

Governing Uncertainty: the contribution of social sciences to the governance of risks in environmental health

Session 1 - The Quantification of Risks as a Mode and Science of Government

Quantification or Qualification of Risk Assessments?

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I. Dealing with Uncertainty

I am a biologist and I do not know whether that makes me a hard scientist or not. In any case, I focus more on empirical data as opposed to the theory of risk. How can we introduce the concept of uncertainty – with uncertainty being the absence of certainty? The Precautionary Principle was based on an empirical conclusion, according to which certainty does show up, but very often when it shows up, it is too late. Hence, just because there is uncertainty, it does not mean that we should not take action. If you talk to people who are supposed to take action, as soon as you talk to them, questions emerge – ‘Tell us more about this concept of uncertainty. What am I supposed to do?’

Decision-makers have a choice between a wide range of different initiatives and responses. They can inform the population and also take much tougher decisions, where they can, for example, establish temporary or permanent bans. You therefore need a scale and a spectrum for risk assessment purposes – and that is the very subject of my presentation. It is based on the idea, according to which, in the traditional approach to risk assessment, you have variables that are well known, to various degrees of precision. The impact of the risk, if it materialises, is generally well known. Uncertainty is something that you can throw probabilities into. You have statistical tools to break things down mathematically speaking and we agree that the ways in which you can combine risk and uncertainty obey a matrix, according to which you multiply the impact by the probability factor. You therefore have a single-dimensional method and you can then place all the various risk levels on a single scale.

The problem with uncertainty is that you are very often dealing with data that is not specific or accurate, and you cannot therefore use it traditionally speaking in the decision-making process. What I suggest, therefore, is that we use so-called semi-quantitative variables to further our understanding of the impact and the levels of uncertainty – and I will explore this further. The fact that this time it is not legitimate to simply combine impact on the one hand and probability or uncertainty on the other – in other words, what we looked at before in terms of the theory that I was talking about. It is not relevant, whether we are talking about

social sciences or mathematics. In mathematical terms, we are dealing with situations where there are undetermined forms. There is a very large impact being multiplied by something uncertain – from zero to infinity. Additionally, from a sociological point of view, we know that when there is a very strong impact, but the probability is uncertain, something that you see experimentally speaking is that people no longer apply this theory, but apply the two components separately. We therefore need a different combination for impact on the one hand and uncertainty on the other.

II. The Impact of Risk

1. Severity

In terms of impact, there are three different parameters: severity, acceptability and irreversibility. With severity, we realise that for a lack of a specific estimate of the potential global damage, there are a number of aspects that are important in terms of how to characterise the severity level. Firstly, will there be individual damage? That can be tracked down. Are there deaths that we can identify? For example, this person was indeed a victim – a victim of Mad Cow's Disease, for instance. What about probability? Is average mortality likely to increase or not? That means that we are indeed dealing with a person who was a victim of Mad Cow's Disease, for example. When there is individual damage – and we saw this with Mad Cow's Disease – the impact on public authorities and decision-makers is even stronger.

The second important thing is the following. Has the target population – all the people who could be at risk – been clearly delineated or not? Let us take a risk such as exposure to low-dose chemicals. We could say that the entire French population could potentially be affected. We therefore introduce the concept of disaster potential and, at the end of the day, everybody is concerned. Even if the number of people who will be actual victims is low, the concept of disaster potential is very important.

2. Acceptability

I am sure that you are familiar with acceptability. This is an approach that shows that a risk can be low quantitatively, but from a qualitative perspective it is completely unacceptable. There are about 20 attributes here that mean that a particular risk is either acceptable or not. Is it a risk that you take deliberately or is it being forced upon you? Let us take the Chernobyl situation. Will the risk be manifested immediately after you take a chance or do you find out 10 years later?

