Energy Policy and Externalities:
An Overview

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Paper prepared for OECD Nuclear Energy Agency
Keynote address to Workshop on
Energy Policy and Externalities: the Life Cycle Analysis Approach

PARIS November 15-16 2001

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The uses of externality adders

Substantial progress has been made in estimating the monetary value of the environmental impacts of different energy systems. Perhaps the best known study in Europe is that sponsored by the European Commission and known as the ExternE programme (1995a-f, 1998a-d). In the USA a comparable project is that jointly sponsored by the US Department of Energy and the European Commission (1992, 1994a-d, 1995, 1996, 1998a-b). There are many others. In each case what is sought is a monetary value of an environmental impact arising from a unit of energy, usually standardised as a kilowatt hour (kWh). These environmental impacts are usually termed 'externalities'. An externality exists if two conditions are met. First, some negative (or positive) impact is generated by an economic activity and imposed on third parties. Second, that impact must not be priced in the market place, i.e. if the effect is negative, no compensation is paid by the generator of the externality to the sufferer. If the effect is positive, the generator of the externality must not appropriate the gains to the third party, e.g. via some price that is charged. In the energy externality literature, the procedure of expressing the externalities in, say, cents or milli-euros (1000th of a Euro = m€) per kWh results in an 'adder'. An adder is simply the unit externality cost added to the standard resource cost of energy. Thus, if an electricity source costs Xm€ to produce or deliver, the final social cost of it is (X+y)m€ where y is the externality adder.

While externality adders have been researched most in the context of energy, they are increasingly being estimated for other economic sectors, notably transport (ExternE, 1998and 1998c) and agriculture (Hartridge and Pearce, 2001; Pretty et al. 2000; Steiner et al. 1995). What are the uses of such figures?

First, such figures could be used to guide *investment decisions*. If major electricity sources remain in public or quasi-public ownership, then the full social cost of electricity by different sources could be used to plan future capacity, with preference being given to the source with the lowest social cost. Where electricity is privately owned, then full social cost can be used by regulators to guide new investment or to act as an effective environmental tax, leaving the private owners to respond accordingly.

Second, adders can be used to estimate *environmental taxes*. While the use of adder estimates is not typically used in this way, the UK has at least two taxes based on externality estimates which, in turn, contain elements of external estimates taken from energy adder studies (the aggregates tax and the landfill tax).

Third, adders may be used as an input into *modified national accounts*. Here the idea is to replace GNP (or, more correct, net national product, NNP) with a measure that accounts for the depreciation of natural resources, so that 'green' NNP becomes NNP - depreciation on resource - damage to the environment).

Fourth, adder estimates may be used for *awareness raising*, i.e. simply drawing attention to the fact that all energy sources have externalities which give rise to economically inefficient allocations of resources.

Fifth, adders might help with some notion of *priority setting* for environmental policy. The basic principle would be that, as a first approximation, attention should be paid to those activities generating the highest externalities. Better still, activities should be prioritised by some cost-benefit principle, so that adders are ranked according to the ratio of reduced adders to the cost of securing that reduction.

Clearly, then, estimating externality adders is potentially very informative for policy purposes. While a huge amount of research, and a large sum of money, has gone into estimating adders, problems remain. In some cases, the search for figures that can be used has perhaps obscured the need to think more fundamentally about how adders are derived, the conditions under which they might be considered reasonably valid, and the uses to which they are put. We therefore focus on just a few of the more important issues.

Externality and environmental impact: consistency with economic theory

Some of the adder literature proceeds as if any 'external' impact constitutes an externality. But this is not so. The second attribute of the definition given previously is that any third party impact must be unpriced, i.e. uncompensated or unappropriated. To illustrate the problem, consider occupational health effects, i.e. impacts on those who work in the energy industries, and, for that matter in industries that supply the energy industries, e.g. mining, or which dispose of waste from the energy industries. Since the adder literature adopts a life cycle approach, these impacts could be important. But one of the methodologies used to estimate the value of risks to life is in fact based on the notion that wages in risky occupations are higher, other things being equal, precisely because of the risks involved. In other words, risks are 'internalised' , i.e. compensated for, in wage payments. If this is true, then one cannot include occupational risks in adder estimates. Indeed, there is a contradiction in using 'values of statistical life', most estimates for which come from wage-risk studies, whilst simultaneously arguing that occupational risks reflect an externality.

