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## **Social Decision-Making under Scientific Controversy, Expertise, and the Precautionary Principle**

Olivier GODARD\*

### **Abstract**

Integrating scientific inputs into the regulatory process is generally attributed to experts. But, environmental issues are often characterised by an all-pervasive uncertainty and scientific and social controversies which make the experts' task difficult. This paper presents the concept of social decision-making under scientific controversy and comes to an examination of the implicit but decisive roles expected from expertise in those contexts. It also gives some examples of misunderstandings about the very nature of scientific statements. Since a new principle, the Precautionary Principle, is said to bring appropriate responses to uncertainty, we examine the change in the course of relationship between science and decision-making it may have and we test its operational capability to solve decision problems on a scientific basis. Our conclusion is that in controversial contexts, the Principle has no definite content and is not able to frame a scientifically determined hierarchy of options. So, to have it reasonably translated to deal with practical matters, some effort is expected from scientific communities : they should organise a collective validation of their expertise, distinct from the organisation of expertise by public administrations. This expertise by scientists should consider with due attention the process of formulation of assumptions and building conjectural possibilities, so as to define the boundaries of relevance for action and to establish some gradual scale in the qualification of scientific products which may authorise gradual precautionary measures to be taken.

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## ***Introduction***

Among environmental issues, some are condemned to remain in a scientifically controversial, non-stabilised state of knowledge for a long period of time. Because of threats of possible, large, and irreversible damage, some of them have nevertheless been placed on the public agenda, without waiting for a stabilisation of knowledge. Groups of experts have been committed to elucidate if any action should be taken, which one and when it should be. On the basis of their advice and reports, some decisions have been taken nationally or even internationally. As a matter of fact, in public presentations and debates, decisions to act or not to act have been quite frequently said to be based on scientific evidence, as if science was unambiguously forcing action in some way, even in controversial context. And more often than not, the storytellers of exemplary cases do not avoid becoming "revisionists" in their way to develop a linear and rational story full of hard facts, scientific evidence and policy responses coming in due time, in the place of a confuse process showing strange sequences where, for example, decisions are taken before the events which are supposed to explain them. To which extent this image of a science-based decision-making process can be hold to be true is very questionable in several recent cases.<sup>1</sup>

This is not to deny any role to science or experts. But it has to be looked at which roles exactly have been played in these cases. Science is an ingredient, but within the holes and shadows of science, we find choices and approximations typical of some social framing of issues. The role seemingly tacitly expected from experts is not just to be the spokesmen of objective qualities of natural or technical systems. Which one is a first topic of this paper.

Beyond answers to this first question, it seems more and more difficult to separate the logic of knowledge from the one of action : in scientifically controversial contexts, science is invested by different types of strategic use for the benefit of political projects or economic interests. It is in this context that a new principle has recently emerged in the rhetoric of justification as a response to uncertainty and controversies, the "Precautionary Principle". It first began to be incorporated in soft law and is now in the phase of trying to be translated into operational and binding rules. But what sort of change can we expect to observe in the relationships of decision-making with science ? This will be a second topic of this paper.

Throughout this contribution, our ambition is to draw the attention to some stylised facts that may affect the common perception of the role of scientists and experts in public decision-making. The empirical basis will be extracted mainly from regional and global environmental cases : forest decay in Europe, ozone depletion, global warming.

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1.- On this general issue of science, expertise and decision-making, see for instance R. Smith and B. Wynne (1991) and GERMES (1991, 1992).

## ***1. Decision-making under scientific controversy : variables and stylised facts***

A classical way of looking at the regulatory process assumes a stabilised social and material (technical and natural) world, and an appropriate knowledge. Experts are supposed to have the key role for identifying the real problems to be resolved and the range of solutions it will be rational to put into practice. By following these competent recommendations, political decision-makers will just confirm their general attachment to public good and general interest, defined through general principles (justice,...) and aims (preserving public health,...). Quite understandably, conflicts may happen, but these conflicts only involve contradictory interests emerging from a common knowledge background about possible states of the world and causal relationships ; these are conflicts resulting from divergent projects or, very simply, distributive conflicts. Real-life cases are not so clear-cut. Not only uncertainties are often pervasive as regards matters targeted by new regulatory efforts, but the context is also non stabilised as regards the social actors or interests of concern. By contrast, we suggest to consider an ideal class of problems, named 'controversial risks' (Godard, 1992b, 1993b).

### **Four characteristic variables of controversial risks'**

The ideal class of "controversial risks", as opposed to various forms of regular risks commonly addressed by insurance activities, is proposed to be characterised by the position of four variables :

- *A predominance of the scientific and social construction of issues on direct perception by people* (who has ever heard about the stratospheric ozone layer before some scientists say it will be depleted by human actions such as Supersonic flights or CFCs' emissions, resulting in additional skin cancers ?) ; lay-people cannot have a judgement on these issues but through the social process of issue building and communication involving scientists, public administration, social groups devoted to some cause (green ONGs, for example) and the media.
- *Interests of third parties absent from the social scene* (foreign countries, future generations, natural species if they are recognised rights for their own sake), or of non-reducible forms of collective concerns, being said relevant, at least by some influential actors ; such interests are asked to be taken into account through explicit and specific forms in the decision-process ; so public debates involve at the same time various and competing people claiming to be the spokesmen of absent third parties interests, and of people speaking for actual, "here and there", groups of interests. This hybrid composition certainly adds to the complexity, if not confusion, of debates and to the difficulty of resolving the problem of coming up to a justified decision ; for instance an economic

analysis based on methods for revealing individual preferences of existing people (such as contingent valuation) may have to be combined with ethical discussions about the possible rights of future generations.

- *A long-lasting controversial state of scientific knowledge on critical parts of the problem relevant for policy-making* (causation chains, consequences for human activities and welfare, time profile of expected changes...); not only are there several contradictory explanations and theories coexisting at one given moment, but there is also a dynamic of changing equilibrium between these theories, and of emerging new views changing the whole landscape, as regards the process of causal imputation. Depending on when decision-makers decide to do something about it, they will not focus on the same causes... To take one example, it will suffice to recall that the whole ozone story began in the seventies by an attack against Supersonic flights, said by some scientists in the US. as depleting the ozone layer. Twenty years later, the current scientific view is that these flights, at the altitude at which they would have been achieved, would have contributed to increase ozone density...
- *Notably because of potential irreversibility, a perception at least by some social actors, that the problem is a major stake*, touching questions such as political or national security, public health, historical or cultural identity, group survival, or general conditions of life enhancement on earth, and so deserving immediate and strong action, without waiting for the very momentum of certitude. The rational view, i.e. "learn first, then act" has to be partially reversed into "act first, then learn". But on which base are decision-makers going to decide, and what can be the role of experts in that case ?

Global environmental risks such as the ozone layer case or global warming, clearly belong to the latter class, but they are not the only ones : hazardous substances or emissions possibly generating threats for human health stand in the same class. In these controversial contexts, economic and social interest and strategies have taken a significant part in the process of building a new institutional and regulatory landscape. They have done it, not only through traditional forms of lobbying but also by investing the domain of science and expertise. So reconsidering the role of experts in the management of controversial risks seems to be of interest to elucidate the social processes combined in those contexts.

## **The regulatory process as part of a self-organisational, self-referent, socio-economic process**

Unfortunately or not, what can be observed does not fit with the conditions of relevance of the pure economic model of substantive rationality, which poses full knowledge (deterministic or probabilistic) as a condition of rational action, rational regulations, in this case. Here, actions chosen will interplay with the process of opening and closure of possible courses of evolution (in technical terms, states of nature and actions are not independent). Two variables have a key role to this regard : a) technological progress, which has to be seen as a socially endogenous process depending on social rules and strategies of actors and so, on social values and expectations about the future ; b) timing of processes : a sharp change of environmental conditions may forbid any evolutionary process of ecosystems ; a slow change may not be disruptive. So the idea of founding decision on a consistent and independent reality has to be substituted by a broader view of the regulatory process as a self-organisational, self-referent socio-economic process in which representations and realities are condemned to be intertwined, inducing a process which can be seen at the same time as producing the reality and learning from this new reality through an interpretation of the signals that real changes seem to give.

Turning this view in economic terms, we should learn to add to the standard statement that, for example, "*one technology has been chosen because it was the most profitable one*", the complementary view that "*this technology became profitable because it has been chosen first*", i.e. being made credible by persuasive stakeholders, this technology has attracted an important flow of economic resources to increase its initial productivity and to organise the economic and institutional space<sup>2</sup> so as to get the best from it. It is true that every story of this type does not turn out to be a success ; economic and technological development cannot be understood just as automatic self-fulfilling prophecies. There may be physical limitations or obstacles on the road ; a contradiction between the time-scale of the period of investment needed (how long is it necessary to invest before having an operational and competitive technology ?) and the time-horizon of investors may emerge, depending on predominant models of economic organisation, and so on.

Keeping the same idea when looking at the dynamics of emerging new regulations (Seveso directive, new insurance arrangements for accidental pollution of seas, ...), we should add to the view of the regulatory process as a response to critical events a complementary understanding in which some social actors and groups are strategically using events as a

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2.- Laurent Thévenot (1985) has introduced the concept of "investment in forms" to embrace types of investments required to establish and maintain some new forms of organisations and conventions, implying the constitution and stabilisation of networks of actors whose behaviour is to be aligned ; examples are the development of new types of accounting (think of the challenge of setting environmental accounts) and audits, a framework for collective negotiations, a branch business organisation, ...

resource to achieve their projects or, more frequently, to advance some steps towards this achievement. In that case, the course of evolution, apparently obeying to contingent events, should be looked at as the progressive achievement of some leading purpose connected to some leading "vision of the world". This is the reason why some events, though catastrophic, remain meaningless for the evolution of institutions and rules, and others, maybe scaled at a lower rate as regards magnitude, may have a critical influence on the process of change.