Another parameter that I think is very important, particularly in terms of GMOs, is whether the risk is fair or unfair. In other words, are the people generating the risk exposed to it or are they dissociated from the people who actually bear the risk? We have modern and traditional food-related risks. If we idealise things a little, this has to do with the production and consumption of your own food. We all know that experts usually say that food-related risks have never been so low – they mean from a quantity point of view. However – and I will not go into detail here – if we look at the traditional food risks, they are usually on the right side and are so-called good risks. Modern food-related risks in connection with prions, pesticide residues and GMOs and so on have all the attributes of so-called bad risks. I have produced a grid that we can use to ascertain the concept of acceptability, as there are various levels of risk, some of which are acceptable and some of which are not.

3. Irreversibility

The third parameter is irreversibility. If I make a decision at a particular point in time, the risk will decrease at varying degrees of speed. However, if I make that decision later, there is a so-called good risk from a reversibility point of view, meaning that at any time the risk will decrease accordingly. If I postpone my decision, it does not increase the irreversibility of the risk. Conversely, if I put off my decision, I have less control over the situation – and that is the situation that we are dealing with in Mad Cow's Disease. In other words, the irreversibility factor is low.

III. The Uncertainty of Risk

1. Plausibility

We also need to characterise the concept of uncertainty, and here again I will use three different parameters: plausibility, reducibility and observability.

Looking firstly at plausibility, when you are dealing with a phenomenon, the very existence of which you doubt – and that is why things are different from probability, where you do not doubt the existence of the phenomenon, but just wonder about its frequency – what you can do is ask yourself how much information you have. Is there a lot of literature or reports published on the subject and so on? How is the information processed? In other words, how much consensus is there between the various experts in terms of how we should interpret this information? We have a lot of information and everybody is in agreement, and we then have a traditional situation where there is a lot of certainty. However, we can also deal with other types of situations.

There are different kinds of controversies and the question is how they should be ranked. The controversy has to do with the very validity of the data. A former Research Minister said that all the data on climate change could be disputed, so the debate is about the very validity of the data. . You can then also have controversy on the scientific models that you will use. It all depends. Are we talking about dissemination of GMOs? Is it geneticists or biologists who are talking? Who is talking, basically? Are they conservationists or environmental groups? Depending on where you are coming from, you will have a different interpretation. All this, of course, has an impact on the concept of precision, for example, the confidence interval, and the extent of climate change and cross-pollination with GMOs. That has an impact on how you calculate the precision level.

There is therefore a very first situation. There may be very little information and this leads to violent controversy, and here we have the example of water. There was a publication on water, and cold fusion also made headlines at one point in time. However, you then have intermediate situations. For example, there is so-called consensual uncertainty. In other words, you do not have a great deal of information. Nevertheless, there is a convergence between experts. In 1996 and 1998, with mad cow disease and other such prions, there was very little information or experimental data and very few publications on the subject. However, experts gradually converged and said that we needed to admit that the idea of the prion being transmitted from cow to man was more and more plausible. There are also other cases that AFSSET is very familiar with and a lot of literature on the subject, such as the impact of electromagnetic waves. Yet there is still a lot of controversy, despite the extent of the literature. You can therefore draw some curves for this, and these curves are equi-positive, and you can signpost the very concept of plausibility, using a two-dimensional plot.

2. Reducibility and Observability

As regards the other two parameters of reducibility and observability, as we saw earlier, there is a wide range of situations that go from one point to another. These range from several models being possible, with very few parameters, all the way to a so-called radical lack of determinism. No matter how much respect we have for research, just doing research does not mean that you reduce the level of uncertainty. You either test models or you need to estimate your parameters better. However, in some complex situations, all you can do is take a better look and identify the phenomenon better. Nevertheless, targeted research is unlikely to reduce the level of uncertainty. Reducibility is therefore actually the possibility of reducing the level of uncertainty within a short period of time, using research.

This then raises the issue of vigilance. If we strengthen vigilance, does it mean that the phenomenon can therefore be observed? With the well-known example of GMOs, will GMOs lead to more allergies within the population? I believe that current vigilance systems indeed detect the global signal, but they have a very hard time ascertaining the causes of a potential increase in allergies.

