HAZARDOUS WASTE POLICY MANAGEMENT – INSTITUTIONAL DIMENSIONS

CHAPTER 2: RISK ASSESSMENT OF TECHNOLOGICAL SYSTEMS – Dimensions of Uncertainty

B. Wynne

May 1984
WP-84-42

INTERNATIONAL INSTITUTE FOR APPLIED SYSTEMS ANALYSIS
2361 Laxenburg, Austria

PREFACE

This paper has been produced as part of IIASA's hazardous waste management work, which is the main component of the Institutional Settings and Environmental Policies project. The overall aim of this work, reflected in this paper, is to systematize our understanding of interactions between institutional and technical factors in policy making and implementation. The influence of institutional processes upon technical knowledge built into policy has been increasingly recognized. However, it has yet to be adequately clarified in comparative research on different regulatory systems. Institutional structures canot be easily transplanted from one culture to another. Nevertheless, through the normal flux of policy, institutional development slowly occurs anyway, in more or less *ad hoc* fashion. Comparative insight may help to direct reflection and adaptation in more deliberate and constructive ways.

This paper forms one draft chapter of an intended book on hazardous waste management. The reader will therefore notice references to other draft chapters in this study which are also being circulated separately, and which are available from IIASA. A full list is given overleaf. At this stage the papers are drafts, and are not intended for publication in present form. They are being circulated for review and revision.

I would like to thank those policy makers and others who have exchanged papers and information with us, and those who generously gave of their time and experience in the many interviews which form a substantial input to this work. A full list of acknowledgements will eventually be published.

Brian Wynne
Research Leader
Institutional Settings and
Environmental Policies

HAZARDOUS WASTE POLICY MANAGEMENT — INSTITUTIONAL DIMENSIONS

INTRODUCTION B. Wynne

CHAPTER ONE **Hazardous Waste – What Kind of Problem?**
B. Wynne

CHAPTER TWO **Risk Assessment of Technological Systems – dimensions of uncertainty**
B. Wynne

CHAPTER THREE **Risk Assessment and Regulation for Hazardous Wastes**
B. Wynne

CHAPTER FOUR **The Listing and Classifying of Hazardous Wastes**
M. Dowling and J. Linnerooth

CHAPTER FIVE **Government Responsibility for Risk: The Bavarian and Hessian Waste Disposal Systems**
J. Linnerooth and G. Davis

CHAPTER SIX **Enforcement of Hazardous Waste Legislation in the UK**
E. Ley and B. Wynne

CHAPTER SEVEN **Summary, Implications, and Further Problems**
B. Wynne

Further Case Studies

Hazardous Waste Management in Hungary — E. Kiss

Hazardous Waste Management in the Netherlands
Central processes in policy and implementation — J. Dirven

Dutch policies from a local perspective – J. van Eindhoven, R. Hortensius, C. Nauta, C. Worrel

CONTENTS

INTRODUCTION	1
DEFINING RISK	6
DEFINING THE RISK GENERATING PROCESS	10
Dimensions of Uncertainty — Ignorance and Perception	12
LEG Facility Risks	13
From Technical Imprecision to Social Contradiction	16
UNCERTAINTY BY STRATEGIC DESIGN	24
(a) Data Uncertainties	25
(b) Institutional Diffraction of Key Data and Key Terms	30
CONCLUSIONS	35
REFERENCES	38

CHAPTER 2:
RISK ASSESSMENT OF TECHNOLOGICAL SYSTEMS – Dimensions of Uncertainty

Brian Wynne

INTRODUCTION

The main aim of this chapter is to identify the fundamentally different kinds of uncertainty in risk assessments, especially the difference between conventional technical uncertainty, and incompatible socially influenced *definitions* of the risk-generating system. This distinction is crucial, yet the second kind of uncertainty is often very subtle. Recognizing the extent of this second kind of uncertainty in the technical and institutional context of risk assessment, regulation and implementation has far-reaching implications.

The following chapter three examines the conflict between the need, on the one hand, for standardized technical formulae and methods in risk assessment and regulation, and the contradictory logic of tailoring risk assessment and regulatory controls to risks arising in real situations in all their diversity and instability. This conflict in technical frameworks straddles a deeper institutional conflict as to where to allocate responsibility and power to interpret regulatory aims. It parallels the disparity which exists between central policy formulation of regulatory rules, standards, etc., and the often very different informal realities of their diverse local implementation, when other influences and logics come into play.

This chapter develops these questions through several examples, and shows how they are connected, for example in the ways in which genuine situational risk-variation overlaps with and often looks identical to varying perceptions of the same risk-situation or process.

Perceptual differences are often treated as rather exotic matters of public "irrationalities" only, having little to do with technical realities and discriminations. The present analysis concludes to the contrary, that perceptual differences of what a technology is, what are its significant components and connections (in detail and in the large) influence experts and their rigorous technical risk assessments also. Yet this influence is usually unrecognized, and conflicting analyses attributed instead merely to un-closed technical imprecisions and uncertainties.

These assumptions or perceptual commitments underlying technical analysis for regulation are part of a tissue of informal judgments in science which ultimately cannot be justified by tight, unambiguous rules of inference, method or logic. This 'informalist' model is sometimes regarded as criticism of science: in fact it is a tribute to its flexibility and resilience. Yet the opposite model dominates public attitudes, and policy making institutions [1]. As emphasized in this Chapter, the pervasiveness of this intrinsic, informal dimension of science complicates the requirements of formal accountability and standardization for authoritative regulation. This is especially true where the real world character of the issue is so ill-defined and extremely variable, and where public skepticism and more elaborate justification — especially on siting and transport —is increasingly being demanded of regulation [2].

The impossibility of objective definition of risk problems and of assessment or regulation decision-rules is stressed in this Chapter. However, the aim is not to suggest that formal risk assessment should not be pursued, but to lay bare the extreme fragility of the authority of such decision processes to public skepticism, if this begins to assert itself. This inherent vulnerability is multiplied by the large unknowns and indeterminate, ill-defined nature of the policy field in the hazardous waste case, properties which undermine attempts to discriminate and even rank with scientific precision the risks associated with different regulatory options. In these circumstances, administrative cultures and institutional arrangements which fragment the overall process of risk management and regulation are more likely to find their policy implementation picked apart and undermined or paralyzed due to the interacting uncertainties, complexities and conflicts involved, than systems which manage to coalesce and absorb the different phases into more unitary institutional forms.

Although the immediate problems facing policy makers have been about the establishment of an effective industrial treatment and disposal (T & D) infrastructure, this focus has been complicated by the increasing need — arising out of growing public concern — to address the risk management issue more explicitly and systematically. Thus a circular obstacle has tended to confound attempts to develop the appropriate infrastructure.

Due to basic ignorance, the particular configuration of wastes and thus risks is badly defined, and cannot be better defined until a better knowledge of waste arisings, properties and specific environmental dispositions is gained. An industrial T & D infrastructure needs to be developed to control these waste arisings now, but may require adaptation and thus possibly costly abandonment or changes of large investments when risk-estimates are revised. However, whatever regulations are established will directly affect public acceptability of those plants, and the size of their markets, thus their viability in two dimensions. Hence there is a very great reluctance on the part of private industry to take initiatives or be involved in the T & D field. Thus the interaction between "industrial innovation" and Risk Assessment (RA) definitions is strong, and not necessarily free of contradictions.

Risk assessment requires reliable estimates of the toxicity or hazard causd by exposure to a given waste, and estimates of the chance of exposure. This is a combination of intrinsic material properties and situational variations - how it is packaged, mixed, treated, confined, etc. Unfortunately, physical, chemical and behavioural heterogeneity, and unpredictable behavioural freedom in the system, mean that "downstream" unpredictables may swallow up putative risk-differentials.

Yet however underripe the field may be for it, a RA management framework seems inevitable. It is therefore necessary to explore what the possibilities and implications are for using formal risk assessment approaches to hazardous waste management. There is currently a lively debate amongst policy makers as to how elaborate RA can and should be for hazardous waste management.

Even in the United Kingdom, the traditional stronghold of non-quantified, informal methods of decision making on issues involving risks, a recent Royal Society Study Group and the Royal Commission on Environmental Pollution both expressed strong support for more quantification of risk assessments [3]. Not only in the US therefore but also in Europe there is already, and will continue to be, growing pressure to adopt formal RA methods in hazardous waste management. This chapter will therefore review the possibilities and limitations of present risk assessment methods applied to hazardous waste management.