We can illustrate by looking at the ExternE 1995 estimates of the externalities from a pressurised water reactor (PWR) (European Commission, 1995e, p.191). Taking a discount rate of 3%, the estimates there suggest that the total externality in mECU (now m€) is 6.00E-02 or $6x10^{-2}$mECU. Of this, 5.73E-02 consists of occupational effects. In other words, over 95% of the externality is accounted for by occupational effects. But if these effects are internalised in wages, they should not be included and the resulting externality is trivial at 0.27E-02.

Occupational effects are perhaps one of the easier sources of double counting to identify. But there are similar problems with accidents in the transportation phases of the life cycle analysis. Some accidents are undoubtedly truly external, but in many cases risks are already internalised in the decision to drive or go by train etc. The conclusion has to be that more care needs to be devoted to the consistency between the estimates and the underlying economic theory that must be obeyed if the estimates are to be regarded as useful for policy purposes.

Valuing statistical lives

A second major issue concerns the valuation of health effects in combined LCA and valuation studies. A glance at both the externality adder literature and cost-benefit studies of pollution controls shows that (a) health damages tend to dominate measures of externality and health benefits dominate cost-benefit studies, (b) within health effects, changes in life expectancy dominate. Table 1 shows a selection of studies relating to air pollutants and reveals that health benefits account for a minimum of one-third and a maximum of nearly 100 per cent of overall benefits from pollution control. Moreover, in most cases these benefits exceed the costs of control by considerable margins. Health benefits therefore 'drive' positive benefit-cost results. Nor is this outcome peculiar to the European Union. The US EPA's retrospective and prospective assessments of the Clean Air Act produce extremely high benefit-cost ratios, e.g. 44 for the central estimate of benefits and costs (US EPA, 1997). Moreover, EPA regards these as probable underestimates. In turn, the benefits are dominated by health benefits (99% if damage to children's IQ is included). The EPA's analysis has, however, been subjected to very critical analysis (Lutter, 1998; Sieg et al., 2000). By contrast, the European studies appear not to have attracted much by way of critical comment.

It may be the case that there are very high benefit-cost ratios for air pollution control, but there are at least two reasons for a feeling of unease about the results that are being obtained.

(1) The relevant studies tend to omit ecosystem benefits, despite the fact that, for acidifying substances in the wider Europe, ecosystem protection is the driver for the UN ECE region air pollution Protocols under the Convention on Long Range Transport of Air Pollution (LRTAP). If the presumption of the Convention Parties that ecosystem damage is of dominant importance is correct, this would suggest that benefit cost ratios are substantially higher than the factors of three to five being recorded in the European studies. Some would regard this is adding to doubts about the analysis, rather than reducing them.

(2) The European studies suggest that benefits exceed costs even for scenarios defined in terms of 'maximum technologically feasible reduction' (MFR) of pollutants, i.e. scenarios in which the most pollutant-reducing technologies are used. Such scenarios should be characterised by very high marginal abatement costs at very high levels of pollution reduction, precisely the context where one would expect incremental benefits to be less than incremental costs. While the benefit cost ratio does appear to fall for such scenarios relative to other more modest abatement targets, the reduction is not dramatic and benefits continue to exceed costs. Thus, AEA Technology (1999) finds a benefit cost ratio of 2.17 for a MFR scenario, compared to 2.87 for practical targets based on the relevant Protocol. The incremental benefit cost ratio of going from Protocol targets to 'MFR' targets is 1.6.

A similar picture emerges in the ExternE studies. Taking the UK National Implementation study (AEA, 1998), but omitting occupational health for the reasons given previously, Table 2 shows the percentage of externality due to 'public health' effects and to global warming. We return to the global warming issues shortly. The estimates suggest the following conclusions. First, global warming and public health effects account for virtually all the externality from all fuel cycles[1]. Second, for coal, oil and orimulsion the two effects are broadly comparable. For gas, global warming is the overwhelming impact. For nuclear and the renewables, public health dominates and global warming is relatively unimportant.