IV. Applying the Impact and Risk Parameters

Going back to the initial question, of the various possible decisions that public decision-makers can make, some are of an informational nature – setting up a vigilance system and targeted systems and warning the population. You then have regulatory decisions, such as restricting usage. If I introduce my six parameters – and I am not putting them in at random – we can see that some parameters will push actions of an informational nature, where we can strengthen vigilance and launch a research programme and, where if it is a serious matter, the research agency at the national level will be motivated on the issue. If the acceptability is good, the social perception of this risk will not be too problematic.

At the other end of the spectrum, where rapid decisions need to be made because the irreversibility may deteriorate, when the plausibility becomes strong and the observability is a major element, there will be a move towards regulatory action.

When we were at primary school and wanted to know where the centre of France was, we used needles and cardboard maps – and that is similar to the principle that we are going to use. We will use the levels of impact and uncertainty and plot a small polygon of the risk analysis. I used the case of Mad Cow's Disease because in 1996 and 1998, we were in a similar situation. Acceptability was very bad, because it looked like a bad risk – as I mentioned earlier – and even if we had targeted research, the non-conventional vehicles were quite disturbing for biologists. The observability was bad. We knew that there was a deferred effect and even if we were to strengthen vigilance and surveillance, it would take years to observe the extent of the phenomenon. Epidemiologists at the time said that there would be between 75 and 140,000 deaths, so the range was not very good. The situation could deteriorate rapidly in terms of irreversibility, not to mention severity, and experts were saying more and more – and this was the key phrase for AFSSA – that they regarded the transmission to human beings as being admitted.

My barycentre holds the regulatory actions, which is a typical situation where you have to make hard decisions on soft science. We demonstrated that it was necessary to take regulatory actions, even though we were in a situation of high uncertainty.

V. Conclusion

I could mention further examples, but to conclude I would like to say that the possible interest in this exercise is to have a kind of stability and style of management for decision-makers where stability is a function of their situation. A decision-maker may be quite sensitive to the issue of irreversibility and will say, for instance, that he will need to make sure that the person who replaces him will not have a situation that has deteriorated. He will favour irreversibility and will not be as sensitive to social acceptability. He will be an enlightened technocrat. At the other end of the spectrum, there may be a decision-maker who is highly sensitive to the social acceptance of the risk, and he can plot his own diagrams, with the criteria he wishes to favour. He knows that if he adopt this decision-making diagram on one criterion it will be useful to him.

This is therefore a tool that I am happy to put forward to address uncertainty and help make decisions. It is a decision-making tool and those of you who assess risks know very well that the link between assessment and risk analysis is not deterministic.

Paul FRIMAT

Thank you for looking at all this in a mathematical and algorithmic way and for defining a number of terms. You have made the distinction between informational action and regulatory action, and this is a useful tool. Through it, we might be able to decide which direction we should take.

I would now like to invite Denis Bard, a physician and professor, to talk to us about the uncertainties in the assessment approach to health risks.

The Assessment Approach to Health Risks

Denis BARD

EHESP

I. Background

Good morning. I would like to thank R2S and AFSSET for inviting me to speak and for organising this symposium. We have a very interesting agenda and, additionally, the symposium is being held at the right time, as we will see through our discussions. It is essential that the symposium is being held today.

The organisers have asked me to revisit this basic decision-making tool of the assessment approach to health risks in the field of environmental and health risks. This is not a new approach, dating back to 1983, which is some time ago. It is made up of four steps and I will look in detail at each step and at the uncertainties related to each phase. The first step is the identification of danger and how we can establish a causal link between a chemical or physical agent and a noxious effect on health – and this is in line with the previous presentation. We will see the different variables there.

Secondly, once we have identified the noxious or hazardous agent, we need to know at which level it is dangerous and what the severity and consequences of the agent are. This is therefore about assessing the link, dose and response, and it is also essential to know whether there is a dose threshold below which there is no effect.

