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## **ECONOMIC EXPERTISE AND DECISION-MAKING IN CONTROVERSIAL UNIVERSES**

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Integrated assessment aims at gathering a variety of information sources and analytical pieces into one unique framework in order to authorise a broad look at a specific issue of concern in a policy context, such as acid rain, or global warming. A major direction has been to develop appropriate model frameworks to achieve such an informational integration, while providing an explicit tool to perform uncertainty analysis (Dowlatabadi and Morgan, 1993). Less efforts have been devoted to the ways of establishing a link between the framing of such assessments and the diverse concerns of stakeholders taking part to the policy process. Such a link should be specifically addressed (Tognetti, 1996) for avoiding integrated assessment to become a new black box strategically instrumentalised by some social actors. Typical environmental issues are not only affected by extensive uncertainty about critical variables for decision-making, which is acknowledged by most scholars, but by long-lasting scientific and social controversies based on an indecisive confrontation of different theories of the world (Godard, 1996). Enough consideration has not been given to the consequences of this controversial state of knowledge in respect to the type of framing of issues that should be elicited. For instance, an ex post analysis of the acid rain case in the USA (Perhac, 1991, p. 26) concluded that “*the utility industry put too much faith in science as a means of resolving a complex scientific/political issue,*” consequently perceiving themselves as a net loser in the acid rain regime adopted.

There are two questions to be considered: a) to which extent the type of framing which may be appropriate for scientific explorations of some issues can also be relevant in a decision-making context, in which achieving an agreement among stakeholders is the key problem? b) what is the practical usage being made of economic tools and frameworks when they cannot

contribute in scientifically legitimate conditions to the achievement of an agreement? For illustrative purpose of the processes involved in the circulation and usage of scientific results and frames, I will take one case among economic modelling exercises that have been developed for studying the global warming issue.

Prominent economic model assessment generally assume that optimising long run pollutant emissions and development trajectories within the framework of classical growth theory or the cost-benefit rationale is meaningful for decision-making (Cline, 1992; Nordhaus, 1991). Such approaches do have positive analytical contributions to enlighten the logical structure of some issues: identifying the trade-off involved as regards consumption flows; stating "impossibility theorems" (with not substitutable natural assets and no margin for technical progress, any consumption path will not be sustainable); showing the critical importance and indecisiveness of the choice of the discounting factor (every value between 1% and 8% has been justified by rational arguments), which leads to valuations differing by several order of magnitudes; or revealing the implicit or shadow value of natural assets or scientific information implied by such and such decision, etc.

In spite of these qualities, these exercises are not able to directly give reliable, substantive results for making practical decisions in controversial contexts. They are made of so many assumptions and approximations that they can only be judged impressive or decisive by people who only take end results without questioning the means used to obtain them. As said in the IPCC WGIII last report ( Bruce et al. 1995, chapter 8): "*Although researchers attempt to incorporate their best understanding of development processes into the studies, neither the baseline nor the intervention scenarios should be interpreted as representing likely future conditions, especially more than a decade into the future.*" So, what is their social use? Quite often, such economic assessments receive a rhetorical role to force some framing of social issues well beyond what they can legitimately provide in terms of validated results. What seems to matter in developing this role is the appropriateness of results and conclusions to the preconceptions that some prominent social actors (governments, lobbies) want to disseminate within society and public opinion. In these cases, instead of being a way to achieve a sound social agreement on climate strategy, they take part to the very process of controversy, and may even amplify it in raising new points of dissent, as it has been shown by the IPCC case of valuing human lives in the North-South context.

To really be a means to build a broad social agreement, long run substantive optimisation would require that its debatable assumptions and preconceptions become largely accepted social conventions, not only within the community of mainstream economists, but among main social stakeholders in a position of having some influence on the decision-making process. Welfare utilitarianism would have to be generally accepted as delivering the right principles for identifying the public interest. This is not well-adapted to a decision universe where several legitimate justification orders coexist and welfare considerations mix with concerns for obligations and rights. To positively contribute to the social process of decision, economic expertise should then be adapted to this controversial context. What is on the agenda is to look for a framing of issues that could be less-demanding as regards the level of information on states of nature, technologies and preferences, and about the content of an agreement (priorities and values). This can be better done through the adoption of a sequential approach to decision-making (Bruce et al., 1995; Hourcade and Chapuis, 1995), a one which