### **A set of stylised facts for a descriptive model**

It is useful to typify some of the facts we can observe in the social process of decision-making under scientific controversy. This way, we may contribute to the modelling of the way this class of controversial risks is tackled.

#### *Scientific warnings being sent in spite of a non-stabilised state of basic knowledge*

Some scientists are launching warnings to public authorities and opinion against possible current and future detrimental trends of evolution and threats affecting the biophysical environment (atmosphere, forests, biodiversity, ...) or public health ; they are presented as potentially very damaging and implying human responsibility, either directly because the involved phenomena result from human activities, or indirectly because man has the technical capability to change the course of things. This dimension of responsibility is critical : to which extent may the observed phenomena be said 'natural' or attributable to human activities?<sup>3</sup> If some human responsibility is involved, sharp debates are occurring about its attribution. So in the greenhouse gases issue, hard discussions took place, dealing with the level of deforestation and the burning of the Amazonian forest, or the methods used for calculations of emissions from various countries.

Another point of interest is that standard prerequisites of probabilities calculations cannot be satisfied in the class of risks here considered, since 'states of the world' are not whole well-identified (depending on actions, for a part), do not belong to the same basic worlds and their list is continuously changing.

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3.- How far global warming is related to natural variability or changes ? How far the ozone hole is a periodic natural phenomenon which was simply unknown before its discovery ? How far the general forests decline in Germany in the eighties can be viewed as a time-lagged effect of severe droughts having hit Europe in the seventies ?

*The jump of scientific controversies onto the public agenda*

Up to that point, the very existence of scientific controversies is just quite a normal course of scientific affairs. But scientific debates used to concern only a relatively close scientific community and to be conducted along specific professional procedures and rules. What seems typical of the recent period, some scientific controversies are now rapidly coming on the public agenda through a large impact in the media, before being closed on the scientific ground. This arises when, through receptive media, some specific events give a striking and perceptible 'face' to phenomena - severe droughts, the Antarctic ozone hole, trees losing leaves or needles, several people seen in hospitals or reported to be died because of some illness related to hazardous substances. Suddenly a lot of people, opinion groups, politicians and economic actors realise that something is going on, which may affect them. Moreover, the plurality of competing scientific theories proposed involve many more interests groups than a well-defined, limited issue.

This shift from the scientific field to the political arena is not a simple and transparent one. It implies a translation of experimental facts, models, theories, assumptions and contingencies into the universe of concerns, interests and values. It is a critical moment, with two tendencies operating : some actors or groups try to dress interests and strategies with scientific arguments for giving them the strength and authority of science ; others suspect any scientific result or assessment to shelter some hidden vested interests. Institutionalised expertise is one of the main channel through which this process of translation is achieved, and we will have to consider the roles they are given in this operation. This exercise can be exposed to various sorts of criticism. Questions are raised about the type of legitimacy experts groups can rightly mobilise or about the composition of such groups : who are the people being said experts ?

One typical and famous controversy about experts conclusions accused to hide political and economic interests is the severe controversy that arose between the Centre for Science and Environment (New Delhi) and the World Resources Institute (Washington DC) in the global warming case. Anil Agarwal and Sunita Narain (1991) attacked the results and methodology of the study realised by the latter, which notably gave figures for net emissions of greenhouse gases (GHGs) of each country on the basis of one implicit assumption, i.e. that the carrying capacity of the global environment as regards absorption of GHGs emissions is "naturally" allocated to countries on the basis of their present level of emissions. With other rules, more appealing to Third world countries, which allocate this global "service" to countries on a per capita basis, it is shown that the responsibility of developing countries may not be superior to 20 % instead of the nearly 50 % found in the WRI's study.

The importance of expertise in this phase should not be underestimated since some scholars (Haas, 1990) give a key role to the uprising of an international 'epistemic community'

for setting up a new regime of international co-ordination.<sup>4</sup> But in the context of controversial risks, there presumably are several epistemic communities in competition.

*A widespread feeling of urgency and a pressure for acting before knowing*

The potential gravity and irreversibility of imagined consequences of phenomena lead at least some experts, opinion groups or public and private managers to consider it necessary to act without waiting for scientists to fully elucidate the main questions. There is a widespread feeling of urgency and the sudden awareness that action needs to be taken before a sufficient knowledge has been reached. But in those cases, action cannot be rational in the classical substantive meaning, i.e. founded on "hard facts" (stabilised knowledge within the scientific community) and on precise cost-benefit analyses.

It is possible that decision-makers find some benefit to dress up the actual process of decision as if it were following a pure rational process. That sort of theatre is often what is expected from experts : to give an illusion of rationality and of scientific incontestability in order to maximise the authoritative value of decisions already taken.

The pressure of the feeling of urgency opens up the Pandora's box of temptations of manipulating science for the benefit of political and economic interests. Concerned social actors and big corporations significantly involved in R & D activities are spreading their strategic field of action and competition to the scientific representations of the world (nature, but also society). The content and timing of action is at stake : some want immediate action based on technological standards, in order to push new markets ; others try to postpone any action to preserve their own business,<sup>5</sup> and so on.<sup>6</sup> In the field of science, the purpose of this strategic action may be as well to obtain a premature and favourable closure of the controversy, as an artificially sustained state of controversy, since persisting uncertainty and doubt permit lobbying to continue. More often than not, actors are making an argument of the

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4.- An 'epistemic community' is understood to be a network or community of 'experts' sharing beliefs in causal relationships (even if these beliefs turn out to be wrong) and values about what public action should look like. It is supporting a 'vision of the world', and takes place at the interface of scientific community and social and political arenas. Such 'epistemic communities' operate as "translators" between science and social concerns in both directions. Not everybody agrees with the role given to "epistemic communities". Other scholars prefer to keep an interest-based explanation (Sprinz and Vaahoranta, 1994).

5.- In the ozone case, an Alliance for Responsible Atmospheric Policy was established in 1980 with the initial purpose to oppose regulation of CFCs and support scientists who disagreed with the new theory of F. Rowland and M. Molina first published in a *Nature's* article in June 1974.

6.- P. Roqueplo (1988), when comparing France and FRG as regards the forests dying-back issue, suggested the concept of 'reversed threat' to describe the fact that for French industrialists (car manufacturers, oil companies, coal industry), the threat was not coming from the forests disease, but from the decisions of the FRG's government and the European Community's Commission about car emissions, lead-free gasoline and SO<sub>2</sub> emissions from power plants. This does not mean that French industrialists were a priori more insensitive than the German ones to environmental considerations, but that the content of regulations was perceived as unduly favourable to the German technology and reflecting mainly German conditions and priorities.

state of uncertainty and controversies to postpone any policy change or new decision. So experts judgements tend to become the instruments of social actors' strategies, even if these experts do not take part consciously to this game. In the meanwhile, some scientists, as individuals or as interests groups, are accepting to dress with the new clothes of advocacy, with the hope of gaining some financing for their research<sup>7</sup> or simply for various ideological motives. Even if experts maintain their independence, their social credibility is generally affected, and any statement is from there on suspected to hide specific interests. Then, common people, but also official circles, are keen to believe in statements, not for their scientific value, unfortunately indiscernible, but according to the degree of convergence with their a priori attitudes.

*A competition game for rule-setting, which comes to an indirectly arbitration of scientific truth for economic and political motives*

In controversial contexts made of several competing explanations of basic phenomena of concern, actors have an opportunity to influence the ultimate course of public action by pushing the 'vision of the world' most suitable to their interests. On the winning 'vision of the world' will depend the possibilities of development of technologies, markets, and the ensuing comparative advantages that some firms may gain nationally or internationally. So there is a competition for the guidance of public action. Groups of experts have a key position in this game, mainly through their composition. This is not to say that every individual position can be reduced to narrowly defined economic interests ; but that legitimate economic and non-economic interests take part in the process resulting in setting-up the positions of experts.<sup>8</sup>

At one moment, this competition leads to some decisions and policies. The specific meaning of those decisions and policies deserves to be stressed. In targeting and reorganising specific economic fields (by changing incentives, or by defining new technological requirements for emissions of pollutants), the action of public authorities will come practically to an indirect arbitration of scientific controversies. Here, some 'vision of the world', regarding

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7.- Applying the interest-based explanation to the behaviour of scientists when they try to attract public attention onto some environmental risks is the basic argument of the work of S. Boehmer-Christiansen about the various cases she has been studying (acid rain, global warming) (1990, 1994a et b). About the issue of climate change, she writes for example in the abstract of her paper : "The primary interest of research is the creation of concern in order to demonstrate policy relevance and attract funding".