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[1] This needs to be qualified because the ExternE estimates, along with other studies, generally omit ecosystem impacts beyond crop damage.

Table 1 Health benefits as a percentage of overall benefits in recent cost-benefit studies

Study	Title and subject area	**Benefits as % total benefits**
Holland and Krewitt, 1996	*Benefits of an Acidification Strategy for the European Union*: reductions of SOx, NOx, NH_3 in the European Union	86-94%. Total benefits cover health, crops and materials.
AEA Technology, 1998a	*Cost Benefit Analysis of Proposals Under the UNECE Multi-Effect Protocol*: reductions of SOx, NOx, NH_3, VOCs	80-93%. Total benefits cover health, crops, buildings, forests, ecosystems, visibility
AEA Technology, 1998b; Krewitt et al, 1999.	*Economic Evaluation of the Control of Acidification and Ground Level Ozone*: reductions of NOx and VOCs. SO_2 and NH_4 held constant.	52-85% depending on inclusion or not of chronic health benefits. Total benefits include health, crops, materials and visibility
AEA Technology, 1998c	*Economic Evaluation of Air Quality targets for CO and Benzene*	B/C ratio of 0.32 to 0.46 for CO. Costs greatly exceed benefits for benzene. Benefits consist of health only.
AEA Technology, 1998d	*Economic Evaluation of Proposals for Emission Ceilings for Atmospheric Pollutants*	B/C ratios of 3.6 to 5.9. Health benefits dominate.
AEA Technology, 1999	*Cost Benefit Analysis for the Protocol to Abate Acidification, Eutrophication and Ground level Ozone in Europe*	VOSL + morbidity accounts for 94% of benefits. B/C ratio = 2.9.
IVM, NLUA and IIASA, 1997; Olsthoorn et al, 1999.	*Economic Evaluation of Air Quality for Sulphur Dioxide, Nitrogen Dioxide, Fine and Suspended Particulate Matter and Lead:* reductions of these pollutants	32-98%. Total benefits include health and materials damage

Note to Table 1: we have selected results using VOSL (value of statistical life) rather than 'VOLY' (value of a life year) since the latter are not correctly estimated in the studies that also provide VOLY results. See text for discussion.

Table 2 Percentage contribution of public health and global warming damages in all damages: ExternE, UK

	Coal	Oil	Orimul-sion	Gas	Nuclear	Wind	Biomass
Health	44	48	41	20	81	68	85
Gwarming	53	50	56	79	15	22	9
Total	97	98	97	99	96	90	93

Notes: health refers to public health only.
Source: adapted from AEA Technology (1998e.)

The fact that health and global warming effects dominate is important since both are very controversial both in terms of the 'dose-response' literature and in terms of economic valuation. We deal first with just a few of the health issues. A fuller discussion can be found in Pearce (2000).

The epidemiology literature linking air pollution to health effects is large. In terms of risk of death, there are two types of literature. The first relates acute episodes of pollution to life risks and the second, a far smaller literature, relates chronic exposure to air pollution to life expectancy. Most of the economic valuation literature deals with the former, i.e. with acute effects. But it is becoming increasingly clear that the chromic exposure epidemiology is more important, although acute studies still have a role to play. One of the problems with acute studies is that they may tell us numbers of people dying from acute effects but not the period of life that is foreshortened. There is a debate as to whether the life periods concerned are very short indeed, a matter perhaps of just a few days in OECD countries, or whether what evidence we do have on life foreshortening understates the true effects. This debate is well rehearsed in the contributions in Pearce and Palmer (2002). The second problem is that, surprisingly, the epidemiology tells us little about the age groups that are affected. But the available evidence suggests, as one might expect, that the relevant deaths tend to occur in much older age-groups, usually in the over 70s. Combing these two likely facts of short duration loss of life expectancy with the age effect suggests that the relevant economic value will be the willingness to pay of over 70s to avoid days rather than years of life loss. Again surprisingly, we have limited evidence on how willingness to pay relates to age, but what we have suggests that it will be lower than the willingness to pay of median age groups involved in accidents. Yet it is studies relating to the latter that tend to determine the 'value of statistical life' (VOSL) used in cost-benefit and life cycle externality studies. Figures such as €3 million are common in the ExternE studies, for example. No-one is suggesting that the values of the older generation do not matter, but we do have to question whether values such as €3 million can possibly be relevant to such impacts.

