an indicator for environmentally sustainable development.

8. Sylvie Faucheux and Martin O'Connor

As Sylvie and Martin's paper is so akin to the opening contribution of Bart de Boer and myself in this volume, the present rejoinder has been written together with my co-author.

Hueting (1974a) proceeds from the principle of possible uses, or functions, of our physical surroundings (the environment, *cf.* H&B, p.8 and 11, draft). From this point of departure no distinction can be made between weak and strong sustainability: in order to attain sustainability some elements of the environment cannot be substituted, while other elements have to be (*cf.* Hueting 1998b, my response to Beckerman, p.32, draft, and H&B, p.39/40, draft, and Section 1, Consequences of the principles, 2). We certainly appreciate the breadth of scope and didactic value of Faucheux and O'Connor's Section 3. In our opinion, however, by adopting the above starting point the conclusions of the 'weak theory' (general growth theory) can be readily refuted without conflicting with that theory, allowing the conclusions of their Section 4 to be drawn immediately. We consider ourselves in full agreement with most of these conclusions, while on several others we differ for reasons set out briefly below. We hope that our response will encourage further discussion and publication.

In my introduction to the 'Rejoinders to symposium authors' I have argued strongly for as much as agreement as possible, because of the pivotal importance of our subject for society and the negligible influence of ecological or environmental economics on the economics of environmental politics in the absence of such agreement. In H&B (this volume), which represents an attempt to attain such agreement, we conclude that the theory behind the SNI (a term I introduced, I may add) is in accordance with 'general growth theory'. The substantial differences between our results and those of exponents of this theory can be traced back to differences regarding assumptions, one of which is that concerning substitutability; *cf.* Conclusion 2, H&B. This circumstance has the advantage that it allows the information flow to citizens and policy-makers to be rendered vastly simpler as well as more transparent: assuming a, the conclusion is b, and assuming c, d, and so on. Politicians and the public need then no longer be burdened with often very complex theoretical ballast and (apparent) differences in theoretical approach.

Besides substitutability there is another important difference between us and some authors whose work , like ours, rests on general growth theory, discussed by Sylvie and Martin in their Section 3. This other difference pertains to whether or not it is assumed that the economy is currently on an optimum path. We justify our assumption that such is not the case by pointing to the existence of 'blockages' preventing preferences for environmental functions from being expressed. We then draw the conclusion that exponents of general growth theory would arrive at the same results as we do if they admitted the existence of such blockages and at the same time assumed absolute preferences for sustainability, defined as the preservation of vital environmental functions for future generations.

In their Section 3 Sylvie and Martin define a green Net National Product (gNNP) and argue that within 'weak' or general growth theory Hartwick's rule (applied to all future moments in time) is not a sufficient condition for obtaining an indefinitely constant (or growing) gNNP,

which would then be equal to the sustainable national income SNI(i) as defined by them. However, if one proceeds from the more general welfare indicator *v* (*cf.* H&B, p.24, draft) a different conclusion is reached, provided the availability of each environmental function is included in the analysis right from the start. Constancy of environmental function levels is then found to be a precondition for sustainability. This condition follows - within the weak theory - from Hartwick's rule for the special case of an invariant (*i.e.* not explicitly time-dependent) environmental-economic model and human welfare being determined only by the quantities of goods consumed, for the production of which environmental functions are essential. In the more general case of welfare also being influenced directly by such functions (for example, 'air for physiological functioning', *i.e.* breathing) the precondition of constant function levels and Hartwick's rule (second condition) do not boil down to the same thing.

Together, though, these two conditions are certainly sufficient for either an indefinitely constant (or rising) welfare indicator *v* or a ditto *volume* of consumption y_s. *We* define the latter welfare indicator (consumption volume) as the SNI, provided it is maximal and the model is invariant. See, for example, Asheim (1994). If due allowance is made for all preferences, *i.e.* including an assumed overriding preference for sustainability, then in theory the welfare indicator *v* must be maximised. However, calculation of *v* depends on an (as yet) unsolvable model (*cf.* H&B, Section 5.4) and a comparison must moreover be made with the standard national income. In practice, therefore, we have opted to estimate the indicator y_s. As *v* and y_s are not at a maximum simultaneously, each of these indicators is associated with a path of its own on which, approximately speaking (*cf.* H&B, Section 6.6), the assumed predominant preferences for sustainability are optimally satisfied; for each indicator, then, a particular Hartwick condition is valid. Contrary to what Sylvie and Martin state, the prices of manufactured and natural capital goods and human capital (labour) are certainly allowed to vary along that path; *cf.* Asheim (1994) and of course the deduction of Hartwick (1978). In fact they must of necessity do so, because non-renewable resources will have to be substituted in order for their functions to be sustained. The authors' conclusion that prices must remain constant to arrive at an SNI is therefore incorrect.