An initial question is whether formal RA should apply at a central policy level where decisions may be more discrete 'events' (such as whether to develop local or regionalized facilities), or at more routine but perhaps equally significant levels of regulatory implementation (such as siting or licensing conditions of given facilities; design of particular processes, including containment devices; or trigger standards filtering different materials into different levels of regulation and different levels of treatment and disposal). Once, one could say that the former may have involved *justification* as well as internal technical analysis, whilst the latter were purely technical, with no symbolic justificatory dimension. Nowadays, even these tend to require justification as external scrutiny and scepticism advance. This changes the role of RA.

The US Office of Technology Assessment Report reflects a typical view, of "technical optimism", that formal hazardous waste RA should produce hazard classifications ranking degrees of hazard and indicating appropriate T & D routes [4]. Being based on the same scientific knowledge, thus would presumably generate consistency amongst definitions and classifications. Yet as chapters 3 and 4 show as well as this chapter, "scientific" definitions of hazard are ambiguous: they are not merely physically uncertain, but actively incorporate different social assumptions reflecting different, and even incompatible administrative purposes commitments and needs in different systems. Chapter 4 describes in detail some of the origins of such different mixes of "science" (including uncertainty) and other factors in different hazard classification schemes. As we shall see in this chapter, risk assessments, even for relatively uniform, well defined technologies let alone hazardous chemical wastes, have suffered large intrinsic uncertainty and inconsistencies due to implicit differences in the assumptions structuring technical analysis.

DEFINING RISK

The conventional definition of risk is the product of the degree of harm a given event would cause, and the probability of that event's occurring,

$$R = P \times C$$

This would express a risk as, say estimated number of attributable deaths or other damage per unit time of operation of a given activity. But, say, a chemical plant might accidentally emit lethal clouds of toxic gases every year in a remote region, and cause zero harm. Or a given chemical waste may be extremely toxic, and thus in principle of high hazard, but environmentally highly immobile and remote, therefore of low risk. Hazard may therefore describe the intrinsic "worst-case" damage a process or material could cause whilst the above definition of *risk* incorporates variable situational qualifications which reduce the probability of this worst case damage [5].

In the case of industrial plant, some such qualifications are that: properly designed, constructed and operated equipment has a low chance of failure; many parts of processes have reserve parts in case of failure; have fail-safe or redundancy built into the system; and have monitoring systems which automatically react to early signals so as to prevent major failures. Other factors affecting P might be that operating staff are of greater or lesser professional expertise, that regulation and inspection is lax or tight, that there is greater or lesser pressure economically to cut corners, that there is more or less design, construction and operating experience, remote siting, etc. etc.

In the case of hazardous chemicals, some equivalent qualifying factors might be the physical form of a chemical, (e.g., if it is an inhalation danger, is it in fine powder form?); chemical state (e.g., is it in a soluble compound valency state) and form of containment; volume; local disposition (is it accessible to environmental pathways back to human populations); state of mixing with other materials; the kind of human handling it receives, etc., etc.

A typical schematic form of Risk function would be as in figure 2.

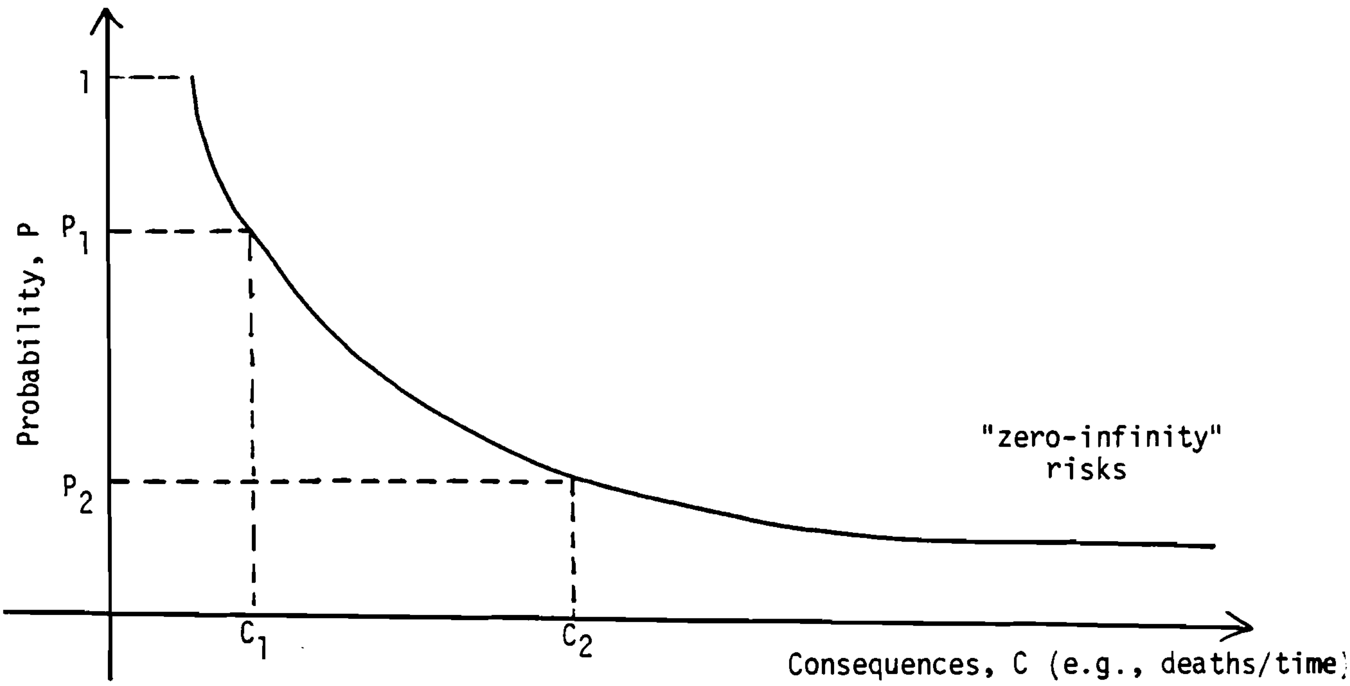

FIGURE 1. A Typical risk-function (schematic).

The same Risk, R on this formulation, can be given by different combinations of P and C, for example, $P_1C_1 = P_2C_2$, but these may represent radically different events and experiences. Thus a compelling criticism of the $R = PC$ formulation has been that the universal dimensions thus produced, take no account whatever of other, perhaps major differences in the kinds of damage under consideration — it does not at all compare like with like. "Risk" as conventionally defined is thus an artificially narrowed concept which may or may not capture the essential features of an issue or decision problem which its different participants define.

A related difficulty of compound risk approaches is that they may conceal value commitments in their definition. Thus risk expressed as product, PxC, may incorporate different kinds of harm — mortality, morbidity, other losses — without specifying these, and be measured implicitly against different yardsticks. Thus a comparison of risks per unit time may be very different from a comparison of the same risks per unit of output (or capital or labor input) if one process is more productive than another in terms of time, capital or labor. A work-force may wish to know risks per unit of work time; a manager per capital input or output; and a local resident, per unit of residence time. These often implicit yardsticks can suddenly change the apparent scale and importance of risks very considerably [6].

Another problem with the conventional approach has been that the probability of a given harm actually occurring usually depends upon a compound of probabilities: (i) of a set of necessary, or facilitating sub-events occurring; (ii) in a complex plant or with hazardous materials the same damage could be created by many different possible accident sequences, involving different chains of events in different components. Thus even in the same plant or system, the same "risk" may be posed by different sequences and combinations.

In other words, the use of the formula $R = P \times C$ may often conceal more than it illuminates, for one thing because it may confuse different routes to the same risk end-point; and secondly because it may conceal different assumed end points which should be carefully distinguished. Thus for example a highly hazardous facility may be recorded as a low risk *facility* because it is remotely sited, when it is the *remote siting* which is low risk, not the facility.

In any real analysis, the P of a given end point, C is actually the integral of products of probabilities P_i of each set of sub-events which could end up in C. If one includes external doses and health damages as end points (rather than, say, releases from a plant) the network of events and chains proliferates. Given that risk analysis is supposed to be a policy decision aid, there is a trade off to be made between (i) decomposition of risk calculations, which can show sensitive points (either of ignorance or failure probability) in the overall risk system, but which may not give policy actors end points which are meaningful to their decision language; or (ii) composite risk terms which have the opposite pros and cons.

In the light of these problems, other experts therefore advocate that the term risk be used to define only the ***probability of occurrence of a specified end-point*** or harmful event [7], so as to clarify the distinctions between (possibly multiple) intrinsic hazards of any material or activity, and qualifying factors such as siting, containment, operating rules, treatment, etc., which reduce the probability of a given hazard's being realized in practice.

We can therefore distinguish between what might be called the "fundamentalist" approach (in that it attempts to distinguish between "fundamental" hazard characteristics which it is supposed are invariant), and a situational approach which recognizes variations according to different physical situations, chemical forms, environmental conditions and human actions and decisions. This distinction is analyzed in Chapter Three.