8.- For instance, many experts in groups created by public authorities are coming from industrial circles ; these persons, like those coming from other circles, combine specific competencies and a function of spokesmen of the circles to which they belong, i.e. they also defend specific industrial interests and diffuse the corresponding "visions of the world". In France, it has not been uncommon that the representatives of France in international negotiations about the protection of the ozone layer were in fact persons belonging to ATOCHEM, the industrial firm involved in the production of CFCs.

ecological phenomena and scenarios for future economic development, will be fixed by convention in laws, technical regulations, budgetary decisions and institutions.<sup>9</sup>

In the "Waldsterben" case in West Germany, the initial issue was made of a sudden discovery by media and public opinion that the German forests were 'dying' fast at an unprecedented scale in low-polluted regions. In a first stage, SO<sub>2</sub> was told to be the main cause and a growing popular discontent arose against industrial activities and thermal power units. But, in a second stage, the ozone hypothesis has been introduced by some scientists and gained quickly audience among political authorities. Early 1983, more severe regulations were enacted for emissions of thermal plants, and in July, the government released its first draft of a clean car regulation, implying the use of catalytic exhaust pipes. In both sectors, the Germany's strategy was to press the European Community to adopt similar rules in order to limit costs differences with competitors or, even better, turning potential additional costs into competitive advantages. To some extent, the government decided to share out the "Waldsterben" burden between the automobile sector and the energy one, which comes to having decided, among the various scientific theories, the one of the joint effect of acidity of soils and of the necrotic impact of ozone on leaves and needles.

*The eventual convergence of a dominant coalition on a conventional restraint of uncertainty built on available technologies and social acceptability*

If no coalition is able to definitely win, the competition contributes to extend the period of hesitation and instability of expectations between several possible alternative courses of actions. This is impairing the demand for predictability and contextual stability which characterises the industrial universe. There is a moment when the main actors, although still supporting conflicting views about what should be done, converge on a request to the government for a context stabilisation. This can be achieved through public regulatory action. Persisting uncertainty by which economic actors are suffering is then expected to be reduced by fixing social conventions and rules, even arbitrarily.

On the other side, before any decision is made, the government has its credibility and legitimacy put into question by the very existence of an unresolved problem. At one moment, it has to 'do something' in order to justify the trust of the citizens and its own claim to be in charge of public affairs. So supply and demand for public action are matching.

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9.- For example, some consensus has emerged from OECD experts groups about the standard of quality for underground water that should be adopted as a goal for management purpose : it should satisfy all the requirements of a drinking water. This specific convention, based on an human health perspective, was not self-evident : it will have important consequences in terms of developing costly programmes of pollution abatement from non-point sources (agriculture, municipal flows of surface water,...). An ecological viewpoint would have led to another approach insisting upon local conditions.

At this very moment, contextual stabilisation cannot be deduced from 'hard facts' or scientific consensus. For example, if we except the 80% rate of emissions reduction requested for stabilising greenhouse gases concentrations in the atmosphere,<sup>10</sup> any particular realistic objective of greenhouse gases emissions abatement has to be arbitrarily set as regards both the rate of abatement and the reference year to be adopted. Although the general acid rain case is much better documented, some basic arbitrary decisions are still inescapable : after a thorough scrutiny of the problem, David Newbery (1990) recognized not having been able to define the marginal damage cost functions in absolute terms and used relative proxies. In the same vein, Karl-Göran Mäler (1989) turned out the standard but heroic economic assumption of equalising the marginal damage and abatement costs into a means of deducing the damage cost from the present marginal abatement cost, with the justification that it reflects the current assessment of government preferences...

So contextual stabilisation is generally based on the more sound elements at hand in the situation : available technologies that have come to an operational stage and are supported by a network of actors and corporations having vested interests in their development. Since then, the social process is gaining autonomy as regards the future evolution of the scientific controversy. Groups of experts have a key role in this translation of a problem-identification issue, based on knowledge, into a practical question of selecting one action among a set of possible ones. Clearly, the sorts of expertise needed for these two tasks are not the same. Interestingly, specialists of technology, industrial affairs, economics, policy science or communication will be - or should be - asked to trace the frontier of reasonable action before natural scientists can really give a decisive picture of what the problems are and where they come from. Alternatively, when the issue of reasonable action is at stake, groups from which advice is looked for are not only experts, but various types of formal and informal spokesmen of stakeholders : on them depend the acceptability of actions.

Once institutionalised, compromises that have been reached appear to be reversible only with great difficulty, even if they do not fit to the future evolution of knowledge. This is not a physical type of irreversibility but one which is a socio-economic construct : if the new rules are perceived to constitute a credible commitment of authorities, investment strategies of all actors tend to be re-organised around them ; this fix-up of conventional decisions in equipment and infrastructures is an important factor of inertia and contributes to the self-maintaining of the rules and conventions they organise (Godard & Salles, 1991).

In the ozone case, it has been possible to phase out CFCs when industrial firms producing these chemicals were reasonably sure to be able to supply for substitutes. Among

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10.- But why should we give a normative value or imperative strength to a return to a pre-industrial level of GHGs atmospheric concentration ? It would mean that our societies will decide to give credit to an ideology of natural equilibrium which, as such, is quite different from the modern scientific vision of nature insisting upon disequilibrium and evolution.

them, HCFCs had a critical position : after having been said to be a solution, inducing technological investments to produce these new commodities and to adapt chain of uses downwards, HCFCs have been discovered to be part of the problem of ozone depletion in the same way as CFCs, though their lifetime is shorter. So phasing out HCFCs began to be discussed in June 1990 in the context of the London Amendment to the Montreal Protocol and resulted in the formulation of a goal of complete elimination of these products by 2040, such a deadline leaving enough time to amortise investments already made. Subsequent Amendment led to advance phase outs, but an agreement has been difficult to be achieved, because of the costs of this new change. Since the Copenhagen Amendment of November 1992, complete elimination is asked world-wide by 2030, and specific intermediate targets have been set up. In December 1994, the European Union finally reached a decision of phasing out HCFCs in members countries by 2015.

The timing dimension of the period preceding hard decisions is crucial for determining their content. If it is a short period, the ultimate decisions will confirm available 'classical' technologies and block the development of innovations which need more time to come to an operational stage. So there will be a future opportunity cost which may be important. This was the conclusion of the story of the speeding-up of regulations of car emissions in Europe : they practically forced catalytic exhaust pipes rather than giving time for innovations in engine combustion. On the contrary, if this period in which no hard decisions are taken may be kept for a sufficiently long time and this time-lag is made predictable in advance to industrialists by public authorities - i.e. through official announcement of a calendar of management of the issue -, industrial firms receive appropriate incentive to invest in specific programmes of R & D, which probably will increase the chance to get better technologies at the moment when hard decisions are made. But, under potential irreversibility of environmental changes, how long is it possible to postpone action ? So the decision problem is structured by the respective dynamics and time constants of natural processes, progress of scientific knowledge, technological innovation, capital investment, opinion movements and law changes.

*A policy answer failing to fit exactly to either the initial or the final terms of the question*

At the end of the pipe of this social process, it is quite possible that the actions taken and the rules defined have only an approximate link with the initial environmental alarms that triggered the whole process. The singular trajectory of the process depends on the random order of public appearance of scientific hypotheses put forward as regards the possible causation of natural phenomena behind environmental issues or health hazards, and on the nature of the fashionable hypothesis at the moment when the government decides to do something about it. This moment is not decided only for scientific motives, but also for public opinion or political ones. If the government waits for some more time, perhaps a new

hypothesis appears as the most plausible explanation, and the action of authorities has then to be different. Generally speaking, this paradoxical conclusion may become completely clear only "ex post", at the end of the most active phases of the process. But, this paradox may also be perceived during the process to some extent.

In the "Waldsterben" case, the initial phenomenon was coined with sudden awareness<sup>11</sup> and feeling of urgency. At the end of the pipe, some actions have been taken quite rapidly, but these policy measures had a time-schedule unable to respond to the ecological dimension of the urgency felt : the only action having potentially immediate, though limited effect, on the NO<sub>x</sub> emissions was to fix a severe restraint on the highway speed and to reduce automotive traffic. But this option has been dismissed, considered as unacceptable for car manufacturers and users. Today, the scientific explanation of the decline of forests is still not clear-cut and involves a complex set of factors, among which acid rain is just one among other factors. As an "ex post" scientific article (Frank, 1991) puts it : *"The assumption that air pollutants are primary causal or contributing factors is based mainly on correlations without rigorous proof of causality"*.<sup>12</sup> But nobody suggests to dismantle the existing set of rules for SO<sub>2</sub> and NO<sub>x</sub> emissions.

So the final answer is generally displaced as regards initial questions. For one part, it is because these questions have been reformulated into more relevant ones. For another part, because these questions could not be answered. But there is also a third aspect. The general challenge of policy-makers is not to resolve ecological problems or reduce health risks as such, but to manage political equilibria and achieve some political project. In this perspective, questions being raised operate as a trigger for a self-organising process of social readjustment. Public actions which are decided at one moment cannot really be seen as just solutions matching the problem as initially formulated. They address the political problems that environmental and health issues, as perceived by the public opinion and various sorts of actors, could generate. Through translations and instrumentalisation, this self-organisation process depends more on technological possibilities, institutional framing, and political and economic strategies than on a rational analysis of means to solve a well-identified ecological or health issue.

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11.- P. Roqueplo (1988) explains how, after the first moment of stupor, specialists and ordinary people have learnt to see the symptoms of forest decline in practical situations where they did not see anything before, shifting suddenly their stance from denial to overemphasise. In the ozone case, after the discovery of the Antarctic ozone hole, scientists committed themselves to a re-analysis of all the space data, and then came to extract from these data significant long-term downward trends in ozone levels they have not seen previously ; see K. Warr (1990).