The third step, where there are uncertain answers to the first two steps, is to assess exposure within the impacted or exposed populations. The fourth step, which is the summary of all this, is the impact that Bernard Chevassus au Louis mentioned. For various reasons, you need to check the past and the situations of exposure, such as the nuclear fallout from Chernobyl in France or, from a regulatory perspective, making projections based on an industrial facility and seeing whether it goes beyond acceptability limits in terms of impact.

II. The Four Steps to the Assessment Approach

1. The Identification of Danger

a. Experimental data versus epidemiology

Let us therefore look at the first step of identifying danger. Traditionally, and very often, there is a base that is made up of the results of experiments obtained from animals, and it is quicker and cheaper, in most cases, to have experimental data from animals rather than carrying out epidemiological studies. However, there is radical uncertainty here. This is the predictive value of the data obtained from the animals transposed on the human population and even if we know the action mechanisms in detail, we see that it is observed in animals and human beings, which is quite seldom, in my experience, and this radical uncertainty remains. We can therefore never be sure that what we observe in animals is valid in humans. However, epidemiology can lead to a final decision because it looks at human populations.

The problem is that we have the epidemiological data but, as I said, it is costly, takes time and is uncertain – and I will look at this in detail. I am an epidemiologist, of course, and we may have some discussions with the experimentalists and biologists, but the causality evidence is

brought forward, according to the epidemiologists, by the epidemiological studies, provided that we have convincing causality arguments and evidence – and I will look at that in detail. As well as the causality evidence, there is an observable effect aimed at reducing the impact. These circumstances occur quite seldom, but in the case of air pollution, we were faced with quasi-experimental situations where we could observe during the Atlanta Olympic Games in 1996 or in Dublin in 1990, where the use of coal for heating was forbidden and air pollution dropped to a very large degree and we observed a very rapid decrease in heart and respiratory mortality caused by air pollution. There are further examples of this and we have started to explore these. For instance, the recent ban on smoking in public places in several countries, including France - although we still need to quantify it there - shows that within a few months of the ban on smoking in public places coronary mortality dropped.

b. Defining causality

I have taken two examples relating to air pollution and tobacco, but it is of course much more difficult to assess the results when you have, for instance, a multifactorial cancer disease. We rarely have the possibility of observing the impact of an intervention. What do the epidemiologists do to conclude the causality? They apply an approach. I am not going to talk about a matrix here, but they have various pieces of evidence or arguments that are put forward to establish this causality. In modern epidemiology, we refer to the proposal of 1765 that put forward this articulation. An association between an exposure and an effect is regarded as causal if there is a set of positive arguments. [Inaudible] you should not talk about criteria, but viewpoints. When you say criteria, it is as if you are able to weigh those criteria, and there is a causality because of this weighting factor or you do not choose the causality because the weighting is not there. It does not work that way. There is an uncertainty on this causality in epidemiology as well.

The first argument is the existence of a strong association – what is called a relative risk. For instance, we see that the fact that being exposed to ionising radiation increases very significantly the risk of leukemia and we observe the same phenomenon when looking at cancer of the larynx in uranium miners, where there is a very high risk. Another argument, which is not specific to epidemiology, but which is inherent to the scientific approach, is the replicability of results – and I will return later to the throat cancer of the uranium miners.

The specificity of the effect is therefore one criterion or argument, and we always have to check. If we take the example of the thyroid cancer from the Chernobyl accident, this type of cancer has increased significantly in France. That is perfectly true. However, if we look at the sequencing and whether the cause comes before the effect, we see that the increase in this type of cancer started well before Chernobyl and the rise in the incidence rate is due to improvements in diagnosis. Another criterion is that there should be a link between the dose and the effect and there are other criteria, such as plausibility, which has already been mentioned, consistency with the acquired knowledge, the analogy principle and the experimental piece of evidence. If we revisit the association of ionising radiation and leukemia, we can see that the argument is positive for almost all criteria except the specificity of the effect. The ionising radiation is not the only cause of leukemia, although there is no doubt that there is a causality link between ionising radiation and leukemia. Conversely, for throat cancer, we may argue about it and the strong association is of interest to epidemiologists. However, this causal association was put off because we observed a strong rise in the number of throat cancers among uranium miners in France, but not in the other countries. Significant risk is therefore not enough to establish this link between cause and effect.