Situational discrimination seems to represent an overall improvement in clarity of risk definition, but it should be noted that the distinctions are not absolute. For example if a chemical waste is treated (deliberately or not) thus reducing its hazardousness by making it say less soluble therefore less environmentally mobile (as well as less gut-ingestible), is this an *intrinsic* change or a situational one (especially if it is a reversible change)? Containment or back-up devices for nuclear reactors or other hazardous installation may be regarded as 'intrinsic' parts of plant design in some countries, but optional extras, thus 'situational' elsewhere [8]. There is no clear-cut 'natural' state of a material or technology by which to define its intrinsic hazards and which could act as a definitive base for explicating all situational risk-qualifiers. We return to this point later. Nevertheless, with this concept in mind the distinction may still be a valid methodological principle, and an improvement over compound Risk terms.

Formal, quantified risk assessment was first developed in military and nuclear systems especially in engineering reliability for design and construction standards. Although the human components in such systems have recently received increased attention, the approach was dominated by probabilistic estimation of failure rates in mechanical components. More recently however, this engineering strand has been complemented by developing biological approaches to analyzing the hazards of released materials either from large accidental discharges following such mechanical failures or from routine emissions [9].

As is discussed below, even risk analysis of relatively standard technologies such as liquid energy gas terminal facilities or nuclear power station, suffer colossal differences according to different implicit process and problem-definitions and underlying assumptions.

DEFINING THE RISK GENERATING PROCESS

Formal probabilistic risk assessment of complex, potentially hazardous systems examines component reliability and the knock-on effects of failure through a causal chain in the system to some harmful consequence. Thus "fault tree" analysis begins with a hypothesized failure at some chosen point, then identifies the possible branching sequences, attaching an estimated probability to each sequence aiming at a composite estimated probability of occurrence for a range of harmful consequences. A sister-technique, event tree analysis starts the other way about, analyzing the various possible chains of events in the system that could lead to a given end point, then estimating the probabilities of each linking failure, leading to overall probabilities for each identified harmful end point.

Various controversies around such risk analyses, notably that over the US Rasmussen Nuclear Reactor safety analysis [10], have shown how deep are the uncertainties and opportunities for choice at almost every step in such analyses:

(i) there are many different potential release-events from a given plant, each of which has to be analyzed for its estimated work-force and external consequences. In order to reduce the consequence-estimation to feasible scale these are usually grouped into a smaller number of families. For example 14 failure or release categories were used in the Rasmussen Study, as the inputs to analyses of external consequences.

(ii) each release event end point usually has many, possibly interacting chains of possible failures that can lead to the same end-point. There is no guarantee that all possible significant release end-points or other pathways increasing the probability of even a known end point, have been identified.

(iii) even before composite probabilities are estimated, the description of possible chains of events is in itself so complicated that there is room even after a real event such as the Three Mile Island accident, for dispute as to whether or not the real world event sequence was actually described in the preceding analysis [11].

(iv) the basis of probability estimates of component failures and cascading sequences of events is highly variable. In some cases good empirical experience exists for reasonable statistical extrapolation; in others the applicability of historical data is questionable (e.g., samples of boiler failures — can data on conventional steam boilers of smaller size, thicker or thinner metal etc. apply to nuclear pressure vessels?); in others theoretical estimation has to suffice but may be incapable of experimental validation; in yet others, sheer ignorance prevails and either expert subjective judgments have to be orchestrated using Bayesian statistical methods [12], or crude guesses are made.

(v) normally, such uncertainties could be expressed as confidence limits or error bars around each component probability. But just as the probabilities (if independent) multiply through the analysis, so do the uncertainties surrounding each figure. Furthermore, in many real cases, the specific events considered do not have independent probabilities. This so-called "common-mode" [13] escalation of normal failure probabilities is especially prominent where human actors are more influential in the system. Legitimately different implicit judgements about these have large effects on analytical outcomes. This factor has not been widely recognized until recently.

Dimensions of Uncertainty — Ignorance and Perception

There are several basically different sources of conflict or uncertainty in such risk analyses. These are often confused. There is of course the possibility that one analyst or another has simply been less careful or competent than another. More important however, are those apparently frequent cases when equally competent analysts reach vastly different conclusions as to the risks from ostensibly the same process or system. Sometimes this is attributable to very different but equally plausible (or implausible) guesses as to component behavior or process connections about which little or no knowledge exists. This would create different risk estimates even if precisely the same process or system were being defined. But consider the case in which the estimated probability of one link in an event-chain leading to an accident is infinitesimally small in one analysis, and significant in another. In the latter case it may become the critical path to an accident, in the former it may be placed amongst those conceptually possible pathways that are regarded as virtually negligible. Notice now that strictly speaking, each different analysis may described a ***different detailed risk-process*** as the one leading to the most significant risks.

This point may be generalized, because in many real cases ignorance about causal events such as: hydrodynamics in complex pipe-work under extreme conditions; human behavior affecting risk-pathways; materials failure under very specific, often extreme conditions; and thus of pathways to subsequent releases, is overwhelming. Assumptions by analysts therefore about the boundary of the technology or process under analysis, and also about its internal structure, may be legitimately very variable. There is a continuum from systems where the different choices between analysts may be very narrow, detailed and technical (though still highly significant) to systems where the differences may include implicit behavioral judgments, large-scale differences over system-boundaries, internal cause—effect structures, etc. Whether broad or detailed, these differences of system- or problem-definition may be determined by social positions of analysts rather than freely chosen.

LEG Facility Risks

I will give some examples ranging from narrow to broad differences: The IIASA study of the risk analyses produced during the four different national siting decisions for LEG terminal facilities can be used as an example of narrow differences [14]. In all there were fifteen different risk analyses, but although the technology and process analyzed was very similar in all four cases,* they reached very varied conclusions. In part, this was due to different analytical definitions of what is meant by risk — what kind of potential cost? Given that such definitions vary according to social positions and values, it is not surprising that the initial, often unreflective process of narrowing down the analytical 'problem' to one kind of cost out of the wide choice in principle available (e.g., population-risk, critical group; per day, per tonne of gas, per job provided, etc.), produced sometimes incompatible *starting* points, let alone finishing points.

However there were deeper problem and uncertainties than this.

As Mandl and Lathrop note [15]:

> ...several decisions must be made in the course of performing a risk assessment, such as how to characterize risk, what presentation formats to use, what gaps to fill with assumptions, what assumptions to adopt, which of several conflicting models to use, how to indicate the degree of confidence of the results, and which events simply to omit from the analysis. These decisions can push the results in any direction...

Thus for example, some studies included shipping collisions or grounding and spills, others focused only on storage tank rupture, others included transfer spills, none analyzed potential sabotage. Even on specific events, estimates varied without any explanation. One study assumed that for a typical layout of six tanks surrounded by dikes, a valid estimate for a credible spill size was 15% of the contents of one tank, whilst others took at least the full contents of one tank as a conservative estimate. The estimated probability of a spill at one site varied by a factor of 10^3 (10^{-3} to 10^{-6}) in three separate analyses. When the analysis is extended to cover *effects* of a release, the conflicting assumptions multiply. Different models of dispersion and ignition were used,

* The major difference was that three facilities were for liquefied natural gas, methane, which requires very low temperatures (−161.5°c), the other was for liquefied petroleum gas, mainly propane and butane, which are less volatile and can be stored at nearly ambient temperature and pressure. In fact this technical difference was dominated by other differences introduced by the analysts in the studies.

different causes of damage were assumed — some took secondary blast effects to be the sole cause of deaths while others took thermal radiation.

It may be initially tempting to say that analysts chose their detailed problem-definition to suit the conclusion they wanted; but not all such assumptions have identifiable effects on the conclusion, and one must also accept that some of the shaping of problem-definition is unconscious and determined by social positions, specific intellectual traditions to which analysts belong, etc. This has been widely found to occur in science generally [16].

As Mandl and Lathrop conclude [17],

> ...what is striking about the estimates is the magnitude of the differences. Societal risk, individual risk, and the risk of one or more fatalities vary over four orders of magnitude across sites, and the risk of ten or more fatalities varies over eight orders of magnitude across sites. It is hard to imagine another area of political concern where performance measures receiving as much attention as these did could vary over such a wide range. Yet even more striking are the differences between the three reports prepared for Point Conception. There is about a factor of ten difference in both societal and individual risk.... There is a difference of four orders of magnitude in the risk of ten or more fatalities. A policy maker faced with such variations could conclude that all three reports are based on very limited knowledge of the risks of LEG.

furthermore,

> .... Each report poses as a representation of the current state of knowledge regarding LEG risks, but because that knowledge is incomplete, some of the reports represent it using probabilistic terms or error bounds. Yet each report is based on a different state of knowledge: different assumptions are made, models used, probabilities estimated, etc. No one report in fact represents a comprehensive representation of the current state of knowledge. When SAI gives a probability of 9.9×10^{-7}, and FERC gives a probability of 8.1×10^{-3}, for the same event, the policy maker is likely to be somewhat at a loss as to the appropriate figure upon which to base his or her decisions. ...each represents only a subset of the total state of knowledge. Yet neither report acknowledges that the other estimate exists!...