Turning to chronic mortality, while the relevant studies are far fewer and, even then, some of them simply borrow dose-response coefficients from previous studies, there is some suggestion that chronic exposure foreshortens life by perhaps six months and maybe one year. Supposing this to be true, the question then arises of what economic value we should attach to such epidemiological effects. If the effect of chronic exposure is to induce illness which may itself ultimately result in life foreshortening, then we should definitely be concerned to estimate the willingness to pay to avoid that illness. For the reduced life expectancy itself, however, it looks as if the correct value is what we are willing to pay today to extend our lives by, say, six moths when we are in our 70s or even 80s. We have very little evidence on what these sums are since the willingness to pay studies we do have relate to risks that are reduced (or increased) with effectively an immediate effect (e.g. a road accident).

Johannesson and Johansson (1996) report a contingent valuation study in Sweden where adults are asked their willingness to pay for a new medical programme or technology that would extend expected lifetimes conditional on having reached the age of 75. Respondents are told that on reaching 75 they can expect to live for another 10 years. They are then asked their WTP to increase lifetimes by 11 years beyond 75, i.e. the 'value' of one extra year. The results suggest average willingness to pay across the age groups of slightly less than 10,000 SEK using standard estimation procedures and 4,000 SEK using a more conservative approach, or say €600-1500. This is for one year of expected life increase. Using the formula:

$$VOSL(a) = VOLY \sum_{t} 1/(1+r)^{T-a}$$

Johannesson and Johansson suggest these values are consistent with 'normal' VOSLs of €30,000 to €110,000, *substantially* less than the VOSLs being used in cost-benefit and externality adder studies. Since T-a is obviously less the older the age group, then the relevant VOSLs will decline with age.

It is perhaps worth noting that 'rules of thumb' used in the ExternE work are not valid. The approach to valuing a 'life year' in the ExternE studies proceeds as follows. A 'value of a life year' or VOLY[2] can be thought of as the annuity which when discounted over the remaining life span of the individual at risk would equal the estimate of VOSL. Thus, if the VOSL of, say, £1.5 million relates to traffic accidents where the mean age of those involved in fatal accidents is such that the average remaining life expectancy would have been 40 years, then

$$VOLY = VOSL/A$$

where $A = [1-(1+r)^{-n}]/r$

and n is years of expected life remaining and r is the utility discount rate. Examples are shown below in Table 3 for n = 40 years[3].

Table 3 Deriving VOLYs from VOSLs: examples

VOSL ?m	Utility discount rate = 0.3%. A = 37.6	Utility discount rate = 1.0%. A = 32.8	Utility discount rate = 1.5%. A = 29.9
1.0	26,595	30,460	33,445
1.5	39,894	45,690	50,167
2.0	53,190	60,920	66,890
3.0	79,787	91,138	100,000

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[2] The ExternE notation is 'YOLL' for 'year of life lost'.

[3] Another way of saying the same thing is that VOLY = VOSL/Discounted expected lifetime. Strictly, the relationship holds only when utility of consumption is constant in each time period.

These VOLY numbers can then be used to produce a revised VOSL allowing for age. At age 60, for example, suppose life expectancy is 15 years. The VOSL(60) is then given by

$$\mathrm{VOSL}(60) = \Sigma\mathrm{VOLY}/(1+r)^{T\text{-}60}$$

where T is life expectancy. In the case indicated, this would be, at a 1% discount rate and a 'standard' VOSL of ?1 million:

$$\mathrm{VOSL}(60) = (30{,}460).(13.87) = ?422{,}480.$$

The result is that the age-related VOSL declines with age and this appears to accord with the findings noted earlier that willingness to pay probably does decline with age. The generalised formula for age related VOSL is:

$$VOSL(a) = [VOSL(n)/A]\sum_t 1/(1+r)^{T-a}$$

where a is the age of the individual or group at risk, T is life expectancy for that group, VOSL(a) is the age-adjusted VOSL and VOSL(n) is the 'normal' VOSL.