12.- This author suggests that airborne halocarbons and secondary air pollutants are involved in forest decline through the action of derived phytotoxic compounds, as trichloroacetic acid (TCA).

## ***2. The roles of experts in tackling controversial risks***

If the precedent lines of description of the context typical of controversial risks are accepted, we may wonder which role is expected from groups of experts commissioned by governments or international bodies for tackling this type of risks. Part of this role is in fact common with any other types of situations of expertise. This feature often leads involved actors not to perceive the differences. Here lies precisely one dimension of the role taken by experts, maintaining the illusion that controversial risks can be treated in the same rational framework which is supposed to be the standard of public decision-making : a model of *"decision made by responsible public bodies guided by the general interest on the basis of a rigorous examination of facts and scientific evidence"*.

### **Constructing a problem relevant for public action**

As such, scientific discoveries and controversies are not enough to construct the idea that a problem does exist and that it needs public action. So, the first role of experts is to construct the problem or to confirm and validate one pre-existing construction of the problem. This means translating a continuously changing state of knowledge, within numerous disciplines, into issues involving the idea of potential damage done to some relevant beings and, at the same time, the idea of a damage having such a cultural, political or legal relevance that socially protected interest were at stake. Very often, experts are not approaching the framing of issues through a virgin context. Policy-makers, economic actors, NGOs, others, have generally begun to frame issues in some ways, usually not fitting together. In this most frequent case, experts have to test the proposed framing and elaborate their own, on the basis of authority of knowledge. Quite evidently, this task may be organised practically with some division of labour among several committees and councils. In some case, public authorities give the framing and experts are only asked to fill it up with data and facts. But the point is that, globally, this framing activity associated to expertise is not identical to the one in which scientists are involved in their basic professional activity, because it implies the meticulous establishment of a connection between facts, data, trends and some public concerns.

One example may be useful here. Why should we consider the disappearance of one species of spider to be a problem ? To answer this question, public authorities need not only researchers specialised in the study of spiders to give a detailed scientific picture of the ecology of arachnids, they also need some conceptual shaping relating the destiny of these animals to some social concern or value. The final result has to be expressed in terms of obligations, rights, damage, benefits, preservation of values, risks, and so on. To achieve this result, the case should be elaborated within one of the main rhetoric of legitimacy used in our

societies. For instance, from an industrial, productive, viewpoint generally interested by the exploitation of natural resources, a concern will be put forward about the possible future use of these animals in bioengineering and medicines and the related loss that will be suffered if the species disappears ; a market-oriented economist may stress that this type of spider, because of some specific feature, has a market value for collectors or as an ingredient in cooking (!) ; a conservationist ecologist will have to explain how this species is connected to more general features of the functions of ecosystems on which man depends, and is part of the natural heritage the present generation has the obligation to keep for future generations. And so on.

We can observe that in the recent period, the class of relevant beings has been progressively extended to non human beings, as it is shown by the evolution of the meaning of the expression "ecological damage". Some years ago, "ecological damage" only meant damage done to some human persons or their property assets through an ecological channel. Now it frequently embraces a notion of a damage done to natural equilibria or ecosystems, without direct reference to human rights or properties.<sup>13</sup> Experts coming from natural sciences have played an important role to diffuse this concept in legal practices. But in so doing, they were not acting as pure scientists, people mastering some piece of knowledge, but as social engineers. Which legitimacy is theirs to do so ? Not the one which is the official justification of their role.

This is one typical result about the role of experts : what they are doing goes beyond the official justification of bringing facts and knowledge in order to enlighten an issue. Through repeated uses of language, a community of experts diffuses into law, administrative circles, and the media a specific semantic in which some normative "vision of the world and society" is embedded. In the case of "ecological damage", we may be soon embarked in a situation in which, on the one hand, regarding explicit philosophical options, statements supported by deep ecology are not generally accepted in most European countries, but on the other hand, our legal conceptions begin to be conceptually adjusted to such statements.

### **Constructing a bundle of alternative schemes of action**

Saying there is a problem is one thing. But experts have to go further. They have to say how the problem can be tackled, and to come to a judgement on the practical feasibility of possible schemes of action. If a problem stands, but with people having no idea of possible

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13.- For instance, the Lugano's Convention adopted by the Council of Europe (1993), dealing with rules of civil responsibility as regards environmental damage, establishes a distinction among damage done respectively to human beings, the environment, and goods. In that context "the environment" includes biotic and a-biotic natural resources, goods being piece of cultural inheritance, and typical features of landscape. On this, see G. Martin (1994).

solutions, it can be said no more to be a problem. It should be called a catastrophe, a calamity, a drama, a fate... In the latter cases, the type of expected public behaviour cannot be described in the same way : the issue is just about helping or compensating victims, or delivering symbolic rituals to give a cultural meaning to unavoidable courses of events. So, this is one important task of experts to say if one emerging phenomenon is to be considered as a problem on which action may have significant effects, or if it is beyond any influence of public action. In controversial context, this is not a simple job, as we already noticed about the very debated global environmental changes and risks : are they natural or man-made ?

If the problem-characterisation is confirmed by experts, the latter have to draw the bundle of possible actions. In fact, for so doing, experts tend to concentrate on parts of the issue about which causal links can be reasonably defined. In that case, the problem tend to be reformulated around some of its more solid segments. For example, since we do not know for sure that GHGs emissions are provoking a major climate change, attention will be devoted firstly to the link between technological and economic activities and the level of GHGs emissions, and secondly between GHGs emissions and GHGs atmospheric concentrations. Though complex, this relation is less evanescent than the one between levels of GHGs atmospheric concentrations and climate changes. So the initial problem "mitigating climate change so as to prevent a dramatic change" has been translated into "how can we stabilise GHGs atmospheric concentrations at some level ?". There is clearly a missing link between the two statements, but in spite of many efforts, scientists can do no more that give the uncertain, still controversial and contingent, results of modelling efforts based on numerous restrictive assumptions. So the gap has to be filled, but this cannot be done on a pure scientific basis.<sup>14</sup> As regards economic models, we observed the same process of reduction/translation. According basic economic theory, we should optimise GHGs emissions so as to minimise the total social cost (adaptation cost plus residual environmental damage cost plus mitigation emission abatement cost) for all the relevant time period. In fact, with a few, very controversial,<sup>15</sup> exceptions (Nordhaus, 1991), most economic exercises have been limited to

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14. Interestingly, W. Nordhaus (1994) has recently organised an opinion survey among 22 recognised experts being professionally active into national or international assessment of global climate change. This poll included social, mainly economists, and natural scientists. Asked about the magnitude and probability of loss resulting from climate change as a percentage of Gross World Product (GWP) in three scenarios (3-degree-Celsius rise by 2090, 6-degree rise by 2175, 6-degree rise by 2090) results show an enormous dispersion of estimations : for the less catastrophic scenario, the range goes from 0 to 21% of GWP; it is much larger in other scenarios. This dispersion particularly reflects disciplinary differences. Natural scientists estimates of damage are much more important than those of economists. One explanation quoted in the paper is that "the economists know little about the intricate web of natural ecosystems, whereas scientists know equally little about the incredible adaptability of human economies." (p. 48).

15.- See the recent survey and discussion by P. Ekins (1995). Assumptions regarding revenue recycling, existing price distortions, dynamic effects on technical progress, secondary benefits, and so on, disregarded in modelling exercises which conclude to a high level of abatement cost, may change the whole landscape, even possibly turning the global warming policies into an economically profitable action, even with high levels of carbon taxation !

an assessment of the economic cost of achieving a given level of GHGs emission abatement or of achieving this result through alternative means (Manne and Richels, 1991, 1993 ; Godard, 1993a). These operations are reflected in the way the Rio Convention on climate change has been framed, since the stated imperative of containing climate change to a pace compatible with evolution of ecosystems and preservation of natural resources has been translated into an imperative to stabilise GHGs emissions at their 1990 level.

The task of structuring possible action also has an impact on the composition and status of groups of experts. Since public action is at stake, public authorities want to keep some control on the process. In the climate change case, governments having expressed their relative distrust towards independent self-organised groups of expertise, who have been responsible for putting the issue on the international public agenda in the eighties, they decided to create a structure of expertise they could control in connection with the UN system (Boehmer-Christiansen, 1994a & b) : here came the Intergovernmental Panel on Climate Change in 1988. IPCC was mandated to achieve a survey of the knowledge about basic climatology (WG1), potential impacts of climate change (WG2), assessment of response strategies (WG3). This panel was supposed to mobilise the authority of science and facilitate the emergence of a consensus on the directions to take. Its credibility highly depended on the large opening of cross-examination processes to numerous scientists world-wide, and on the fame and authority of the main scientists in charge of its management. At the same time, as an intergovernmental body, IPCC was submitted to some political logic : proposed members were designated by governments ; a concern had to be expressed for an equilibrated representation of each region of the world and for opening meeting sessions to some active NGOs, and so on. In practice, all this resulted in a mixing of very high-level scientists, some more modest ones, diplomats with scientific background, civil servants belonging to national administrations, and NGOs' advocacy. It is the functions and results of such a hybrid assembly we are going to consider in the following.