is focused on preparing the next steps. This involves a radical change in the justification of taking action. Accepting to address the climatic change as a serious threat, the challenge is to increase our future capacity to efficiently react to new progress of scientific information, and to look after the reproduction of running conditions of existing elements of natural conditions we decide to consider as a "common patrimony" to be transmitted to the next generations. Alternative criteria such as "robustness" or the "precautionary principle" and a focus on the articulation of the various time dimensions involved by natural and social processes could help to structure these new forms of assessment. Implementing this approach still requires an agreement on these basic criteria. So, integrated assessment should firstly be posited as a framework for discussing criteria and assumptions on the basis of a collective deliberation among experts, and secondly as a way to connect assessments to the concerns of various social groups, without assuming that the latter share common "visions of the world", values and priorities, or an agreed social preference function.

A short story may illustrate the points. Alan Manne and Richard Richels are well-known for their influential contribution to the international expertise in relation to global warming. Indeed, they have been specifically said to have influenced the US government's position against premature action in the global warming case: due to the presumed high economic cost of any serious action on carbon sources in the USA, they only favoured large investments in research. In February 1992, a few months before the achievement of the Rio negotiation, they released a draft paper analysing the potential impacts on the US economy of the adoption of the EC proposal to introduce in all OECD countries a combined carbon and energy tax. This draft was immediately echoed in the press, notably in the *New York Times* (Passell, 1992). After peer review, the draft was published in January 1993 in the academic journal *Energy Policy*. I suggest to look at these three steps with some detail.

#### ***First step: the draft paper released in February 1992***

Using their Global 2100 model, the authors drew several conclusions, the two major ones being that the EC proposal may not be sufficient to accomplish the stated objective of reducing emissions to below current levels and, secondly, that a tax on nuclear energy will only add to the difficulties of achieving the target by reducing incentives to use carbon-free sources of energy. But they also compare the respective impacts on the US and EC economies and conclude that the cost for the US economy will be 50 per cent higher in GDP percentage than the cost for the EC economy because Western Europe uses approximately 40 per cent less energy per unit of economic output. They present the figure reproduced below as Figure 1 and add (p. 15): "*To the extent that a lower energy/GDP ratio reflects lower energy use by industry, there may be implications for international competitiveness*". They had taken the precaution to acknowledge, in a section called 'Assumptions', that: "*As with any analysis, the results are largely determined by a handful of critical assumptions*".

#### ***Second step: a translation given by a newspaper article***

In the *New York Times* of 26 February, 1992, Peter Passell picked up the idea that the cost will be higher for the USA than for Europe in the following terms: "*The United States uses more energy per dollar of output than Europe. Thus it should not be surprising that the Manne-Richels simulation shows that a broad-based energy tax would be even more*

*expensive in America than in Europe. And while the broad tax might serve the parallel function of blurring the impact on regions and industries, the fact that Europe would end up with an overall economic advantage would surely depreciate its political currency in the United States*". So the public expression of the academic work of Manne and Richels refers to "*the fact that Europe would end up with an overall economic advantage*" in its competition with the USA; this statement does not come directly from the authors, but is not contradictory to what they say and seems to respond to a commonly shared belief in the USA, i.e. the result is presented as common sense.

### ***Third step: the academic release in Energy Policy***

When the Manne and Richels' paper was published in 1993, it displayed no significant difference to the draft, with the exception of one section: the question of the relative cost to the EC and USA. An entirely new argument had been introduced on the calculation of the dead-weight loss cost of a new tax when existing prices of commodities are already taxed. This point had been ignored in the draft model simulations. This addition resulted from peer discussions and notably from the findings published in papers by OECD economists Peter Hoeller and Jonathan Coppel (1992). Without mentioning their already disseminated previous results, Manne and Richels now explain that "*when measured in terms of economy-wide costs, the EC proposal is likely to have an even larger impact on Western Europe than on the USA. (...) the additional tax distortions would be considerably greater than those in the USA. (...) We estimate that the average cost (as a percentage of GDP) would be nearly three times as high in Western Europe as in the USA*" (pp. 9 - 10). They then show what is now the figure reproduced below as Figure 2. The two figures depict completely opposite realities as regards the region which would bear the highest cost. The cost to Western Europe in 2010 makes a jump from 0,4 % of GDP to more than 1,2 % ! This seems to be good news for the USA, but I have not heard of Peter Passell explaining how positive could be the carbon tax for the US competitiveness! Between February 1992 and January 1993, the EC proposal had been killed and the Rio convention signed without any commitment on a co-ordinated instrument.