This leaves unaltered the fact that capital gains may indeed play a role, *viz.* outside the path on which the national income is *itself* maximum and sustainable, for example the national income on the path where the welfare indicator *v* is maximum and sustainable, in other words indefinitely constant or rising. In our contribution to this volume we have therefore defined the SNI as the national income that must be maximum and sustainable and therefore determines the path. Another example is Hicksian income, discussed in my response to Herman Daly.

In their paper Sylvie and Martin do not mention the fact that the economy might not in fact be on an optimal path; but their wording does not appear to exclude that possibility. If they can accede to the existence of blockages and a range of different optimal paths, the main disparity between our respective approaches probably vanishes. There remain several other points, which we also feel can be resolved.

In their Section 3.2 Sylvie and Martin state that the opportunity costs involved in achieving sustainability "usually must be estimated" by comparing the present development path

with the transition path from there to sustainability. They make no mention of the fact that the national income on the present path may also be compared with the SNI on the (admittedly unfeasible) sustainable path, just as can be done in the 'weak' theory considered by them earlier. To our mind this unfeasible, unblocked sustainable path provides far more information about sustainability and the distance to it than the feasible transition path with maximum income. The opportunity costs found in this way are a monetary measure of the distance between these two paths. The SNI in question is the 'strong' variant of the volume of the SNI(i) they themselves introduce; *cf.* the definition on p.10 (draft). The text of the first full paragraph on p.11 (draft) appears to conflict with this definition, however. The depreciation during the current period certainly does not reflect the distance between the paths on which current GNP respectively the SNI are situated, for deduction of this depreciation from current GNP yields a green NNP associated with the same path as current GNP and not the SNI associated with the sustainable path.

We are therefore unclear as to why the authors, in their representation of our work on p.18 (draft), do make a distinction between an indicator for the distance to sustainability and one for the costs of achieving sustainability. In our approach too, we should add, the modelling results include a sectoral breakdown of opportunity costs. On this point see my response to Ekins, p.5, 6, 7, 8 and 9 (draft) and the paper of Verbruggen *et al*. (this volume).

We argue (a) that the SNI, the maximum national income associated with an (unblocked) unfeasible path, can be estimated using a general equilibrium model and (b) that none of the points on the transition path satisfies the definition of SNI, even if that path is sustainable to the extent that environmental functions remain preserved at minimum sustainable levels. In that case it is only at its very end that the transition path approximates SNI. See Figure 3 of our contribution to this volume, in which *f* represents the transition path. We therefore consider Sylvie and Martin's work as *complementary* to our own.

In our calculation of SNI we feel we have resolved the problem of price ratios (p.12, draft) by using prices in which the opportunity costs involved in achieving sustainability are internalised, just as the charges levied to induce shifts to environmentally more benign activities when further technological improvements become too expensive. The reasoning here is that the price ratios thus obtained approximate those holding in the sustainable situation, in contrast to the current price ratios (*cf.* H&B, p.31, draft).

The 'chicken and egg problem' referred to by the authors goes beyond the choice of prices, however, and beyond the feasibility of ascertaining the path of maximum feasible constant (or never-declining) national income in a theoretically all-encompassing environmental-economic model. The problem, rather, lies in extrapolating to a sustainable situation for which the model can be only very partially validated. It is a familiar problem that besets any environmental model used to chart the impact of particular measures on the state of the environment or to assess what measures are to be employed for achieving a particular environment target. The inaccuracy of such projections and the uncertainties surrounding them are often felt to be acceptable, given the elected purpose of indicating the consequences of

human activity for the environment and for society within the framework of a decision-making process necessarily involving due improvement of that process.