### **Indirect testing of respective positions of social actors**

Groups of experts, specially in international contexts, give the opportunity to various social actors, mainly governments, to have a first approach of the position of others, with the aim to try and see what can be the borderline between what will be acceptable and unacceptable for the other parties. This expectation has an influence on the organisation of the work of experts. According to common sense, the allocation of work should be determined by skills and competencies. But this is not the only logic we can observe in such groups. One example will illustrate the process : in the first phase of activity of WG3 of IPCC, it has been asked to Saudi Arabia representatives to prepare the report on "Impacts of industrial countries policies on the world economy". The reason of this selection was not that this country had

specific scientific skills fitting to the subject. The intent of IPCC organisers was to avoid its defection and the opening of a direct conflict. The issue of introducing a carbon tax as a main policy instrument being very controversial to many regards (Godard, 1992c, 1993a ; Liberatore, 1995 ; Ekins, 1995), to give Arabia the responsibility of an assessment of the international economic impacts of such a measure was a means to see how far oil-producing countries are ready to go in their opposition to any policy changing significantly energy prices world-wide. Not surprisingly, the report submitted in 1992 by this country shows how much damaging a carbon tax will be for the economies firstly of industrial countries, secondly of oil-producing countries and thirdly, due to points one and two, of developing countries : the tax is supposed to trigger an economic crisis in the North ; this would mean less outlet for Third World exports ; diminished revenues returned to oil-producers would mean cuttings in development assistance programmes financed by these countries. The message was clear : as opponent to any action on energy prices oil-producer countries present themselves as the best spokesmen of all the developing world. But one interesting point to observe is the following : by accepting to play the game of expertise, one actor - in our example, Saudi Arabia - accepted the risks of having to argue the positions taken with all the equipment (modelling, statistics,...) of economics, and to be exposed to some criticism showing some deficiencies or inappropriate methods or ill-based results. This is the reason why we have to consider the functioning of expertise within its internal dynamics of evolution. By chance, because of the rules of the game of mutual critical assessment, the logic of interests cannot be the only one to impose its logic !

Using groups of experts to test the acceptability of options is not an easy task for at least two reasons : a) in most cases, this borderline is not well-defined *ex ante*,<sup>16</sup> but will result as an end-product of the process of expertise and negotiations organised around it ; b) each party wants to know more about the others but does not necessarily want its ultimate positions being made transparent to others too early. So we have to expect some dissimulation and tactics in this game of expertise, as regards exchange of information. The picture that some parties may think to have obtained about others may be ultimately revealed quite wrong when the expertise phase will have given the way to direct political negotiations. This is the

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16.- This relative indeterminacy of interests is a critical feature of the context of controversial risks. Because several alternative possibilities exist about basic facts and causal relationships, due to this state of knowledge, actors are not able to define and optimise their own interests as regards the specific issue and consequently to defend them in negotiations. Consequently the scene is invaded by what we called "secondary interests" and strategic actions connected to other types of issues, which also involve conflicts and negotiations for rule-setting : for instance, achieving a structural change of the pattern of relationship between the North and the South countries, improving economic competitiveness of some industrial sectors, upgrading one's rank in diplomatic influence, etc. On this, see Godard (1992c).

political advantage of an approach of international co-ordination through scientific expertise : it preserves the political autonomy of governments and avoids premature commitment.<sup>17</sup>

### **Preparing an imputation of responsibility**

An important part of the activity of experts is devoted, not to explore and synthesise results of basic research, but to look for and organise data needed to prepare operations of imputation of liabilities supposed to be involved in the emergence of a problem. For example, much effort is being done for achieving inventories according to standardised methodologies. In the climate change issue, most critical data in the literature take the form of present and possible emissions of GHGs per country and, for each country, per inhabitant. These most simple ratios (one American citizen emits 6 ton of carbon yearly and one French 1,8 ton only) are easy to sell to the public opinion and serve as implicit normative benchmarks for imputing responsibility, before any explicit political or legal judgement is made. They have clearly no scientific interest, adding nothing to the understanding of basic processes of climatic changes.

This pre-framing of responsibilities is very ambiguous. It fits to a period where fixing new legal rules is at stake. So this treatment of information tends to structure the points on which new rules are going to be defined. As such, it contributes to enhance the process. At the same time, it is not sure that this pre-judgement of liabilities responds to validated principles of justice. There is a chance that some standard, having all appearances of fairness and justice, but only these appearances, will gain an attractive strength because of its general diffusion among experts, social actors and public opinion.<sup>18</sup>

### **Structuring data so as to authorise a rationalisation of decision-making**

Rational decision-making, whatever meaning we give to this expression, involves making comparisons (of factors, impacts, options), defining equivalence relationships, setting hierarchies (of factors and problems), and producing assessments according to some scales of value. These operations need specific operators and formats to shape data into a useful form. To this regard, issues of defining appropriate indicators and methodological standards are

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17.- One typical example coming from the global warming case is the following : although US experts have repeatedly expressed a strong preference for an international tradable permits system for carbon emissions and sinks at IPCC and OECD expert meetings devoted to the climate change issue, the US government provoked a big surprise in 1991 when, during the negotiations developed within the Intergovernmental Negotiating Committee of the Framework Convention on Climate Change, it dismissed this approach as completely unrealistic.

18.- We have tried in another article (Godard, 1992a) to show why none of the main criteria proposed to achieve an initial international allocation of permits to emit GHGs (per capita, GNP, present emissions) can be said unambiguously to be fair and just. They belong to different justification orders, each criterion expressing at the same time what is just and realistic within one of these orders. But there are no general agreement on which order should take precedence to arbitrate the climate case.

quite ordinary ones. All these operations have to be achieved or validated by groups of experts. But, in uncertain and controversial contexts, this necessary step is hard to achieve and is a moment of subtle interference of social or ideological inputs within the process of producing what is said to be hard and objective facts and data.

So, in the climate change case, two steps were required to establish a perfect rational framework for decision-making : a) to find a common unit for measuring all the costs ; b) to find a general index of equivalence between all types of actions as regards their incidence on warming. With these two elements, it would be possible to set a complete classification of mitigation and adaptation strategies and to optimise the choice of one of them on the basis of a cost-effectiveness ratio. This programme is not at hand from the very beginning.

A first step that IPCC has tried to make in that direction is to elaborate one index setting equivalencies between the various GHGs (CO<sub>2</sub>, CH<sub>4</sub>, CFCs, N<sub>2</sub>O, ...). In its 1990's report, IPCC gives such an index for all the relevant substances, the Global Warming Potential Index (GWPI). To the extent that all gases do not have the same lifetime, these equivalencies have been set for two time-periods : 20 years and a 100 years. These values have been used for numerous calculations made by experts, official bodies and academic researchers to establish a classification of emission sources and possible actions, and to give a picture of respective responsibilities of countries. As a matter of fact, this index has been exposed to hard criticism which reveals the gap between the real state of knowledge and the type of information that should be provided to authorise decision-makers to be fully rational.

There were many sources of difficulties with this index :

- important margins of error due to the fact that indirect effects were voluntarily not taken into account because of deficiencies of basic knowledge ;
- a mutual dependence of the values that should be included for one gas on the concentration of others, which render an Index practically intractable since each unit value should be substituted by a multi-factorial function or a vector ;
- an instability of the values for each gas as regards the progress of the understanding of the climate machinery.

Let us give an example for the last point. In the 1990's report, CFCs were classed as the gas having the highest potential (7.000 times more for CFC12 than for CO<sub>2</sub>). Two years after, the integration of indirect effects of CFCs, including destruction of stratospheric ozone,<sup>19</sup> has led to a cut of their estimated net potential impact by 80% (Wigley & Raper, 1992).

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19.- If the ozone layer has a useful role in protecting the earth against solar radiation, it is at the same time contributing to the greenhouse effect. If CFCs are powerful GHGs, their impact is reduced through the destruction of ozone they achieve.

The final word of this story has been a situation of emerging distrust of social actors against scientists ; scientists have felt this result as particularly frustrating since they committed themselves to elaborate this Index with much reluctance, only to answer a demand from policy-makers. For many scientists, this Index was not "scientific" at all, because of all the holes and shortcomings in the basic understanding of climate that were artificially veiled and because it has not got any scientific use.

In spite of the fact that IPCC said, in its updating of 1992, that it refuses to take into account indirect effects in its new values and that 1990 values were suffering from a substantial margin of error, subsequent proposals to organise an international regime of regulation of GHGs still continue to refer to the GWPI as a basis of calculations, particularly in proposals to establish an international tradable permits system or its preliminary stage, "joint implementation".<sup>20</sup> To some extent, this persistence proves the critical benefits expected from simple conventions as co-ordinating devices, even if these conventions are perceived as somewhat arbitrary. But it is understandable that all parties will not agree easily on these conventions shown to be arbitrary but having important distributive consequences.<sup>21</sup>

### **Artificially solidifying an uncertain knowledge through inter-disciplinary and multi-circles dissemination**

Groups of experts have generally a pluridisciplinary composition and experts have various professional origins (university, industrial firms, administration, ONGs). Such a mixture is inescapable as regards environmental and health risks which are transversal to the capabilities of disciplines and the types of knowledge to mobilise. But this very fact has a singular unexpected consequence, that of artificially solidifying pieces of knowledge : unless he is in a systematic critical mood,<sup>22</sup> the more an expert is distant from one speciality, the more he tends to consider statements and results coming from this distant speciality as a solid fact, whereas a member of the speciality generally acknowledges all the qualifications that should be added to set the exact validity of a statement and all the doubts and uncertainties which persist. As a result, as Brian Wynne (1996) put it : *"The doubts and uncertainties of core specialists are diminished by the overlaps and interpenetrations with adjacent disciplines. Given that all technologies and environmental policies require knowledge to be*

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20.- See for example the proposal of Tom Jones, from the OECD's Environment Directorate, at the International Conference organised by OECD and IEA on the economics of climate change (Jones, 1994).