There are several reasons for doubting the usefulness of the VOLY approach when it is based on a VOSL.

First, the basis of the VOLY approach is the life-cycle consumption model with uncertain lifetime. It is well known that such models assume utility depends on consumption alone and not on the length of life. Lifetime utility does indeed vary with life expectancy but the route is via consumption not via time itself. It seems unlikely that individuals are indifferent to time remaining. There are also additional restrictions on the model to ensure that WTP is proportional to the discounted value of life expectancy. Thus, it can be questioned whether the underlying theory needed to derive VOLYs from VOSLs is itself tenable.

Second, the theory forces the age-distribution of VOLYs to take on a monotonically declining form: VOLY simply declines with age. What evidence we have, however, suggests that willingness to pay follows an inverted 'U' shape curve, rising to a median age and then falling. If so, the VOLY construct is a poor representation of 'true' WTP over the lifetime of individuals.

What can we conclude on the health effects and valuation in externality adder studies? While the usual academic conclusion that 'more research is needed' always seems frustrating to policy makers, the fact is that we do not know enough about the epidemiology and we certainly do not know enough about the economic valuation of life risks to be confident about the kinds of adders being produced in externality studies. In some cases, being more certain of the absolute magnitudes of the adders may not matter too much. For example if we simply wish to prioritise investments by social cost, a ranking may not be affected by what values we use. But if we wish to use the values to set, say, energy taxes, then the absolute magnitudes do matter.

Global warming

Table 2 above showed that the other major component of externality values derives from global warming effects. Everyone is familiar with the scientific uncertainties in warming studies. To those uncertainties we must add economic valuation uncertainties. Uncertainty is not a reason for neglecting economic valuation - there is a widespread but erroneous view that if we avoid trying to estimate economic values what we will end up with is a more certain base for policy making than if we do not. The reality is that whatever decisions we make about global warming policies they will all imply some set of economic values. It is better to be as explicit as we can about the numbers rather than masking them by procedures that allegedly do not use them.

Table 4 shows some of the results taken from economic studies of global warming. The relevant magnitude is the economic value of one tonne of greenhouse gas emitted now. This must allow for residence time in the atmosphere and the fact that greenhouse gases are cumulative. The relevant concept is therefore a discounted economic value of damage due to the 'marginal' (i.e. extra) tonne of pollutant. This is the basis of Table 4.

Table 4 shows that the relevant values per tonne carbon vary substantially with the discount rate assumed. This is hardly surprising. Unfortunately, while economists are reasonably good at estimating social discount rates for a single nation, the relevant discount rate for the world as a whole is a more elusive concept. Yet it is the relevant one because the damages recorded in table 4 relate to the world as a whole. More complex still, economists have not yet secured a consensus on what the relevant rate would be for very long-lived environmental effects of the kind that would typify global warming damages and, just as relevant for externality adders, nuclear waste disposal. The most promising contribution to date appears to be that of Weitzman (1999) who shows that the long term discount rate should almost certainly be declining with time (Annex 1 to this paper is an attempt to derive Weitzman's result in a much simpler way). Our first observation about global warming estimates, then, is that we need a far more rigorous look at the way in which discounting should be integrated into the damage estimates.

A second observation is that all of the estimates in Table 4 are based on the 'dumb farmer' syndrome. They do not make any allowance for adaptation to global warming. Yet, if we know anything at all, we know that people do not stand idly by and do nothing in the face of environmental damage. Unfortunately, we appear to have only one set of studies that give us any idea what would happen to the damage estimates if we do assume adaptation. In an important contribution, a volume edited by Mendelsohn and Neumann (1999) shows that total damages to the US economy could be zero instead or positive once adaptation is assumed.