The implication, not fully spelled out, is that formal risk assessments may be unreflectively pretending to contain ignorance and "uncertainty" within apparently probabilistic bounds, which then appear to be analytically manageable, therefore definable as 'risk,' [18]: whereas the real scale of uncertainty is more properly characterized by (i) ignorance (there are factors and combinations that are just not even identified, let alone 'estimable'), and (ii) such a wide scope of legitimate analytical choice in defining the relevant system structure that the resulting knowledge is not characterized only by 'passive' uncertainty, that due to the effects of *imprecisely known quantities*, but also by 'actively' shaped uncertainty (and certainty — five studies did not even mention uncertainties!) This kind of uncertainty, although usually perceived as the 'imprecision' kind, is actually *implicit conflict*. It may or may not be reducible by *negotiation* between the analysts, but it will not be resolvable by more precise observation or analysis which is the usual fallacy. The conflict is *due to the effects of implicit analytical choices even in defining what the 'technology' is*. When technology is viewed as it should be, as a social–organizational entity (embodying 'hardware' but also behavioral relationships), this point can be more clearly seen [19].

From Technical Imprecision to Social Contradiction

Cox has given a useful discussion of unrecognized uncertainties underlying risk assessments, caused by variation in the actual processes being evaluated, when fixed processes are being assumed in the risk analysis [20].

In order to simplify his example Cox takes the evaluation of only workforce risks, thus excluding for now the further domain of problems of external emissions and associated risks. Although he discusses risks of electricity production technologies, the point applies to all technological process. Following is an outline of his argument:

Modern analysis can define technology as a network of stages connected by input–output flows. A given stage is defined by its input–output structure. For example: stages of mining, smelting, refining, manufacturing and finishing in a typical metallurgical industry; waste arising, in-plant mixing or treatment, 'packaging,' transport, storage, possible transfer and further mixing, final treatment and disposal, in the case of hazardous waste. More detailed models can be made of single plants.

The occupational risk per overall unit of output associated with a set of stages, J in the process, is

$$R_J = \sum_j^J \alpha_j L_j Q_j r_j \quad ,$$

where the set of states, J, is defined as a technology (say, incineration) which is assumed to be well-defined, with a constant input—output structure; Q_j is the number of units of output from stage j per year; L_j is the number of man-hours of labor used in the production of one unit of output from stage j; r_j is the number of deaths per employee-hour in stage j; and α_j is the fraction of the annual output from stage j (e.g., x tonnes of enriched uranium from fuel reprocessing) needed to support whatever overall production unit is used as risk yardstick (e.g., per 1 GWe of electricity produced or consumed).

Conventional risk assessment uncertainties arise, and multiply in the multilinear combination of values, each of which is a product of other uncertain estimates and data, and so on. However, there are more basic uncertainties in defining the 'system,' 'process,' or 'technology' in the first place. For example every process has to be an open system, with inputs from and outputs to an outside environment. Therefore a process or *technology* has to be defined by placing limits on it, thereby also defining its environment: but then arises the thorny question of appropriate system or problem boundary, or — put another way — attribution of risk responsibility:

Should nuclear *reactor* risk assessment include the risks of reprocessing, transport, waste disposal, even possibly horizontal nuclear weapons proliferation since these are arguably associated with it as inevitable entailments? Should the risk assessment of fluorine contaminated hazardous waste from aluminum smelting incorporate an element of the risks of coal mining or nuclear risks because of the intensive use of electricity in aluminum production? Does one include in coal risks, the risks involved in the manufacture of, say, the trucks found at mine heads, even though the same trucks would have been made had there never been such a coal mine? Once begun, the possibilities of such connections are limitless and paralyzing.

Inhaber's use [21] of essentially the same approach to analyzing energy system risks for example found high total risks for wind and solar power. But closer examination showed that these high risks resulted from an arbitrary assumption that dirty coal would be used as back up for these (intermittent) technologies used as base-load supply systems. Thus Inhaber's definition of 'solar technology' included dirty coal technology too! A normal definition of solar and wind technologies has them organized with storage systems, or with clean back-up. This is a different definition of the technology as a social–organizational unit.

As Cox emphasizes, the real economy and real technologies are far less simple than that implied in fixed internal structures of technologies, processes or even industries, and thus fixed technological coefficients. Both the boundaries and internal structure of a technology can (a) vary in the real world, and (b) be defined variably by the risk analyst (and others) as "the" technology or "the" risk problem in question. It is important to note that these are uncertainties in risk analysis over and above, and *qualitatively* different from, those associated with *imprecise* measurement in analysis. It is suggested that they are also key unrecognized variables in the strong dislocations of risk perceptions between different 'experts' and different public groups which is now a major concern in policy making.

Another set of examples enlarging the same point come from scientific disputes over the environmental risks of the proposed MacKenzie Valley pipeline from Arctic Canada to the USA [22]. Implicit, and eventually revealed, in the analyst's conflicting scientific conclusions were different social–behavioral judgments which created different problem-definitions. Thus some scientists assumed that one pipeline could realistically be evaluated for its effects in isolation from further pipelines, roads, telegraph lines, airfields, residential service-towns and other developments (the "corridor") which other scientists assumed would inevitably follow, and which should therefore, they believed be a 'natural' part of the system to be evaluated.

In another part of the same dispute, the damage to tundra from construction work was assumed by some analysts to be limited to that within *official* limitations of construction to winter months, when the tundra was hard-frozen. Other analysts assumed this was unrealistic because they believed the pressure of deadlines and huge investments would inevitably cause these limitations to be broken in practice, with summer-season construction leading to far greater damage.

Nuclear Technology

In these cases, as before, there is no objective, singular problem-definition or technological system which can be more and more precisely "revealed." A further example showing a slightly different but essentially similar dimension arose in the Windscale Inquiry in 1977 into a proposed oxide nuclear fuel reprocessing plant [23]. The inquiry chairman, the nuclear industry and government agencies defined the risk assessment decision as that concerning a single reprocessing plant, and nothing more. Objectors on the other hand, assumed that the plant, which generated plutonium and uranium for further rounds of nuclear power systems, would create institutional momentum for more nuclear developments including widespread fast breeder reactors and plutonium commerce. The risk assessment question, and the associated technological system, was defined as much larger and more diffuse, and was dismissed as "emotive" nonsense by the chairman.

Here was a conflicting choice of technology- or problem-definition which was not a 'facts' versus 'emotions' division. Nor was it clearly perceived and debated in the Inquiry as a conflict of founding problem-definitions. Yet the conflicting, equally legitimate definitions were a symmetrical pair based upon the different behavioral judgments and objective social experiences of the contending groups. To members of the establishment, it was rational to draw a boundary round the present plant, because they could objectively expect to influence and identify with the subsequent decisions whether or not to make further commitments. These decisions, and the technologies involved, could be logically fenced off and neglected. For outsiders to the decision making establishment however, an incompatible, but equally objective logic prevailed. From their objective social position, with their social experience, it was rational to assume that they would have no real part in any of those subsequent decisions, as they had been excluded in the past. It was therefore rational to condense all possible foreseeable future developments onto the present single plant decision. The technology or risk-system was thus defined to take these extensive further probabilities into account.

The important point is that *each* position, 'expert' or otherwise, was based upon behavioral judgments and social experiences which were necessary to frame a problem at all. But each was equally defensible, or illogical, according to one's social position. No deeper, more objective definition of 'the technology' existed. Nevertheless, the language of the Inquiry was totally that of an 'objective' technology with 'objective' effects, which could be '*discovered*' through the conflict by more rigorous analysis.

One structural variable which has become increasingly prominent even in highly automated technologies is the role of "the human factor" in bringing about accidents. To the extent that this has been systematically examined at all, it tends to have taken a mechanical, individual operator emphasis, attempting to draw upon empirical experience of "failure rates" for probabilistic extrapolation. Organizational distractions, and dislocations brought about by collectively induced "mind-sets" have been less fully integrated into risk analysis [24].