This story-telling could go on, because this issue of differences in existing taxation schemes and the assessment of dead-weight losses has been intensively debated since 1992<sup>1</sup>, with the concept of possible macro-economic double dividend associated with 'revenue neutral' fiscal reforms: what should be taken into account is a net distortionary cost of a global reform including a new carbon tax, and not only the gross additional distortionary cost associated with the specific carbon tax (Godard, 1993).

Some general lessons may be drawn from this example:

- Specialised scientists do know that their findings are contingent results from models, depend on '*a handful of critical assumptions*', and are not a direct expression of reality. It is thus quite evident for them that changing assumptions and adding variables will change the results. They do not see a threat to their credibility in this change of outcome, inasmuch as it does not just reflect a change in 'opinion'.

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1.- See for instance the review by Ekins (1995).

- When placed in the position of experts, scientists are listened to as if they were talking of reality, not of contingent results and constructs. Even if they do not personally express themselves in this manner, what they write and disseminate is interpreted as such. All the qualifications about assumptions and specific methods are easily forgotten. Therefore, the circulation of scientific results in circles larger than the original specialisation generates an ossification of contingent statements into illusory ‘hard facts’. When some scientific statements are in accordance with pre-existing beliefs, they are even more readily selected by non-specialists as true and direct expressions of reality.
- The value of collective expertise should not be mainly placed in the quality of specific results gained at any moment, but rather in the open process of informed debate among specialists and stakeholders, with feedback to research. Such a process is itself highly valuable, not because it can progressively lead to some consensus - it may on some points, but it may not on others- but because the key issues and variables are made apparent through the debate. Helping to pose the right questions and to provide a sound framing to a fuzzy set of issues is what groups and open debates of experts can best do. All this needs time - several years - and may be contradictory to the want of policy-makers to get clear-cut advice within a short period.

### COMPARISON OF GDP LOSSES

Percentage of GDP

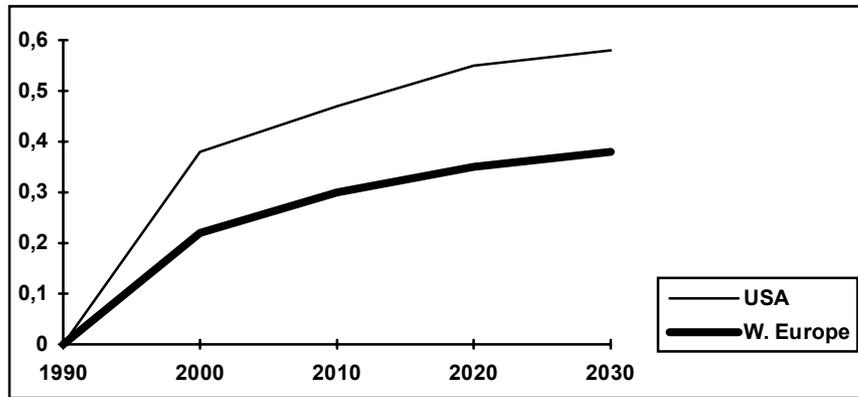


Figure 1. The figure extracted from Manne and Richels' draft of February 1992

Percentage of GDP

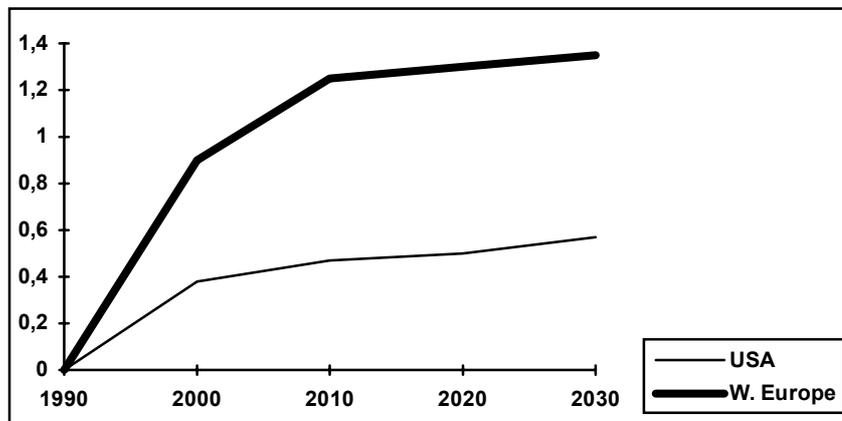


Figure 2. The figure extracted from the *Energy Policy* paper in January 1993

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