Table 4 **Marginal damages from greenhouse gases ($ tonne carbon)**

Study	Estimate $ tC . Base year prices: 2000			
Period	*1991-2000*	*2001-2010*	*2011-2020*	*2021-2030*
Nordhaus 1991	9.3	9.3	-	-
Nordhaus 1994				
p = 0.03, best guess	6.8	8.7	11.0	12.8
p = 0.03, expected value	15.4	23.0	33.9	-
Nordhaus 1998	6.4	9.1	11.9	15.0
Fankhauser 1995				
with p = 0,0.005,0.03	26.0	29.2	32.4	35.6
with p = 0	62.5	-	-	80.5
with p = 0.03	7.0	-	-	10.6
Cline 1993				
with s = 0	7.4-158.7	9.7-197.1	12.5-238.1	15.1-282.9
Peck and Teisberg 1992				
with p = 0.03	12.8-15.4	15.4-17.9	17.9-23.0	23.0-28.2
Maddison 1994	7.6-7.8	10.4-10.8	14.2-14.8	18.8-19.4
Eyre et al 1997				
with s = 0	181.8	190.7		
with s = 1	93.4	92.2		
with s = 3	29.4	25.6		
with s = 5	11.5	9.0		
with s = 10	2.6	1.3		
Tol, 1999	14.1	16.6	19.2	23.0
Roughgarden and Schneider 1999: lower bound = Nordhaus, upper bound = Tol	6.4-14.1	7.7-16.6	10.2-20.5	12.8-26.9

Note: s = social discount rate. Eyre et al estimates are for 1995-2004 and 2005-2014 and the estimates here exclude equity weighting. p = utility discount rate and s = the overall discount rate. Roughgarden and Schneider's ranges derive from placing the models of Fankhauser (1995), Cline (1992), Titus (1992) and Tol (1995) into Nordhaus's DICE model framework. The upper end of the range should, strictly, coincide with the marginal damage estimates in Tol (1999).

Note: Most original estimates are in 1990$ and we have assumed an escalation of 2.5% p.a. inflation. Note also that Table 1 shows the considerable sensitivity of estimates to discount rates. The discount rates given for Fankhauser's estimates relate to the pure time preference rate component, p, only. According to Fankhauser (1995) his social cost estimates based on the distribution of values for p are equivalent to a 'best guess' value of 0.5% for p. To this must be added a value for the elasticity of the marginal utility of income multiplied by the expected growth rate. Fankhauser and Eyre et al take the elasticity to be about unity, so that the only variable is the expected long term economic growth rate of income per capita. Rabl (1996) suggests this is 1.6-1.8% pa, so that the discount rate would be 2.2 to 2.3%. Accordingly, the values in Eyre etc of 0-3% are more relevant for purposes of comparison.

There are problems with this claim if we wish to extrapolate it to the global damage estimates underpinning the marginal damage estimates in table 4. First, the estimates relate to the market sectors of the US economy only. Yet it is the non-market sectors such as ecosystem functioning that perhaps give the greatest cause for concern. Second, adaptation in the USA is likely to be greater than in the developing world where fewer technological options are available. Nonetheless, the global warming damage estimates in the externality adder literature do need revisiting in light of the Mendelsohn-Neumann findings.

While there are many problems with the global warming estimates, one that is much discussed and debated deserves some comment. In the ExternE work the global warming damages are 'equity weighted'. Global warming damages are spread across the world and affect both rich and poor countries. But poor countries have substantially lower incomes than rich countries so that one Euro's damage to them has a higher 'disutility' value than one Euro's damage to rich countries. Cost-benefit analysis typically works with notions of willingness to pay that do not reflect this adjustment for different utilities of a money unit. But there is no unique way to do cost-benefit so it is perfectly legitimate to seek to maximise utility-adjusted benefits and costs. This is what the ExternE programme does (European Commission, 1998b). In terms of the economic value of a tonne of carbon, the effect of equity weighting is to *raise* the value per tonne. This is because damages done to richer individuals tend to be used as the numeraire, with the result that damages to poorer individuals are increased relative to what they would have been without equity weighting. As shown in Eyre et al. (1997), the effect of equity weighting is approximately to *double* the marginal damage estimates. Once again, if the policy issue of concern is one of prioritising fuel cycles the use of equity weighting may not matter too much. But if absolute levels of damages matter, then it is crucial to justify equity weighting. Unfortunately, studies using equity weighting are not very forthcoming on what this justification is. The ExternE reports suggest that it is consistent with maximising 'utility' as opposed to willingness to pay-based measures of costs and benefits. This is correct. But several questions then arise. First, why do we seek to maximise equity-weighted net benefits in the global warming context but not in any other context? Second, if we do adopt equity weights, what is the justification for selecting one particular set of weights rather than another?