2. Dose/Response

a. Thresholds

Let us talk now about the uncertainty in terms of the relationship between the dose and the response. The first uncertainty is the quality of data available and the second uncertainty is of an epidemiological nature – is there an effect threshold? The only way of answering that point is to say that for all possible noxious effects, there is a threshold of action below which nothing happens. However, there is one exception to this – the geno-toxic, carcinogenic agents, where there is no threshold. However, this is an epidemiological choice. These principles are basic principles and they help us organise the approach. However, there are exceptions, such as formaldehyde, which is a geno-toxic, carcinogenic agent. Nevertheless, everyone agrees that there is a threshold here.

Conversely, an effect that does not give rise to cancer and where we do not see any apparent threshold is in the relationship between the dose and the response with regard to the effect of lead on the neuro-behaviour of children, measured by intellectual quotient. We therefore have an overall framework and there is no threshold for the geno-toxic, carcinogenic agents, with just one for the others. Nevertheless, it is just a framework.

b. Defining a low level

What will happen if the dose is at a low level? In order to protect ourselves, we need to define the exposure value below which there is no effect, or there is an effect which we may regard as being negligible, although we need to be extremely cautious here. It is not up to the risk assessment officer to say what a negligible effect is and perhaps after this symposium we might be able to understand better who the legitimate person should be who defines this negligible effect.

To have this protection, therefore, we need to start from observable data, and from the observable data, we will need to make decisions and choices on what we cannot observe. The further away we are from the observable fields, the more uncertain we will be.

c. Choosing the principle of action

How, then, are we going to choose this principle of action? Once we say that there is a threshold, most of time, whether the data are available or not, we are going to take experimental conditions. The determination of a threshold will therefore be very often based on observations under experimental conditions. However, is sensitivity sufficient, given that we want to mitigate this uncertainty? There is unavoidable statistical uncertainty in terms of threshold, because we have population constraints. Often, we may observe that for a given substance there is a threshold of action at 10 micrograms per kilogram of body weight, with groups of 10 animals. However, if you were to move from 10 animals to 100, what would happen?

In terms of a model without a threshold, here we change our language. Using epidemiological data - although we also use animal data - we model the response relationships, and the modelled observations are compatible with an effect up to zero plus ϵ . The extrapolation models that we use give rise to major deviations in the assessment of risk at a very low level.

For example, in an animal experiment on dioxins, we had three models - [inaudible], logistical and [inaudible]. Without doing any statistical tests, which are too complicated, we can see that it is quite consistent. However, where it is a very low dose or an extrapolation, we see that in the relationship between the dose and the response, the dose slopes are extremely different. It is not possible to make a choice on purely scientific criteria. It is done on the equations but several models are compatible with the data and the choice is made on the most

pessimistic models, which is better in terms of protection. Alternatively, it is not a pure statistical model and incorporates a series of biological considerations, which is more satisfactory. However, overall, a choice needs to be made.

The initial choice of the principle of action with a threshold versus without a threshold depends on the availability of data, as I have already said, and the quality of the data and their relevant experimental species. With the example of dioxins, there is very old data and a very approximate approach to toxicology. Dosing kills half of hamsters that were treated with almost 1,200 micrograms per kilogram, but for [inaudible] the same dose is 0.6 microgram per kilogram. You can therefore see that there is a huge discrepancy in terms of the relationship between the dose and the response. Which, then, is the most relevant? How can we say that the hamster is closer to man? Of course, the choice is going to be completely different. We are going to make a judgment on the quality of the data that are available and on the relevant experimental species.