Pesticides

Again in this general case, the institutional origins of the risk analysis influence the definition of the 'structure' of the technology, which is nevertheless presented as if fixed, natural, and 'objective.' "Expertise" in defining "the" system may be open to surprisingly wide dispute. For example, the official government scientific Advisory Committee on the Safety of Pesticides (PAC) in the UK, evaluated the risks associated with 2,4,5-T in the late 1970s, when public suggestions about its pervasive harm were accumulating [25]. Having analyzed the scientific evidence, the Committee decreed that 2,4,5-T could continue in its widespread use. After attempts to reopen the issue by the National Union of Agricultural and Allied Workers, the main labor union involved in spraying 2,4,5-T for farm and other employers (including many local authorities and government agencies such as British Rail), the PAC reasserted the safety of

2,4,5-T, dismissing the large NUAAW dossier of admittedly circumstantial clinical and other evidence of actual harm, as unscientific.

This rather patronizing scientific rebuttal only polarized the gathering conflict even more, and eventually, in the face of further Union action, the PAC advanced the explicit qualification that its assertion of the safety of 2,4,5-T was conditional upon its proper manufacture, distribution and use. These conditions were precisely where the farm workers' and others' direct experience and evidence was focused. In this *behavioral* reality of the technology of 2,4,5-T production and use, they were the experts and not the PAC scientists. This 'behavioral' reality was not *merely* social, it was also physical — it shaped the actual physical processes that led to real damage. The laboratory controlled tests in the scientific literature examined by the PAC produced Risk Assessments which excluded *a priori* the realities of distribution and use of 2,4,5-T, and which potentially radically altered its risks. Furthermore, these were behavioral or institutional conditions — "the human factor" — just as objective as many physical parameters included. For example drums of 2,4,5-T often arrive with defaced or removed labels supposed to describe proper conditions of use. Even if these are known, the organizational realities of farm life often do not allow a farm worker to refuse to spray just because the climate is not correct, or because specified protective equipment is defective or non-existent. Also, the cultural reality of such work life does not encourage a man to say he is concerned about the possible risks of such materials.

The point of this example is to demonstrate again the analytical options available in specifying the process or technology for risk assessment. The narrow, unrealistic technical definition in this case produced arguably false risk assessments, and corresponding social and technical dislocations which have ramified beyond the specific issue in question into general issues of regulatory credibility, even good faith.

Below, we explore this perspective further since it produces additional important questions about the kinds of uncertainty underlying formal risk assessment and policy management.

To summarize the present section, different socially influenced structural definitions of a 'technology' or 'technological system' [including its *implementation*] form taken for granted but different problem-definitions underlying formal, rigorous risk analysis. There is no single objective definition of a technology which supercedes all others. Risk analysts have to make commitments to one definition or another, ***before*** analysis begins. These commitments may differ. This point has been addressed at length, because it clarifies a key confusion frequently found between two quite different types of uncertainty in risk analysis:

Type 1 is the most commonly recognized, for example in the estimation of the probability of a given event, say a key component failure, which combined with other events may lead to human harm or other unacceptable end-points. There may be (is always) uncertainty about such factors because they are genuinely indeterminate, or because although determinate, we have inadequate knowledge for accurate estimation; or due to complex mixtures of both. For all its inscrutability or at best, partial scrutability, one can at least say that this type 1 uncertainty is 'there,' in the system.

All this usually generates uncertainty enough. However type 2 uncertainty intermingles with type 1, but is fundamentally different. It is that induced by different perceptions, different definitions even of the risk problem. But these may be so subtle or socially ingrained as to be unrecognized as such by the analyst, even when they are extreme [as in the 2,4,5-T case]. These are actively, though often sub-consciously *created* uncertainties built into the very structure of the analytical domain, and influenced by institutional factors, such as the analyst's social and professional background, institutional position in a decision making network, etc. This type of difference can give rise to different estimations of the same factor, therefore to recognized bands of type 1 uncertainty. But it does not stop there. Institutional uncertainty *envelops* the system rather than merely resides within it, because "the system" or "the starting problem" is itself subject to conflicting definitions. This uncertainty is not *in* the definitions, but all around them. This may have radical implications for policy and risk assessment.

UNCERTAINTY BY STRATEGIC DESIGN

In the previous part of this chapter we have distinguished in technical risk assessment between 'orthodox' imprecision, type 1 (which may include real system indeterminacy), and structural, or institutional uncertainty, type 2, brought about by (frequently subtle) differences and tacit conflicts of problem-definition.

In this section we will examine different kinds of uncertainty underlying the attempt to define terms and data which are normally regarded as absolutely central to a regulatory scheme. We will progress from 'orthodox' uncertainties in measuring hazardous waste forms, to active socially generated uncertainties in measuring hazardous waste forms, to active socially generated uncertainties in even *defining* hazardous wastes. The point will be to show that as a policy issue, hazardous waste cannot be managed by an approach based on conventional notions of 'decision making under uncertainty' alone. In risk issues this approach tends to locate the origins of uncertainty only in biophysical reality [26]. More technical analysis, and decision-insurance against any *intractable* biophysical imprecision, is thus the conventional way to solutions.* Our conclusion is that the central properties of hazardous waste management make *institutionally generated* uncertainties the dominant kind. These uncertainties include: tacit social conflicts or dislocations over the problem being addressed; over the key terms used for defining the problem and satisfactory management mechanisms; and the key data. The first has already been tackled, so we now address the last two aspects.

(a) Data Uncertainties

Even if the definitions of "hazardous" and "waste" were universally agreed, and there were also no intermediate interests diffracting "real" quantities and kinds of waste arisings into regulatory data, there would *still* be more problems than often recognized, simply in making the "correct" technical observation. There was a treatment and disposal company in the UK which had been criticized for not controlling the composition of the wastes delivered to it. The company contracted experts at Harwell to help devise an accurate analytical sampling device for just one of its many consignments, an oil-water emulsion delivered in 4,000 gallon tankers [27]. A standard vertically sectioned thief-tube was recommended. Trials found even this simple two-phase physical sampling impossible to perform except by very rough estimation. There were not two phases but four — water; sludge; oil; sludge, — with very indistinct boundaries. Samples along the tanker (which was only baffled, not compartmentalized) showed variations by ±50%, though they should have been identical. This was the least difficult kind of sampling and analysis — a very simple, merely *physical* analysis with no chemical complications — yet it proved impossible to perform anywhere near accurately. This was also for only one load, of only one

* Obviously there are different goals and preferences entering into policy decisions and these are dealt with by the standard approaches. However the dominant framework assumes that a unitary factual basis can be found (even if this is probabilistic), from which those preferences can take off.

type of consignment among many different sorts. It is not surprising in the light of such realities why even in the detailed data survey of waste arisings in Hungary (see case study), it was admitted that experts frequently had to resort to estimation to obtain figures at all.

Determining the Amount of Hazardous Waste in Massachusetts

Attempt 1: The GCA Study. In 1976, the Division of Water Pollution Control of the Commonwealth of Massachusetts commissioned a study from an environmental consulting firm, the GCA Corporation to "survey the quantities, the geographic distribution, and the current practices of hazardous waste disposal in the commonwealth" [29]. As a first step, to determine the quantity of waste generated in the state, GCA reviewed the Division's file containing the permit applications and monthly reports from waste transporters licensed to operate in Massachusetts. These reports were required under a Massachusetts law prior to the enactment of the federal RCRA regulation. The reports were supposed to include monthly summaries of where transporters picked up a waste, where it was sent, waste type, and methods of treatment and disposal. However, GCA found their information incomplete and difficult to track or compile [30]. They decided therefore to conduct a telephone survey of a selected number of firms. Some 446 plants responded to their telephone requests for information concerning type of waste, amount generated per month, etc. This information usually represented the "best guess" of the plant manager or the plant's environmental engineer.

In order to yield statewide totals the waste figures reported by the firms were simply extrapolated on the basis of *number of employees* in the firms surveyed, compared to total number of employees in the industries state-wide. The firms surveyed represented 36% of the State's manufacturing employees.

This procedure assumed a linear relationship between waste generated and number of employees in a particular firm which GCA admittedly had no evidence was correct. But they felt that the estimates of waste so generated were good lower limits, "probably accurate to within a factor of two" [31]. With this methodology GCA estimated that 37.57 million gallons of waste were being produced per year in the state.

Attempt 2: The New England Regional Commission Study. In 1979, the New England Regional Commission employed Arthur D. Little (ADL) consultants to develop estimates of hazardous waste generation for the six state New England Region [32]. ADL performed no new analyses, rather it used the data of previous state studies including the GCA Report in Massachusetts. Taking GCA's raw data and performing the same extrapolation based on waste generated per employee ratios, ADL estimated the total wast generated for Massachusetts in 1979 was 49.2 million gallons, an increase over GCA's total of approximately 30%, presumably due to changes in employee statistics [33].

The difficulty in using waste per employee ratios for extrapolation is shown by the wide range of ratios ADL found in New England.