Our method addresses the unsolvability of the all-encompassing model by breaking it down into two smaller models: one for computing environmental pressure standards, the other for computing the effect on national income of achieving those standards. This does not fundamentally alter the chicken-and-egg problem posited by Sylvie and Martin, who thereby follow Norgaard. However, introduction of an SNI calculated in this fashion and of the associated economic analyses that implies would certainly render decision-making a far more structured process. We are keen to further develop our contribution to these methods in dialogue with kindred researchers like Sylvie, Martin and the other contributors to this volume.

9. Harmen Verbruggen, Rob B. Dellink, Reyer Gerlach, Marjan W. Hofkes and Huib M.A. Jansen; Robert Goodland; Karl-Göran Mäler

As already mentioned, my responses to Paul Ekins and Herman Daly have been rather more extensive than I first anticipated. There have been no complaints from the editors, however, for they consider this volume first and foremost a record of a debate - and I am in full agreement. After all, it is only in the course of serious debate that misunderstandings come to the surface and theory can be advanced. Still, I am glad my response to this final cluster of authors can be brief, keeping this rejoinder chapter within reasonable bounds.

I am grateful to all the authors who have contributed to the symposium and to the present volume. I am sure nobody will be blame me, though, when I say that the most important contribution for me has undoubtedly been that of Harmen Verbruggen and his colleagues. I can do no better here than to reiterate the thoughts expressed in my speech on the occasion of the symposium:

“It goes without saying that special thanks are due to researchers Reijer Gerlagh and Rob Dellink, with Huib Jansen and Marjan Hofkes behind them, and to the man with ultimate responsibility for the exercise with which we are today concerned: Harmen Verbruggen. They have used the model they developed at the Institute of Environmental Studies to produce a provisional estimate of the Sustainable National Income of the Netherlands. They have carried out their work loyally, following the guidelines set out in the 1992 publication ‘Methodology for the Calculation of Sustainable National Income’. The study is being overseen by a broad-based committee chaired by Frank den Butter and received funding from the ministers of the day: Hans Wijers and Margreet de Boer, whom I have already thanked in an interview in the magazine ‘Natuur en Milieu’ (Nature and Environment). […..] I hope that SNI exercises will be undertaken in other countries, too, particularly in developing countries and particularly for use by the World Bank.

I mention the loyalty of the researchers, and implicitly the committee members and ministers, mainly because in the past they have not all been equally enthusiastic about my ideas concerning the SNI. I am convinced that the frank and friendly exchange of letters between Frank, Harmen and myself prior to the study brought our standpoints much closer. They both

understand all too well that when the SNI research was transferred away from Statistics Netherlands, after thirty years, it was not the happiest day of my life. Perhaps that's why Harmen has called his end product "the SNI according to Hueting". I value that gesture enormously. Harmen, Frank and others have explained to me that, for obvious reasons, transfer of the research was in fact a blessing in disguise. I believe them and reiterate their words here."

Robert Goodland's perspective on my work is ecological. I value that enormously, and have little more to add. Whilst reading his paper I wondered briefly "What has music got to do with my environmental work?". But then I realised that Robert, as an ecologist, has understood my work far better than many an economist. Playing the piano is, after all, one of those activities that have a negligible impact on the environment and make no contribution to production growth as measured in the national income (Hueting, 1981b, 1992d).

One of the esteemed speakers at the symposium was Karl-Göran Mäler, whose draft paper was included in the symposium volume along with those of the other speakers. For reasons I fully understand, he has declined to finalise his paper. In our own contribution to the present volume Bart de Boer and I do address his work, however. We thereby conclude that the marked difference between the results of his approach and ours follows from Karl-Göran's assumption that the economy is on an optimum path, while I have consistently assumed that such is not the case. This assumption is justified by the existence of blockages preventing preferences for preservation of the natural environment from being adequately expressed (*cf.* H&B).

References

As mentioned in the Introduction (Section 1) all references to my published work, alone or with co-authors, are according to the list of publications appended to Robert Goodland's contribution to the present volume. To facilitate reading, wherever feasible reference has been made to the summary paper by Bart de Boer and myself (referred to as 'H&B' for convenience), which then points readers to my earlier work. Other references are listed below.

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