3. Assessing Exposure

With regard to exposure, this is traditional sequencing. The ideal thing would be to have the environmental experimental data direct. You often do not have that and you therefore have to model exposure with a number of variables or data that you might have. There will be data on emissions and we will therefore have to model what happens between the source of emissions and the experimental data, knowing that this modelling is not certain as regards the different variables, such as the accumulation in the environment, the channel of exposure and so on. There are also meteorological questions – do we have the right meteorology and is it precise and accurate? In terms of the source of emissions, for example, with an incinerator I use a super three-dimensional (3D) model and validate it in situ somewhere in the centre of France. I validate it because I have data in the field and nothing proves to me that if I did it in the northern part of France it would be the same.

III. Conclusion

In conclusion, therefore, at every step of the way, as you assess the risk, there is uncertainty. At the end of the day, the risk assessment approach is an old one. However, it is still an approach that is operational when it comes to providing an ordered and systematic framework for qualifying and quantifying uncertainty. Risk assessment is a vital approach and is instrumental in facilitating the decision-making process. However, we of course need to base ourselves on scientific data as much as possible, and make a decision and pass judgment at every step of the way. This is therefore an object that is so-called trans-scientific.

Paul FRIMAT

Thank you. It is true that this approach, which combines epidemiology and is the assessment of health risks, is important and you correctly outlined the importance of points of view as opposed to criteria, as well as looking at the predictive value of everything that you achieve and read about, bearing in mind that the decision-maker will wonder about thresholds, and that models with and without thresholds are possible. However, it is also difficult to interpret the various observational models and the recent examples you gave us show that, as part of the scientific approach, there is some lingering uncertainty and even subjectivity. For symposia like today's and as part of Scientific Committee meetings held in health and safety organisations and agencies, it is very important to have a multidisciplinary approach and to trade our experiences.

I would like to hear now from Sylvio Funtowicz's replacement. Sylvio has been delayed and Pierre Benoit Joly from the Institut Français des Relations Internationales (IFRIS) will stand in for him. Pierre Benoit is also a member of the Institut National de la Recherche Agronomique (INRA).

Remarks for Discussion

Pierre Benoit JOLY

IFRIS/INRA

I. Summary of the Two Presentations

Thank you. I am delighted to introduce this discussion, but we have heard from the first two speakers and they are a tough act to follow. To save time for a full questions and answers (Q&A) session, I would simply like to discuss two things. Firstly, I have a question. This is an underlying question that has run through the two presentations we heard. How do we factor uncertainty into the risk assessment process? Additionally, by way of introduction, I would like to go back to the place that is occupied by the stakeholders, including social scientists and politicians.

I will be very quick, but of course that means that I will have to simplify things as much as possible. In the two presentations, we heard about two different concepts and approaches in terms of uncertainty. Denis Bard talked about uncertainty as something that could be calculated – and I really enjoyed his presentation. We saw the wide spectrum of arrangements, standards and agreements that are necessary to change the level of uncertainty and ensure that it is no longer radical and that you can turn it into something that you can calculate and factor in in the risk assessment. I will not paraphrase what Denis said, but I would just like to stress what he talked about in terms of the inferential methods. How do you extrapolate whether you are talking about the dose/response relationship or when you extrapolate pre-clinical results to human beings? You need to bear in mind that when there is a threshold and an LMR or a ratio, it is the result of a calculation, and the calculation brings into play a number of agreements. Some people might call this a black box.

II. Understanding Uncertainty

1. The Standard Risk Assessment Method Focused on Prevention

The first presentation talked about uncertainty as being more radical, and here we need to focus our attention on situations where we do not know what will happen. We do not know the various conditions of the world, particularly in terms of the standard framework. We do not have experimental, statistical or scientific proof of a causal relationship. Uncertainty is therefore defined on the basis of an absence of scientific knowledge and on certain cause to effect links, which take pride of place.

As I said earlier, it all boils down to knowing whether we can turn radical uncertainty into something that you can calculate, so that you can factor uncertainty into your standard approach in terms of risk assessment - and I am sure that we will have further opportunities to discuss this as part of the symposium. Perhaps because we asked the presenters to focus specifically on the quantitative methods, the two presentations have provided a positive answer to the question. However, to tell the truth, I really doubt that that is the case. If I may say so, when you introduce uncertainty into traditional risk assessment models, as Denis Bard has done, the risk would be that you exclude all the quality-related impacts, for which traditional science does not have consolidated data. It seems to me, therefore, that the standard risk assessment method remains deeply focused on prevention.