As far as the first question is concerned, the ExternE reports suggest that equity, and by implication equity weighting, is integral to the Framework Convention on Climate Change (FCCC) (Eyre et al, 1997, p.9). This is debatable. The equity notions in the FCCC relate primarily to the concept of differentiated responsibility, i.e. since rich countries are bigger emitters of greenhouse gases they bear more responsibility and should act first. It is a substantial leap from this idea to one of weighting costs and benefits. More telling is the fact that equity weighting in cost-benefit analysis of global warming control will raise the benefit-cost ratio of taking action. In other words, more global warming control would be undertaken with equity weighting than without it. This seems fair until we recall that whatever is spent on global warming control is not spent on other things, and the other things may include foreign aid, technology transfer etc, all of which benefits the poorer countries. In turn this suggests that we should either equity weight all policy measures that have an effect on poor countries or we should not equity weight any of them. Isolating global warming and ring-fencing it as if it is unique is not a tenable proposition. The situation could be even more complex, since global warming expenditures are likely to benefit the descendants of the current poor rather than the poor now. Yet the descendants are likely to be richer than the current poor, so that the policy of weighting damages may simply reinforce a tendency to divert resources from solving the problems of the current poor. This was pointed out by Schelling (1992).

Suppose, however, that we do accept equity weights. What are the relevant weights? ExternE (European Commission, 1998b) and Eyre et al. (1997) suggest that the weights reflect diminishing marginal utility but they actually select a specific value for the weighting procedure. Table 5 shows what is being implied for different values of the elasticity of the marginal utility of income function and for different ratios of income. Table 5 shows incomes differing by a factor of 2 and a factor of 20 (the latter is the ratio of GNP per capita in high income to low and medium income countries).

Table 5 Equity weighting examples

Elasticity of the marginal utility of income =e	-0.5	-1.0	-2.0	-5.0
Social value of high income Y1 relative to low income Y2 if Y1/Y2 = 2	0.7	0.5	0.25	0.03
Social value of Y1 if Y1/Y2 = 20	0.2	0.05	Neg	neg

Note: the relevant formula is $w = (Y1/Y2)^{-e}$ where w is the weight on Y1.

What the Eyre et al. and ExternE reports do is to select e = -1.0. For an income differential of 2 this would imply that we value the higher income group's marginal income as being just 50% of the value to the low income group. This seems potentially fair. But the income differential is actually 20, not 2, so that choosing -1.0 gives a weight of only 5% to income gains in the rich country. Many people might think this is also fair, but it is categorically not how we behave. If it was it would be impossible to explain why OECD countries devote far less than one per cent (on average) of their GDP on foreign aid and very much more than this on domestic life saving programmes.

The point here is not to assert that equity weighting is wrong. Indeed, not adopting equity weights amounts to choosing the equity weights implicit in the prevailing distribution of income. There is no escape from equity weighting! The issue is whether the studies adopting specific estimates for externality adders should adopt equity weights without (a) explaining why a specific set of weights is chosen, and (b) explaining why those weights are relevant to global warming but not to anything else.

'Disaster aversion'

A final issue that is very relevant to the externality adders literature is the treatment of disaster aversion. The idea here is that lives are at risk from energy sources. The usual procedure in the life cycle studies is to estimate accident rates and then value the resulting accidents at the relevant VOSL. In this case, use of 'standard' VOSLs is probably correct because those at risk are the general population. It is widely thought, however, that the population is not indifferent between, say, 10 deaths in one accident and 10 deaths in 10 accidents each with one death. This is the notion of 'disaster aversion' whereby the economic value attached to the former event would be higher than for the equivalent number of deaths in the ten accidents. The issue is obviously particularly relevant for nuclear power externalities, but it is also relevant for, say, gas explosions affecting the general public. The ExternE adder estimates do not in fact contain disaster aversion factors, but the ExternE background papers have discussed the issue quite extensively. While great ingenuity has been brought to bear on the kinds of aversion functions that might be specified, remarkably little empirical work appears to have been done to test whether people really are averse to disasters.