21.- The already mentioned controversy between Indian researchers of CSE and fellows of WRI about calculations of net emissions per country is typical of this difficulty. As regards IPCC, see also J.K. Parikh's discussion of the ingredients of the reference scenario for calculating future emissions (1992) and S. Boehmer-Christiansen (1994a & b) for the whole process.

22 .- This may happen when specialists from one discipline are distrustful as regards the scientific value of any contribution from some other disciplines ; rightly or wrongly, ecological sciences often suffered from such a systematic distrust.

*synthesised from several or many more specialities, the net result is a more secure collective belief in the policy knowledge, or the technology, than one might have obtained from any one of the separate contributing specialities".*

This mechanism can be ascertained by the reading of much economics or political science literature about climate change. Though uncertainties may be acknowledged and stressed in some cases,<sup>23</sup> some basic structures or features are often taken for granted and well-founded to a higher degree by adjacent scientists or non-scientists having a scientific background than by people belonging to the speciality. Many illustrations may be given. One evident process of this type has been the focus on CO<sub>2</sub> in most part of economic literature as if it was the unique GHG of interest. This reduction of issues has been facilitated by the GWPI giving equivalence rates between various GHGs in terms of CO<sub>2</sub>-equivalent. But it may have some side effects when talking about the actions to be taken : a) the artefact of talking uniquely of CO<sub>2</sub> may induce an excessive focus of action on this gas ; b) it may give an excessive credit to the idea that the global warming case is a pure global one, i.e. regional climate changes will be insensitive to the location of emission sources.<sup>24</sup> I will give two more representative examples.

The first one is the beginning of a recent paper by Edward Parson and Owen Greene (1995) about the ozone layer : *"In the mid-1970s, atmospheric scientists discovered that the stratospheric ozone layer, which protects the earth from a certain type of ultraviolet radiation that is harmful to life, was being depleted by chemicals introduced into the atmosphere by human activities. (...) In response to the threat to the ozone layer, countries around the world have entered into agreements to phase out the use of harmful substances"*. In fact, the real story was not that one. The initial event was not a discovery, which implies some empirical test, but the publication of a theoretical model by Rowland and Molina in 1974. Regulation has been prepared in the USA well before any practical evidence confirm the connection between CFCs and variations of the ozone layer (Wuttke, 1994). The Montreal Protocol, this major international agreement, has been signed a few weeks before, and not after, a first empirical confirmation has been obtained in the Antarctic zone (Warr, 1990). Still today, although CFCs' phasing out has been advanced twice, the connection between the Antarctic situation and the evolution of the ozone layer world-wide is not clear.

Another example can be taken from the field of economics. Two American economists, Alan Manne and Richard Richels, whose work is supported by the Electric Power

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23.- See the expressive titles of articles by L. Lave (1991) : "Formulating greenhouse policies in a sea of uncertainty" or E.W. Colglazier (1991) : "Scientific uncertainties, public policy, and global warming : how sure is sure enough ?"

24 .- The discovery of the role of aerosols delivers another picture, since aerosols are regionally stable within the atmosphere : it gives a critical influence to the location of emissions. The future relevance of international joint implementation or schemes of tradable permits should have to be reassessed.

Research Institute (EPRI), have had a prominent influence on the international expertise scene about the global warming case. They specifically have been told to have influenced the US government's position against premature action, because of the high economic cost of any serious action on carbon sources in the USA, but favouring big investment in research. In February 1992, they released a draft paper analysing the EC proposal for introducing a combined carbon and energy tax<sup>25</sup> and its implications for the USA if the US government comes to use the same sort of instrument. This draft has been immediately echoed in the press, notably in the New-York Times. After the draft has been submitted to peer review, it has been published in January 1993 into the academic journal *Energy Policy* published by Butterworth-Heinemann. We suggest to look at these three steps with some detail.

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

Using their Global 2100 model, these authors came to several conclusions in this draft, the two major ones being that the EC proposal may not be sufficient to accomplish the stated objective of reducing emissions below current levels, and that a tax on nuclear 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 USA and EC economies and conclude that the cost for the USA economy will be 50 % higher in GDP percentage than the cost for the EC economy, because Western Europe uses approximately 40 % less energy per unit of economic output. They show the Figure 9 reproduced below 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 dated February 26, 1992, Peter Passell echoed the criticism against an energy tax, as *"adding cost while actually reducing the net impact"*, but also 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*

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25.- For a political science analysis of this EC project of combined energy and carbon tax, see A. Liberatore (1995). For a synthesis of its possible economic impact and use in France, see O. Godard & O. Beaumais (1994).

*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, a statement which is not the one of the authors, but is not contradictory to what they say, and seems to respond to a largely shared belief in the USA.*

*Third step : the academic release in Energy Policy*

When the Manne and Richels' paper has been published in 1993, it demonstrated no significant difference with the draft, but for one section : the question of the relative cost to EC and USA. An entirely new argument has been introduced about the calculation of dead-weight loss cost of a new tax when existing prices of commodities are already taxed. This point was ignored in the model simulations whose results were given in the draft. This addition resulted from peer discussions and notably from results of papers by OECD economists Peter Hoeller and Jonathan Coppel (1992). Without mentioning their previous results already disseminated, Manne and Richels are now explaining 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"* (p. 9 & 10). They show what is now Figure 10 reproduced below. The two figures depict a completely opposite reality as regards which region would bear the highest cost.

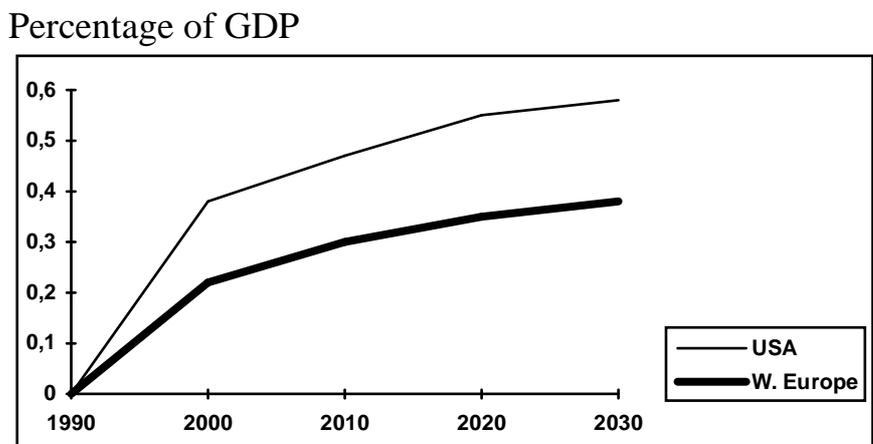
The storytelling could be continued, because this issue of differences in existing taxation schemes and assessment of dead-weight losses has been intensively discussed since 1992 ;<sup>26</sup> this discussion gave birth to a new debate about possible fiscal reforms ensuring "revenue neutrality" and alleviating the most distortionary taxes or charges, such as those based on employment : 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 to the specific carbon tax (Godard, 1993a).

Other cases of this sort have been documented in other fields, showing the possible drawbacks produced by a confusion between reality and results obtained from scientific models. Brian Wynne (1992) has shown for instance how, in the nuclear case in Great Britain, expert groups have stated very wrong predictions about the time-period needed to obtain a stabilisation of radiocaesium in the ground in some areas (three weeks estimated ; more than six year maintained under ban in fact), because of their application of one model of soil behaviour to a type of soil which were not of the same type as the original case.

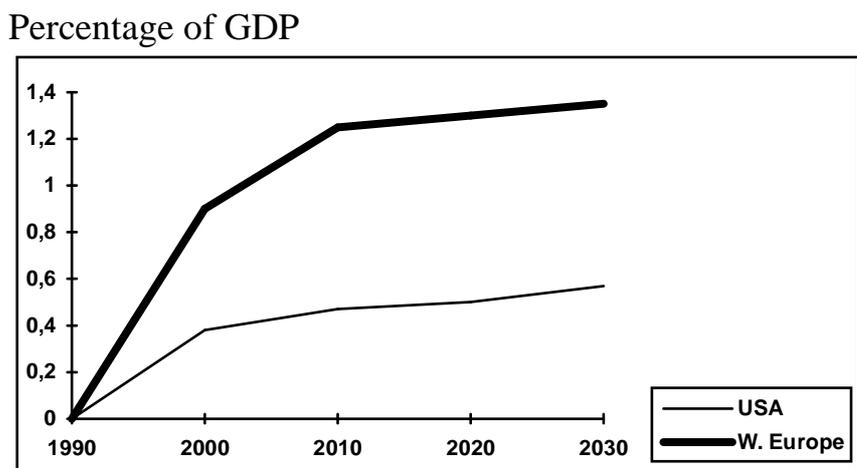
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26.- See the review of P. Ekins (1995).

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



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



We may get some lessons from these sorts of cases :

- Specialised scientists do know that their results are contingent results from models, depend on "*a handful of critical assumptions*", and are not a direct expression of reality. So, it is quite evident for them that changing assumptions and adding new variables will change the results. They do not see in this change of results a threat for their credibility, inasmuch this is not just a change in "opinion".
- When placed in 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 this way, what they write and disseminate is interpreted as such. All the

qualifications about assumptions and specific methods are forgot. So, the circulation of scientific results in circles larger than the original speciality generates an ossification of contingent statements into illusory "hard facts". When some scientific statements are in accordance with pre-existing beliefs, they are still more easily selected, by non-specialists, as true and direct expressions of reality .