State	*Waste generated per employee per year*
Connecticut	255
Maine	258
Massachusetts	82
New Hampshire	156
Rhode Island	72
Vermont	155
Average	163
Std. Dev.	81

The report admitted that "variations between the states are not readily explained on the basis of industry differences" [34].

In addition to this estimate, ADL provided a "high sludge" estimate on the assumption that introduction of planned waste water treatment programs would lead to an increase in hazardous waste generation. Figures from Connecticut, which already had such a program were used to estimate "high sludge" amounts for the other states. This amount for Massachusetts was reported as 84.9 million gallons per year [35].

Estimates Become Fact. It was this crudely estimated range of generated hazardous waste, 49.2–84.9 million gallons per year, that became the official state statistic for hazardous waste generation. It was published however in units of tons, where 240 gallons were assumed to equal one English ton (based on the density of water). These values were 200,000–350,000 tons of waste per year. Just looking at one assumption alone, sludge or solid waste could be several times heavier than water, leading to *tonnage* figures several times greater than the estimates. With little reference to their uncertainties, the figures were used to argue for the enactment of a state hazardous waste control program modeled after RCRA.

Attempt 3: Department of Environmental Management: Obviously not satisfied with these attempts at estimating hazardous waste generation in the state, the Massachusetts Department of Environmental Management (DEM) decided in 1981 to do its own survey [36]. They hired yet another consulting firm, Urban Systems research and Engineering, to computerize and compile the information contained in the state's transporter reports (the same report rejected as too incomplete by GCA). This study calculated 170,000 tons of hazardous waste being produced in the state. In addition, DEM reviewed the EPA notification list of potential generators (compiled under RCRA) and an industrial directory in order to identify "potential" generators not reporting their wastes. Interviews conducted on site and by phone, and reviews of out-of-state manifest totals for waste from Massachusetts delivered to other states, "revealed an additional 17,000 tons of hazardous waste not reflected in or totals." DEM's final estimate was 190,000 tons of hazardous waste per year [37].

DEM identified sources of errors in this estimate, which were drawn from on-site interviews with 25 generators:

1. Under reporting of waste from generators who appear in the transporter reports.

2. Actual generators who did not file transporter reports as required by law.

Finally, DEM admitted that these figures "for the first time generated by hard data" took no account of waste being illegally dumped, "by pouring it down sewers, incinerating it without approval..., mixing it with conventional wastes, or using illegal disposal facilities." Quite honestly they concluded, "the extent of illegal disposal in Massachusetts is unknown" [38].

From a State to the Nation. This case shows that estimates of hazardous waste generation, often presented as hard facts, and used in developing policies for control are inherently "soft" numbers. Even more sophisticated surveys are usually based on information voluntarily submitted by generators who face large incentives to underreport their wastes. Amounts of waste being illegally disposed of is simply unknown.

The problems of a state trying to determine such figures are even more insurmountable on a national level. In August 1983, the preliminary results of a US national survey of hazardous waste generators conducted for the EPA, again by a consulting firm, were released. This survey was again based on a telephone and mail questionnaire of approximately 10,000 generators who had identified themselves to the EPA as generators under the RCRA regulations. However, only 20% of the firms surveyed claimed to have generated any waste in 1981 at all! The study concludes:

> The initial estimates are preliminary in nature and are subject to statistical uncertainties. Nonetheless, the study suggests that 150 million metric tons of hazardous waste were generated across the US and its territories during 1981, in contrast to previous estimates of 40 million metric tons. [39]

This is also in contrast to the 250 million metric tons that the US Office of Technology Assessment estimated by simply adding up state estimates for the 50 states and territories [50]. All the effort therefore has produced figures for regulation which vary by a factor of about six.

In the end, one must conclude that federal and state regulatory resources would be better spent not subsidizing the environmental consulting firm industry to produce numbers which inevitably have spurious authority conferred upon them simply because they exist. The summary by Donovan of her experience tracking fuel-wood consumption estimates in Himalayan deforestation could be applied directly to the hazardous waste issue [41]:

> "All too often, consultants have neglected to explain the methods used to arrive at their expert opinions...some estimates have been boldly quoted and requoted, often without citation, in ever more respectable documents, until a very casually contrived estimate has become the basis for policy formation and program planning."

(b) Institutional Diffraction of Key Data and Key Terms

When the authorities began in 1945 to try to assemble data about people's precise exposure to the blast and radiation from the Japanese A-bombs, they relied upon self-reporting of people's location at the time, and calculated doses from this [42]. Many survivors apparently mis-reported themselves to have been out of town, because a strong social stigma had rapidly grown against association with having been radiated. Later, when free welfare services were offered to bomb survivors, many of those who had been away suddenly recalled that they had after all been in town, and the figures for the exposed population expanded, with corresponding changes in risk-estimates.

In some countries where production is centrally planned and fervently pursued, incentives such as bonuses are offered to plant managers and workers who reach or even exceed production targets. There is therefore a natural temptation to overrecord production levels. Systematic tendencies in this direction have been observed, involving large-scale, informal social coordination to avoid detection [43]. In some cases even entire factories have apparently been created for central regulatory 'data,' so as to bolster 'production' and bonuses. Ingenious supporting elaborations have been developed, such as massive routine wastage and breakages to explain why the extra production did not generate corresponding revenue. In another case, involving new equipment and funds for oil production [44]:

> "Reports submitted by the Ministry in the 5-year period showed modernization proceeding according to schedule, and production figures barely short of plan indices. But an outside inspection in 1981 revealed virtually no attempt to introduce the new methods. Report data had simply been invented, and there was a shortfall of millions of tons of oil. Equipment had been left to rust and be pilfered. Inspectors found that thousands of barrels of imported reagents had been dumped beside railway tracks, thrown into abandoned river pits, and even encased in the foundations of railway embankments."

It took five years for this "data-diffraction" to be detected, by which time responsibility and remedy were extremely difficult if not impossible.

When the Hungarian public health authorities were conducting their survey into hazardous waste generation, it was found that agricultural stations, which were part of the agriculture inspectorate, had been unofficially storing disused pesticides for many yeas when they should have been declaring, properly treating and disposing of them. The stations were found to have been systematically under-reporting their inventories so as to "mask" their own inefficiency and negligence [45].

Reliable data about waste arisings — their types, properties, volumes, origins, movements, etc. — are regarded (along with a meaningful hazard-classification or listing of wastes) as the main foundation of a hazardous waste regulatory scheme. Naturally, everyone expects such data to be fuzzy; indeed the more familiar one is with the practical realities the less one expects of such data. When the Environmental Safety Group of Harwell, UK gave evidence to the Gregson Committee Inquiry of the House of Lords, they gave a range of 2.5 to 4 million tons of estimated hazardous wastes arisings per year in the UK. Discussion with the Committee expanded the range to 6.6 million tonnes. From their experience of the issue, and having conducted surveys, they thought this range pretty narrow. The Gregson Committee, which contained some experienced environmental policy makers, but no hazardous waste specialists was horrified that regulation should have to proceed in the face of such great uncertainty as reflected in the range of estimates [46].

The point about the examples above however, is not so much the range of uncertainty, as if merely that about a piece of reality which is physically difficult to observe, but the fact that the observation itself is *inevitably* mediated by other social actors. Sitting between the observer and the reality he wishes to observe these actors, actively and systematically *diffract* the observation, because they have different (even themselves changing in the A-bomb survivors' case) interests which give the observations meaning and clothe them as 'facts' or 'data.' They are data and *uncertainties*, created by *actively strategizing agents*; even when the strategy is not to deceive a regulator or researcher, the diffraction will still occur, for other reasons.

Thompson and Warburton give a similar example of conflicting data on fuel-wood consumption rates from research on Himalayan deforestation. The range of estimates of the same factor, like hazardous waste arisings apparently crucial in regulation, differ by as much as up to 70 times [47]. Such variations do not only fog, they obliterate the 'discoverable' differences between factual effects of policy options on which normal policy making subsists. Although in principle the key quantity should be objectively measurable without mediation (there *is* a given amount of fuel-wood being cut down: there *were* a given number of survivors in different exposure categories in Hiroshima and Nagasaki), in practice it is not and never could be. One might alternatively say that we can find people's diffracting orientation and their "diffraction coefficients," and adjust reported facts accordingly to arrive at the 'real' facts. But this again is unrealistic in real situations.

The same kind of *actively-created* uncertainty defines the central data in the hazardous waste management issue. In this case however, the problem has a further wicked twist to it.

For hazardous wastes, not only is there diffraction caused by observation and reporting, but there is an even deeper uncertainties over *what* is being observed anyway. Different basic definitions of what is "hazardous," and what is "waste" are also held by different actors in the system, and by different regulatory bodies in different state or national systems. Different definitions of key terms, "hazardous" and "waste" sharply affect estimated volumes of hazardous waste arisings, even before one takes into account more deliberate diffraction. In this case, even the underlying 'solid reality' becomes a mosaic subject to social definition.