Ball and Floyd (1998) reviewed studies of disaster aversion and concluded that ‘there is very little evidence for differential risk aversion by the public where this is based upon number of fatalities.’ For example, Jones-Lee and Loomes (1995) found that, in a transport context, the risk of large-scale accidents did not contribute to public willingness to pay for safety improvements. Ball and Floyd (1998) also cited a study, Hubert *et al*. (1991), which assessed the disaster aversion of senior managers in the petroleum, chemical and transport industries, as well as elected officials, and found that disaster aversion was significant in this group of people.

However, Slovic *et al*. (1980) found that, in the nuclear context, the perceived risk was much greater than the actual risk, despite a perception of having the lowest annual number of fatalities compared to the other risk contexts studied. This discrepancy was assigned to the perceived potential for disaster. Slovic *et al*. (1982, 1984) found that accidents also sent signals about the possibility and nature of future accidents, and asserted that, for nuclear accidents, these secondary impacts may be most important in this because the public perception of nuclear accidents is of poorly understood risks with potentially catastrophic impacts. A core damage accident may send ominous signals that the technology is out of control, even if the number of injuries or deaths was small, and that could be very damaging to the nuclear industry as a whole.

As it stands then, there is an urgent need to test for disaster aversion. It could have a substantial effect on externality adders for nuclear power and perhaps for natural gas. So far, however, we have little evidence to suggest that people are averse to collective deaths in the manner suggested by some of the theoretical literature.

Conclusions

Many other issues in the externality adder literature could have been addressed. Probably the biggest omission here is the extent to which we are justified in 'borrowing' figures from other studies and using them in studies such as those by ExternE. This is the issue of 'benefits transfer' and it is very much debated in the environmental economics literature. Major omissions in the adder studies relate to the absence of 'meta-studies' even though some of the adder studies refer to meta-analysis. But it seems that what is usually meant is that the literature on damages has been surveyed. Proper meta-analysis involves statistical efforts to explain the variance in damage estimates and comparatively few of these exist.

What we can say is that, thanks to the substantial efforts of exercises such as ExternE, we are far better informed about externality adders than we were a few years ago. This is to be welcomed. The problem remains that the theoretical underpinnings are, in some cases, still weak.

Annex 1 Why the long run discount rate declines with time

The discount **rate**, r, needs to be distinguished from the discount **factor**, $1/(1+r)^t$. It is the discount factor that gives the weight applied to each time period. Suppose the discount rate and hence the discount factor is not known with certainty and is a random variable. Suppose it takes the values 1…6% each with a probability of 0.167. Table A1 shows the relevant values.

Table A1 Values of the discount factor

r	DF_{10}	DF_{50}	DF_{100}	DF_{200}
1	0.9053	0.6080	0.3697	0.1376
2	0.8203	0.3715	0.1380	0.0191
3	0.7441	0.2281	0.0520	0.0027
4	0.6756	0.1407	0.0198	0.0004
5	0.6139	0.0872	0.0076	0.0000
6	0.5584	0.0543	0.0029	0.0000
Sum	4.1376	1.4898	0.5900	0.1589
Sum/6	0.7196	0.2483	0.0983	0.0265
r*	**3.34%**	**2.82%**	**2.34%**	**1.83%**

Note: DF_{10} = discount factor for year 10, etc. r* is the value of r that solves the equation shown in the text.

While the weighted average (expected value) of the discount rate stays the same in all periods (3.5%), the discount factor obviously varies with time. The value of the implicit discount rate, r*, is given by the equation:

$$\frac{1}{(1+r^*)^t} = \frac{\sum DF_{t,i}}{n} \text{………}i = n$$

where n = the number of possible discount rates, DF is the discount factor and t is time.

Table A1 shows that the implicit discount rate goes down over time even though the average discount rate stays the same for each period.

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