2. The Multi-criteria Approach

In fact, Bernard Chevassus au Louis has proposed an alternative method, which I think is very interesting because it is part of a broader effort to address the lack of knowledge. When you are using a traditional risk assessment model, how do you deal with a lack of knowledge? You have a multi-criteria approach and there is a measurement system for each single criterion, and here we see clearly why this approach is useful.

3. *Le Cygne Noir* by Nassim Taleb

The question we then need to ask ourselves is this – what price do we pay as we switch to a quantification method? I would like to refer here to a book written by Nassim Taleb, called *Le Cygne Noir*. This is a book on uncertainty and the unpredictable. Nassim Taleb warns against what he calls ‘platonicsim’ – in other words, where you take a model and think that it is reality. You therefore crush reality and its very thickness and fabric – all of its phenomena and unpredictability, which is inherent. You crush it using a model, which in terms of management and how to anticipate people and stakeholders’ actions takes the place of reality. The question is how do we ward off risks like that, which are traditional by nature? We can see the potential gain if we try to create a model for such radical uncertainty and unpredictable factors.

III. The Role of Stakeholders in Risk Governance

1. The Psychometric Paradigm

What role do stakeholders play in terms of risk governance? The two presentations used two different approaches. Bernard Chevassus au Louis introduced social sciences via the idea of acceptability and based himself mostly on the results achieved thanks to the psychometric paradigm, and I believe that it is important to pursue this avenue further and to branch out. There are three different points that I would like to make here.

Firstly, in terms of acceptability, the psychometric paradigm defines acceptability as an attribute which is specific to objects. There may be controversy on the subject in terms of social sciences, but you could put forward the argument according to which the risk is not simply attached to objects but based on relationships – the relationships between various entities. The Institut de Radioprotection et de Sûreté Nucléaire’s (IRSN’s) barometer study shows this very clearly. The perception of risk is deeply associated with the climate of confidence that exists between the various agencies. We may or may not feel that agencies are actually telling the truth and so on, and that has an impact. There is therefore another aspect in terms of acceptability and it has more to do with the relationship between the objects than the objects themselves. Various international benchmarks have shown to what extent acceptability is not just related to objects, but to political cultures, institutions and systems and so on as well.

2. Conditions of Implementation

Secondly, again with regard to acceptability, it has to do with the conditions of implementation. When there is uncertainty and action is needed, the stakeholders in charge of implementing the necessary measures need to be convinced that those measures are useful, and this can sometimes pose problems. We could demonstrate that one of the problems with the whole mad cow disease adventure was not the lack of scientific knowledge and had nothing to do with acceptability by public opinion, but was about the implementation of the measures. Measures were put in place as early as 1988 and 1989 in Great Britain, but they

were not implemented and there was no monitoring until 1996. The question is therefore the following. In terms of risk assessment, how do you factor in all the management and implementation conditions? This then raises the issue of the border between assessment on the one hand and management on the other.

3. Available Knowledge

Thirdly, the state of uncertainty is, of course, based on the available knowledge, and the available knowledge is based on the interplay between various stakeholders. There are three different kinds of phenomena here. For example, there is the strategic manipulation of uncertainty. We know that in some cases, particularly in the US, certain stakeholders are trying to put forward a number of scientific studies that shake the scientific consensus and rock the boat. With climate change or the impact of carbonated soft drinks on health and obesity, for example, there are difficulties in terms of access to data, and that is hard to solve. The question is not how much information we have, but how to make it accessible and available. This information is left in the hands of just a handful of stakeholders – industrial players, for example – and we need to look at how we can encourage them to divulge the information they have.