The context-dependent character of "hazard" was discussed earlier in this chapter, and we will also return to a different aspect of its context-dependence in Chapter Three. Let us now therefore look only at "waste." A very common and serious ambiguity lies in the storage or even sale of toxic waste against possible future recovery of constituents, when it could be defined as a (non-regulable) resource. For example, a litre of silver- or mercury-contaminated oil or solvent may be a (hazardous) waste if taken as such. But if chosen to be defined as one of a thousand different litres, to be collected and combined, it may legitimately be an unregulated resource. Speculation against *future* market prices of contaminants is another large loophole.

An intriguing, but important case of this anomaly occurs when tankers or drums for liquid energy gas are "emptied." Because they are no longer carrying their usual load, they are treated as containing *nothing*, so they are taken as non-hazardous. In fact they now contain a hazardous waste — gas *vapour* at ambient pressure. Many accidents have occurred because of this perceptual anomaly*.

The silt which is carried naturally down the Rhine and deposited as a great economic nuisance in Rotterdam harbor used to be converted into a resource by dredging it and spreading it on agricultural land, where its high organic content was a benefit [48]. In latter years, the toxic contamination of the Rhine has made this silt a hazardous waste, useless for agricultural land, and disqualified from (human) sea-dumping by the London–Oslo Conventions. Notice that if the *Rhine* had dumped it in the North Sea that would not have been disqualified! However the imaginative Dutch have turned it back into a resource by using it to landfill a new sea-dyke and suitably insulated, to create new residential or industrial land. The extensive Dutch civil engineering, dyke-building infrastructure is busy. The material concerned has remained true to itself, but has had various truths invested in it by the surrounding institutions and actors strategizing over it.

A conference on risk assessment of hazardous materials transport, University of Waterloo Institute of Risk Research, Ontario, Canada, April 1984, recorded this problem.

CONCLUSIONS

Regulation is the crucial system point where universal principles and ideal type methods and concepts embodied in science and political rhetoric must be converted in real effects, grappling with real organisational and physical constraints and contradictions [49]. Science and risk assessment are one of its few valued resources in making policies, and making them credible. In the field of risk assessment of chemicals, scientific uncertainty itself is for the moment being relentlessly expanded by institutional driving-forces, in comparison to the escalating tasks demanded of it [50].

The dominant model of science in policy and risk management portrays a given pool of facts defocussed by surrounding uncertainty, which stands as a "feasibility space" for credible conflicting technical views of risks, etc. [51]. More objective analysis or disciplined debate (or both), so this model goes, will tighten the constraints of reality, and thus diminish the fuzzy outline of this feasibility space at least *towards* a singular technical reality within which all policy options must be located.

The present chapter used examples from technological risk analysis to show that intrinsically different definitions of the relevant problem–system can always exist. (This does *not* mean that *any* definition can be made up.) It showed that the "feasibility space" of technical uncertainty may be being actively drawn in on some sides by some social groups, but actively pushed out on other sides. Other groups may be attempting to do the same, but with a different profile of contraction and expansion. Each is trying to defend enough technical uncertainty, in the right areas, to make a viable association between a not incredible technical justification and its bundle of institutional interests or values.

These profiles and processes are not arbitrary, nor are they fully and deliberately *chosen*. They are strongly determined by pre-established institutional factors. Social analysis of science has corroborated this view of uncertainty and cognitive conflict [52]. When a new area of "uncertainty" is identified, such as chronic toxic chemicals effects, the different entrants bring with them their different conceptual and methodological resources already shaped by previous problem-definitions, explanatory interests, and research traditions. These scientific positions evolve in different ways through the unfolding policy issue so that at any one time what is regarded as known and what problematic by each school of thought is already shaped by these previous institutionalized (social and cognitive) commitments. In the chlorofluorocarbons-ozone dispute for example, the US government supported attempts to develop one-dimensional models, which would create data that could be compared between studies and thus (it was hoped) create a consensus. British approaches were dominated by two-dimensional models — with correspondingly different profiles of observation, key variables and uncertainties — because there was less anxiety abut policy consensus. As Rip has summarized it [33]:

> ...this way of looking at the socio-cognitive dynamics of a controversy makes it impossible to speak of areas of uncertainty that leave room for different interpretations which are guided by the differing values and interests of the parties in the controversy. These values and interests have already been at work, through problem definitions and research agendas, in determining what the lie of the land will be. And the controversy is often not about the interpretation of a given area of uncertainty, but about which areas are to be considered certain and which areas of uncertainty are sufficiently irrelevant to remain uncertain.
>
> Thus, it becomes vacuous to suggest that there would be no controversy if there were no uncertainty. But this does seem to be an assumption in current controversy studies: because there is a space left open by uncertainty, "controversies over science and technology develop over competing political, economic, or ethical values." (Nelkin, 1979) This might be the case in NIMBY-type conflicts, where people do not want a new plant, a powerline, or some other technological project "in their backyard." Even then, however, socio-cognitive dynamics appear as soon as the assessment of hazards and other science-related aspects of decision making are drawn into the debate

and subjected to further negotiation and articulation.

This is a radically different concept of policy uncertainties, technologies, risk problems, and of the role of risk assessment methods. The implied focus of analytical interest shifts from technical uncertainties towards the institutional patterns generating those technical uncertainties and their associated problem-definitions.

The underlying focus of policy concern shifts *partially* (though not wholly) away from the resolution of technical uncertainties towards building contexts of constructive negotiation and interaction between contending problem-definitions and institutional interests. This opens the door on issues of institutional design and processes of justification. Bringing this chapter home to our present policy topic, hazardous wastes and toxic chemicals risk management generally are much more ill-defined, technically and socially heterogeneous policy problems than the technological systems analyzed in this chapter. Yet even the latter were shown to be pervaded by multiple problem definitions, even amongst experts. The significance of 'actively-strategized' uncertainties and their institutional origins and patterns has to be central in policy analysis, as a context for viable risk assessment and regulation.

This is true for the internal nature of national systems. These internal institutional processes also shape the constraints upon harmonization at an international level.

As will be discussed in the final chapter, these kinds of structural, institutional uncertainty are not easy to articulate into public recognition and policy language. But their lack of recognition may exacerbate public reactions, and undermine regulatory credibility and the viability of management, because this field exhibits more than most those features of inherent heterogeneity, multiple problem definition and structural, institutional uncertainty, which contradict dominant approaches.

REFERENCES

[1] B. Wynne, "Institutional mythologies and dual societies in the management of risk", in E.Ley and H. Kunreuther (eds.), *The Risk Analysis Controversy: An Institutional Perspective*, London and Berlin, Springer Verlag, 1981, pp. 127–143; see also S.B Barnes and D.O Edge (eds.) *Science in Context*, London, Oxford University Press, 1982, part 5, "Science as Expertise". For the origins of the "informalist" view of science see M. Polanyi, *Tacit Knowledge*, London, Routledge, 1959; J.R. Ravetz, *Scientific Knowledge and its Social Problems*, Penguin, Harmondsworth, 1974; M.B. Hesse, *the Structure of Scientific Inference*, London, Cambridge University Press, 1974.

[2] M. O'Hare et al., *Facility Siting and Public Opposition*, Van Nostrand Reinhold, New York, 1983. Our own interviews in several countries, not on the specific subject of siting nor public opposition, nevertheless discovered almost universal concern on this topic.

[3] *Risk Assessment: a Study Group Report*, (F. Warner, Chairman), Royal Society, London, 1983: Royal Commission on Environmental Pollution, 10th Report, *Tackling Pollution — Experience and Prospects*, (R. Southwood, Chairman), London, HMSO, 1984.

[4] US Congress, Office of Technology Assessment, *Technologies and Management Strategies for Hazardous Waste Control*, Washington, D.C., OTA-M-196, March 1983.

[5] This section is fairly standard treatment, though the new reader in the field must be aware that terms like hazard and risk often vary in meaning in the way outlined, without being defined. The literature on risk is huge. See the journal *Risk Analysis*, and *Risk Abstracts*.

[6] B. Wynne, "Nuclear Power — Is the Health Risk Too Great?", *Journal of Medical Ethics*, *8*, (1982), 77-85.

[7] See e.g., the Royal Society report, op. cit. [3].

[8] A.E. Green (ed.), *High Risk Safety Technology*, Chichester & New York, John Wiley, 1982.

[9] W.W. Lowrance, *Of Acceptable Risk*, William Kaufmann, Inc. Los altos, California, 1976. Council for Science and Society, *the Acceptability of Risks*, London, Barry Rose, 1978.