- The value of collective expertise should not mainly be placed in the quality of specific results at one moment, but in the open process of informed debate among specialists, with feedback to research. Such a process is itself very 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 give a good framing to a fuzzy set of issues is what groups of experts and open debates can best do. All this needs some time - several years - and may be contradictory to the want of policy-makers to get clear-cut advice within a short period.

These lessons give us a good transition with the next section, since they may be seen as the basis of some precautionary rules to manage expertise.

### ***3. On the Precautionary Principle***

What is called the Precautionary Principle is the idea<sup>27</sup> that taking regulatory measures to prevent some possible risks may be legitimate even when strong scientific evidence on causal relationships or the extent of the damage is missing. Nowadays, this Principle is often said to be the solid foundation on which to base decision-making under uncertainty. For example James Cameron and Will Wade-Gery (1995, p. 95) state that : "*The precautionary principle provides the philosophical authority to take decisions in the face of uncertainty*" and see it as a "*general principle of international environmental law*".<sup>28</sup> No doubt that, at first sight, this principle may be viewed as an interesting advancement, bringing some practical responses appropriate to the context of scientific uncertainty and controversy we are considering here. But analysis is required before reaching definite conclusions. In particular, it is useful to think about the new course it may introduce in the relationship between science and decision.

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27.- There is no unique definition of the Precautionary Principle. This is the reason why we refer just to "the idea".

28.- We have been told by lawyers that a specific feature of international law is that it is not framed by general principles but by a network of specific conventions... General principles only belong to soft law.

## **A paradoxical position as regards science**

The precautionary principle is just placed at the articulation of a logic of extension of rational knowledge, and of an institutionalisation of the acknowledgement of the possible intrinsic limitation of scientific knowledge to provide for the appropriate information in due time. This is not an easy position. Still more science in one side, but a distance, if not a distrust, introduced regarding its results ! This position leads to contradictory requirements.

On one side, dependence on science is maintained. Implementing the Principle entails a minimum scientific qualification of risks : the precautionary approach can do nothing for risks that are not even represented. To be credible, this minimum representation should pass the test of scientific research, either to validate the possibility of the risks, or because the very initial idea about the risks is a result of a scientific programme.

Scientists are also asked to develop new knowledge about possible risks, with an exploration of numerous virtual possibilities. As a matter of fact, one of the new obligations generated by the Principle is an obligation for decision-makers to search information and do additional research about potential risks that have been identified already.

On the other side, scientists are asked to raise their self-consciousness about the limitations of their scientific constructs and to open their professional activity to a wider social criticism.

Moreover, the Principle introduces a distance between the decision-making process and scientific results and proofs : it is no more required to wait for the latter before deciding what and when to do. This change is sometimes interpreted as a means to restore the primacy of a political framing of issues and political assessment of the opportunity to take action (Cameron and Wade-Gery, 1995). We can accept this statement only to some extent, because of the new social power given to the scientific community : scientists are going to be given the power to influence social games and rule-setting processes through the apparently benign activity of producing assumptions and imagining virtual worlds.

If, as it is suggested by analysts coming from the insurance circles,<sup>29</sup> the Precautionary Principle should come to a restoration of fault as a basis for civil responsibility,<sup>30</sup> this new power will have counterparts for scientists in terms of new responsibilities. In the past, scientists were responsible before their colleagues for their results, i.e. to claim having made a discovery or a new enhancement, a scientist had, and still has, to bring the proof and exposes

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29. On this, see the argument of F. Ewald (1993) in favour of a strict distinction between "normal risks" and "development risk" and his plea for exonerating "development risk" from any civil responsibility. For an opposite viewpoint, attached to a general implementation of the principle of "objective responsibility", see G. Martin (1996).

30.- Responsibility will be involved if it can be shown than the party at the origin of a materialised damage has not taken the precautionary measures that should have been taken on the basis of the current state of knowledge (including conjectures and unproved causal chains), and will not be in the opposite case.

his activity to the judgement of other scientists. Now, by formulating statements about new uncertain possibilities, scientists may alter industrial activities, through triggering new regulatory constraints, and more generally change the balance of costs and benefits of many technological options. This may have two types of effects :

- To exonerate their own responsibility towards society, scientists will tend to over multiply the identification of possibilities which may be the outcome of a situation or a project, leading to a mass of information that will be very difficult to organise and to use. As a consequence, there will be a profusion of risks of all sorts that decision-makers should take into account. At the same time, scientists will be much more cautious before saying something is "certain". The most usual response they will give is : that's possible but we are not sure. In particular they will accept with difficulty to say that a substance or a technology are certified to be safe and will absolutely create no damage to human health or the environment.<sup>31</sup> They will add : "on the basis of current knowledge".
- Through the process of adoption of assumptions and definition of programmes of research, current scientific activities are pointing new possible risks. This very fact will presumably have direct consequences in the economic sphere. So economic agents may begin to attack the responsibility of scientists when ex-post assessment indicates they have expressed an inadequate judgement - for instance, when a substance has been told to be possibly toxic, then provoking a regulation, in a case where this possibility turned out not to be confirmed.

To some extent, courts are going to be made judges of what "the current state of knowledge" is, and will interfere with the scientific process.<sup>32</sup> One thing appears quite sure : if such processes are developing, it will become more and more difficult to be a researcher !

This new power of "making the games" which may be given to scientific activities and statements is much more vulnerable to attempts of strategic instrumentalisation by various types of actors than the usual scientific results. This is so, because only results can be placed under the systematic methodological scrutiny of the scientific community (classically, results have to be shown reproducible, for instance), and assumptions cannot follow the same way : to plainly validate an assumption is to get a certified result ! In controversial contexts, we have seen that various social interests tried to displace their social competition into the field of scientific constructions of the world, in order to shape the vision of the world that will be

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31.- We have to recall that for years, CFCs were considered as the best example of an environmentally-friendly product, because of its stability.

32 .- A recent case in Greece illustrates the point : judicial authorities of this country have launched a criminal inquiry against the Public Body for Seismic Protection for not having taken into account the alarm released by a university team of physicists about a coming earthquake. As a matter of fact, such an earthquake did happen in June 1995, with several persons being killed or injured. But the methodology sustaining the forecast was still highly controversial among specialised scientists (Kuntz, 1995).

socially disseminated. This process will tend, in the future, to be facilitated, and not reduced, by the Precautionary Principle, unless precise interpretative rules are set to give it appropriate procedural delimitation.

Though required they may be to define benchmarks and guidelines, such procedural rules will necessary adjust the social process of decision-making to the scientific life. Specifically, it will be necessary to define various degrees of precautionary response according to various degrees of seriousness of the evidence showing there could be risks. All this will reinforce the dependence of society on scientific judgements and on the internal functioning of the scientific community. The possible counteraction is that more and more citizens take an eye on scientific life.

It is difficult to predict what will be the balance between the two tendencies, contained in the Precautionary Principle, one of reinforcing the dependence of collective decision-making on science, the other of marking a distrust leading to a limitation of science to a minor role. We may imagine some rocking movement of society between these positions. We may also imagine some entangled situation, having both at the same time : science imposing its rhythm and agenda to society, identifying targets or scapegoats, and society being caught by a general distrust towards any scientific positive statement, but giving credit to all the threats. It is not sure that such a realm of fear will be a safe society.

### **Shifting the burden of proof : the missing content of the Precautionary Principle**

To many observers (Cameron & Wade-Gery, 1995 ; Garcia, 1994 ; Wynne, 1992, 1996), one of the practical incidence of the Principle is to shift or revert the burden of proof : it will be no more the people complaining about damage that will have to bring the proof before a regulation is taken, but those who propose a new project or a new technology should have to bring the proof that there will be no damage, before they can be authorised. This seems to be the best possible principle in the best world ! Reality is not that simple. We will advance three points converging to the practical unfeasibility of a straight application of this reversion of the burden of proof.

As Brian Wynne (1992, 1996) rightly points, if the Precautionary Principle only means a shifting in the burden of proof, it will continue to support the same old ideology of positivist science in which science is credited with the capacity to reduce any possible uncertainty through more and more research. On the contrary, Wynne proposes to look at part of the so-called uncertainty as a basic indeterminacy of risks and of scientific statements related to them. If the Principle is adhered to without any afterthought or strategic aim of instrumentalisation, reverting the burden of proof can be said to be a self-contradictory component : we have seen that the Principle is the expression of a new consciousness of the

limitations of science, as regards many issues, in bringing decisive results that will have a predictive value. But, if this position is to be taken seriously, it should also be realised that, if science is not able to proof the causal relationship leading to a possible damage, symmetrically it may also be unable to divert the possibility of its existence. In other terms, requiring the proof of the absence of damage is asking for something science is not able to give in the controversial context we are considering.

If this requirement is supposed to be applied literally, it can only lead to a general prohibition of most significant activities. The very high cost that may result from this position will be judged unacceptable by the population if information on these costs is made available to them. So, presumably, it is through the untold dictatorship of small groups that this extreme interpretation of the Principle could impose its realm. As a general phenomenon, its probability is rather low, but it is not improbable that, in some cases in which the public opinion does not perceive the costs of blocking development and there are some very motivated groups of interests investing this cause, public authorities may prefer to give up in order to avoid conflicts.