[10] US Nuclear Regulatory Commission, *Reactor Safety Study*, WASH-1400 (NUREG-75/014), October 1975 (The Rasmussen Report); H.W. Kendall et al., *The Risks of Nuclear Power Reactors: A Review of the NRC Reactor Safety Study*, Union of Concerned Scientists, Cambridge, Mass. A. Lewis, *A Review of the NRC Reactor Safety Study*, American Physical Society, New York, 1978.

[11] *The Accident at Three Mile Island*, Report of a Presidential Commission (Chairman J.G. Kemeny), Washington, D.C., 1979. See the Royal Society Report, op. cit. [3].

[12] P. Gärdenfors and N.-E. Sahlin, "Unreliable probabilities, risk-taking, and decision making", *Synthese*, *53*, (1982), 361-386. See also B. Fischhoff et al. "Fault trees: sensitivity of estimated failure probabilities to problem representation", *J. Experimental Psychology: Human Perception and Performance*, *4*, (1978), 330-344.

[13] op. cit. [10].

[14] H. Kunreuther, J. Linnerooth et al. *Risk Analysis and Decision Process: Liquid Energy Gas Facility Siting in Four Countries*, London & Berlin, Springer Verlag, 1983. Chapter 7 by J. Lathrop and C. Mandl, "LEG Risk Assessments: Experts Disagree", pp. 148-177.

[15] Ibid, p. 157.

[16] L. Fleck, *The Genesis and Development of a Scientific Fact*, (edited by T. Trenn and R.K. Merton), London & Chicago, University of Chicago Press, 1979 (original 1935); K. Knorr et al (eds.), *The Social Process of Scientific Investigation*, Sociology of the Sciences Yearbook, 1980, No. IV, D. Reidel, Dordrecht, London; Barnes and Edge (eds.) op. cit. [1].

[17] Ibid, p. 166.

[18] D.A. Schon, "The fear of innovation", in Barnes and Edge (eds.), op. cit. [1].

[19] B. Wynne, "Redefining the issues of risk and public acceptance: the social viability of technology", *Futures*, *15*, (1983), 1-31; also B. Wynne, op. cit. [1], and "From public perception of risk to technology as cultural process", paper to NATO Advanced Study Institute, Les Arcs, France, September 1983. To be published in Study proceedings,, ed. V. Covello, US National Science Foundation; R. Johnston, "Controlling technology: an issue for the social studies of science", *Social Studies of Science*, *14*, (1984), 97-114,

[20].L.A. Cox, Jr., "Artifactual uncertainty in risk analysis", *Risk Analysis*, *2*, (1982), 121-135.

[21] H. Inhaber, *Risk of Energy Production*, Atomic Energy Control Board of Canada, AECB-1119/Rev-1, (May 1978); J.P. Holdren et al., *Risk of Renewable Energy Sources: a Critique of the Inhaber Report*, ERG 79-3, Energy and Resources Group, University of California, Berkeley, June 1979.

[22] B. Campbell, "Disagreement between experts: Scientific disputes in the MacKenzie Valley Pipeline Inquiry", unpublished Ph.D. thesis, McMaster University, Hamilton, Canada, 1981.

[23] B. Wynne, *Rationality and Ritual: The Windscale Inquiry and Nuclear Decisions in Britain*, Chalfont St. Giles, Bucks., England, British Society for the History of Science, 1982.

[24] H. Otway and R. Misenta, "Some human performance paradoxes in nuclear operations", *Futures*, 12(5), October 1980, pp. 340-357. For a typical set of nuclear industry responses see *EPRI Journal*, August 1980, which was a special issue devoted to Three Mile Island; M. Rogovin, *Report on Three Mile Island*, US Nuclear Regulatory Commission, Washington, D.C., 1980; J. Kemeny, op. cit. [11]; D. Nelkin, "Some social and political dimensions of nuclear power: examples from Three Mile Island", *American Political Science Review*, *75*, March 1981, pp. 739-751; D. Sills et al. (eds.), *Social Science Aspects of the Accident at Three Mile Island*, Westview Press, Colorado, 1980; J. Pfeffer, "Management as symbolic action: the creation and maintenance of organizational paradigm", in B. Shaw and L. Cummings (eds.), *Research in Organizational behavior*, Vol 3, JAI Press, 1980, pp.1-52; B. Turner, "The organizational and interorganizational development of disasters", *Administrative Science Quarterly*, *21*, 1976, pp. 378-397. Also, B. Staw, "Knee deep in the big muddy: a study of escalating commitment to a chosen course of action", *Organizational Behavior and Human Performance*, *16*, (1976), pp.27-44.

[25] J. Cook and G. Kaufman, *Portrait of a Poison: the 2,4,5-T Story*, London, Pluto Press, 1982; Wynne, *Futures*, op. cit. [19].

[26] W.B. Fairley and F. Mosteller (eds.), *Statistics and Public Policy*, Reading, Mass., Addison Wesley, 1977; R. Keeny and H. Raiffa (eds.), *Conflicting Objectives in Decision*, New York, Wiley, 1977; P. Humphreys, et al (eds.), *Analyzing and Aiding Decision Process*, Amsterdam, North-Holland, and Budapest, Academic Press Hungary, 1982. See also ref. [52].

[27] This account is taken from an interview with E. Finnecy, Environmental Safety Group, Harwell, UK, who was involved in the case.

[28] This section is taken from a case study by Michael Dowling, who was employed by the Boston Municipal Waste Management Division and took part in the survey.

[29] P. Fennely et al. *The Generation and Disposal of Hazardous Wastes in Massachusetts*, GCA Corp. Bedford, Massachusetts, October 1976.

[30] Ibid, p. 13.

[31] Ibid, p. 19.

[32] *A Plan for Development of Hazardous Waste Management Facilities in the New England Region*, Arthur D. Little Inc. Cambridge, Massachusetts, September 1979.

[33] Ibid, p. B-10.

[34] Ibid, p. B-9.

[35] Ibid, p. B-12.

[36] *Hazardous Waste in Massachusetts, 1982 Statewide Environmental Impact Report*, Department of Environmental Management, August 1982.

[37] Ibid, p. i.

[38] Ibid, p. ii.

[39] U.S. EPA, *National Survey of Hazardous Waste Generators and Treatment,, Storage and Disposal Facilities Regulated under RCRA in 1981*, August 10, 1983.

[40] Office of Technology Assessment, op. cit. [4].

[41] D.G. Donovan, "Fuelwood: how much do we need?", Newsletter (DGD 9), *Institute of Current World Affairs*, Hanover, New Hampshire, quoted by M. Thompson and M. Warburton, (1981). *Knowing Where to Hit It: A Conceptual Framework for the Sustainable Development of the Himalayas*, IIASA Working Paper, WP-84-30, April 1984. See their discussion of "Uncertainty on a Himalayan Scale", Chapter 3.

[42] This information was given in interviews with radiation research experts A. Stewart, E.P. Radford, J. Rotblat. How much difference it makes to risk estimates is of course, along with most other parts of the issue, a matter of scientific disagreement.

[43] See the chapters 13-16 in M. Clarke (ed), *Corruption: Causes, Consequences, and Control*, London, Frances Printer, 1983.

[44] S. Shenfield, "*Pripiski*: false statistical reporting in Soviet-type economies" Chapter 14, Clarke (ed.), op. cit. [43], p.243. Of course, such systematic data diffraction is not at all unique to such economies, but is a tendency of all bureaucratic systems.

[45] E. Kiss, personal communication from Hungarian case study research.

[46] UK House of Lords, Select Committee on Science and Technology, *Hazardous Waste Management*, (Chairman Lord Gregson), 3 vols, London, HMSO, vol 2, pp. 246-280, July 1981.

[47] Thompson and Warburton, op. cit. [41].

[48] This case is taken from incompleted research by Dutch case study collaborators.

[49] J.O. Wilson (ed.), *The Politics of Regulation*, New York, Basic Books, 1981.

[50] W.J. Nicholson (ed.), *Management of Assessed Risk for Carcinogens*, Annals of the New York Academy of Sciences, vol 361, New York, 1981.

[51] Wynne, op. cit. [1].

[52] See references [1], [16], [23], [53], and A. Whittemore, "Facts and values in risk analysis for environmental toxicants", *Risk Analysis*, *3*, (1983), pp. 23-34. Typical of the prevalent stance is the US White House Office for Scientific and Technology Policy's, as given by D. Calkins et al., "Identification, characterization, and control of potential human carcinogens: a framework for federal government decision making", *Journal of the National Cancer Institute*, *64*, 1980, pp. 169-176.

[53] A. Rip, "Comparative study of science-related controversies: avoiding blind spots", paper presented to the Annual Meeting of the Society for the Social Study of Science, November 1983, Blackberg, Va., USA. (Mimeo, University of Leiden, Holland.)