So, asking for the proof of harmlessness cannot be but a rhetorical argument in a political game, an argument that will be used depending on the political projects and fights.<sup>33</sup> This is this way than we can say the Principle restores a political logic on a scientific one.

The second point concerns the idea of "worse-case planning". This idea will be difficult to implement, because of practical deficiencies. The definition of the "worse-case" is depending of the imagination of scenario-builders. It is an open-ended exercise which has to be stopped by some extra-scientific conventions, and quite materialistically, through the effect of economic constraints (cost of modelling, of field work, and so on). Even if this first obstacle could be overcome, a second one is challenging : as regards controversial risks on which no scientific consensus exists, the worse case scenario of the various options tend to be equally catastrophic or negative. So, to refer to the worse-case as a benchmark is not enough discriminating to be useful for decision-making. This practical deficiency can only be avoided if some extra-scientific criterion defines the boundaries of the scenarios to consider or if an implicit asymmetry is accepted in the treatment of options.<sup>34</sup>

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33.- One example extracted from the recent controversy about the French nuclear underground experiments is offered by Jim Bolger, Prime Minister of New Zealand, in an article published in *Le Monde*, in September 1995. He argues that "*La région court le risque d'être atteinte en cas d'accident, ce que personne ne peut catégoriquement exclure, quelles que soient les précautions prises. Aucun scientifique n'a jamais démontré d'une façon convaincante qu'il n'y aura aucune fuite radioactive en mer.*" This is quite true : a scientist will never be able to offer such a guarantee, in that case as in numerous other cases. But, so what ? Where is the threshold of acceptable risks ? Zero risk in any case ?

34.- A key issue to this regards is the process through which the public attention is focusing on one issue (Amazonia, ozone layer,...), forgetting other types of risks which rationally deserve the same sort of attention.

The third point continues with the same idea. Generally speaking, the framing of choices is not one risky option against a safe one ; more than often, it is a risky option against one another. We have to deal with contexts with all-pervasive risks, including economic and political risks. This is not to say that one risk is equivalent to any other one, but to stress that the general function of public policy-makers is to arbitrate between risks just as they have to arbitrate between various types of public interests. It will not be an easy task to set an order on all these risks and, since they belong to different parts of reality placed under scientific controversies, this ordering will not ultimately result from a scientific judgement but from a political one. What is called the general interest is the result of such an arbitration process. It is very infrequent that some interest - here a possible environmental or health damage - has such an importance that it should take the precedence on any other legitimate interest. And it is still more infrequent that the same interest keeps this position through time. So, precautionary attitudes as regards the environment will have to be arbitrated with other types of legitimate interests, and arbitration solutions will evolve.

In spite of efforts to stabilise one definition of the Precautionary Principle, this Principle is marked by one significant hesitation unwillingly attested by an effort of James Cameron and Will Wade-Gery (1995, p. 100) to fix up the conceptual core of the Principle. After having said that "*A lack of certainty about cause and effect relationships, or the extent of possible environmental harm, does not legitimate delaying the imposition of some kind of regulatory mechanism over the activity in question*", they express what they think to be the core : "*The precautionary principle stipulates that where the environmental risks being run by regulatory inaction are in some way (a) uncertain but (b) non negligible, regulatory inaction is unjustified.*" These two sentences are not saying the same thing : the first one only says that a lack of certainty does not legitimate delaying action. But it does not say that immediate action is systematically required. This action has to be legitimated for its own sake. So, with this understanding, the principle is open to a public and rational debate about the opportunity to act. As regards the second sentence, the features of uncertainty and non-negligibility are supposed to be sufficient to require immediate action, since inaction is judged unjustified. And, implicitly, this action seems to be a refusal to authorise a project, or a ban on a substance. There is a logical shortcomings here, one that illustrates a frequent twist of statements, which begin by evoking the legitimate right to act without certainty and finish by saying that immediate regulatory action is an obligation. We should notice that the often-quoted North Sea Ministerial Declaration of November 1987 only refers to the idea that "*a precautionary approach may require action (...)*".<sup>35</sup> Viewing a precautionary action only as a legitimate possibility which needs to pass additional assessment before being translated into action is more in accordance with our conceptions of decision-making processes under

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35.- Underlined by us.

scientific controversy : the social body has to be let to make value judgements on how to interpret the Principle.

### ***Conclusion***

We first tried to characterise contexts of decision-making under scientific controversy, which gives a prominent role to the social process of construction of issues. Then, we have shown that the pressure to act leads at one moment to some new regime, generally based on some technological options : since the scientific controversy cannot be solved by itself, technological conventions and social acceptability of options come to an indirect arbitration among competing scientific views. This way to tackle the issue has a direct consequence on the process of expertise. To this regard we have stressed in section 2 the specific, but often implicit, roles of expertise. The main concept is that expertise is an "investment in forms", framing information into the forms required for action. This does not go without misunderstandings and mistakes that appear when results of scientific constructions are perceived as direct expressions of reality.

Among the conventions which result from the work of experts and at the same time frame the course of relationship between science and decision-making regarding environmental issues, the Precautionary Principle has emerged as a leading one, together with the sustainable development concept (Godard, 1994). It may authorise a new equilibration of public decision-making in favour of environmental and health protection. But it may also get hazardous results, because the Principle gives birth to an open-ended process, whose closure will not come from science. It may particularly give birth to important side-effects in changing the course of relationship between science and decision-making : it may place society into a new dependence on a rather contingent dynamics of assumptions within research activities, and at the same time load the research activity with a new weight of responsibility and social interference, contradictory to the classical academic freedom for searching and testing new ideas. There is some chance that, turned into illusory statements and rhetoric, the Principle will contribute here and there to a degeneracy of the quality of decision-making. Another likely risk is that the Principle will cover the dominance of some implicit doctrine responding to an epistemic community disseminating its "vision of the world and society" far from any truly scientific and democratic control.

Since the content of the Precautionary Principle is either not well-defined, or unacceptable in its most extreme version, solutions should be searched not in the idea of reverting the burden of proof, but in organising the conditions of expertise, so as to adapt and protect at the same time the scientific activity against the new social risks to which it will be exposed. The point is to learn how to reconcile two different goals : to organise an open discussion among specialised scientists, and to limit the possibility of a strategic

instrumentalisation of scientific works and conceptions. The media are not the central place for achieving an arbitration of scientific controversies, even if such controversies are not to be hidden. But the media are the place to amplify and multiply the socio-political debate on priorities, criteria, thresholds of acceptability and so on.<sup>36</sup> For such a debate to take place in good conditions, it should have been made clear that science may never give certainty on some issues, but a zero risk approach will be irrational and self-defeating, i.e. impossible.

Scientific communities could profitably develop their own collective structure in order to provide an expertise which will not depend excessively on individual strategies or desires, or on administrative command and political selection. In particular, new mechanisms should be developed as regards the treatment of assumptions and the construction of possible worlds, in order to find a casuistic permitting to discriminate between possibilities and assumptions categorised as negligible from the viewpoint of regulatory action and those for which some initial precautionary steps should be taken. This collective expertise, expressed in forms analogous to reports of the Academy of sciences, should specify a gradual scaling of the state of knowledge about one issue, up to the point where there is "sufficient evidence" to recommend a strict application of the Precautionary Principle. From a law viewpoint, different obligations should be set at successive degrees on the scale of knowledge : at initial degrees, action will be limited to an obligation of developing information (research on marginal assumptions, data collection, experiments); at further degrees, there may be guidelines and information disseminated in the direction of the concerned people, without any regulatory constraints. The latter may be considered at the subsequent degrees and so on.

This approach of collective expertise should not be confused with basic research itself, since its work would be centred on implications of science in terms of social action and commitment, with all the roles of expertise we have identified in section 2. At the same time, this collective intermediation may insure an adequate protection of individual researchers against possible engagement of their civil responsibility.

The general concept supporting these suggestions is to differentiate the scenes and roles, and preferably to avoid a general confusion of stakeholders and scientists within some global forum in charge of saying the truth and prosecuting science at the same time.

Within this perspective, precautionary action is not supposed to support a definitive commitment or prohibition, but presumably a transitory one. Since the real limits of nature are unknown, our regulatory measures should be looked at as experiments which have to be controlled and analysed, as it is done for scientific experiments in order to improve our

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36.- For instance, in the case of carcinogenic toxic products, the controversy about the very existence of a threshold of complete safety, makes it impossible to define standards calculated on the basis of a science-defined threshold. J.-P. Moatti (1989, 1991) explains how, in that case, economic analyses may be used for developing a social debate on the acceptable levels of risks and establish some procedures to avoid large incoherence in public action.

knowledge. Then, we have to organise the regulatory measures so as to be able to welcome future progress in knowledge. This means maintaining a sufficient reversibility of rules. Management of the timing of regulatory action is a key issue<sup>37</sup> to this regard, as we pointed in section 1. If a regulatory process in one domain has a long history that proves that it has systematically come too late to the decision and implementation stages, invoking the Precautionary Principle may add to the strategic resources needed to speed-up the process and overcome all strategies of delaying action in this domain. In other cases, it will be wise not to take premature measures, because the possible irreversible damage costs borne by Nature are not the only irreversible costs to consider.

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37.- Maximising the learning time we still have before any hard commitment should be made, can be elaborated into a decision criterion for tackling controversial risks. It may provide an appropriate substitute for standard cost-benefit analyses which cannot be made relevant for decision purpose in such informational contexts (Godard, 1992b). In the case of global warming, see J.-C. Hourcade (1991, 1994).

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