4. The Production of Data and the Interplay Between Stakeholders

Lastly, it is about the production of data on the one hand and the interplay between stakeholders on the other, particularly in terms of environmental health. There is a wide spectrum in terms of knowledge and information and you therefore need a monitoring and assessment system that will leave a lot of room for early warning systems. For example, if unconventional information emerges – unconventional vis-à-vis the traditional framework – the way you detect it and bring more value to the appraisal and expertise systems is by bringing into play other types of experts.

Questions and Answers

André CICOLLELA, Fondation Sciences Citoyennes

Denis Bard gave us a presentation that looked a bit like the one we heard at a symposium on risk assessment in Metz in 1996. It brought me back at least 10 years. This is a useful advance, but risk assessment problems today cannot be looked at in the same way as before. Endocrine disruptors have to come into play and we can no longer see the dose/response relationship as being linear. We need to take into account all of the technological and scientific advances. I am not saying that risk assessment is unnecessary, but we need to change the paradigm against which we are asking these questions. We need to factor in the latest scientific advances.

Denis BARD

You are absolutely right. However, we need to present the tool which is at the very heart of today's risk assessment method and decision-making process in terms of environmental health, and this tool needs to take pride of place once again. We know that there is some level of uncertainty and it is not just based on quantity. Uncertainty comes from all the decisions you make every step of the way. Perhaps we need a paradigm shift in terms of endocrine disruptors – why not? However, I am still waiting for this new paradigm. I do not think that endocrine disruptors, as an issue, are well delineated. It is a pell-mell term, encompassing lots of different things, and I am really sorry that the term is being used everywhere you go. I

simply do not have a mechanistic point of view with regard to endocrine disruptors. I think that this is a very specific issue that is about the way cells and estrogen receptors work, but we of course need robust scientific foundations for a new paradigm shift.

It is true that this idea is being materialised and there have been a number of publications, but it is still in its infancy because the methodology is posing problems. It is all well and good to welcome a paradigm shift, but we need the scientific arguments to back it up. However, you of course always need to pass judgment and make decisions every step of the way. We need a decision-making tool which, no matter how you look at it, will remain so-called trans-scientific.

Daniel OBERHAUSEN, Activist

I would like to sound the alarm with regard to exposure to electromagnetic fields. I am very interested in symposia like today's, which bring together hard and soft science. However, let us be honest – how hard is hard science? My association is composed of activists and we work in the field. Some people call us troublemakers, but we believe in having a rational approach. There are a number of aspects to our work, but we believe in rationality and do not rank among those who like to spread panic.

There are three points that I think are very interesting, particularly with regard to threshold effects. In terms of electromagnetic impacts, as a physicist, I was wondering why people protest against mobile telephony when they never thought of traditional terrestrial Hertz waves. People seem to wonder why they are coming under attack. With natural exposure, frequencies between 1 gigahertz and 10 gigahertz have extremely low cosmic noise within that window, and I wonder whether the threshold concept should not be addressed with the utmost caution with regard to electromagnetic radiation, particularly with in terms of the paradigm.

There is some confusion regarding the interaction between thermal effects and the environment. At the time of Chernobyl, there was a lot of emotion and public opinion around the world, and the number of thyroid cancers grew. In terms of mobile telephony, I think that what we are trying to do is to ensure that a particular configuration and a particular key scenario is rejected. Low doses of non-ionising radiation are dangerous. If you look at what happened with the National Association of Security Dealers Automated Quotations (NASDAQ), it was quite brutal. I think that social and economic sciences have a fundamental role to play.

There is then a third very interesting point with regard to gambling theory. Some organisations are very familiar with this theory and employ a lot of actuaries – insurance companies and reinsurance companies. In terms of electromagnetic nuisance and disturbance, reinsurance companies have shown their ignorance, but they have been very cautious. This risk is, of course, non-quantifiable, yet they have decided not to cover electromagnetic risks in their reinsurance policy.

Paul FRIMAT

As an occupational doctor, I welcome the participation of every stakeholder in this discussion. Thank you for trying to be very clear in your question and presentation.

From the floor

Bernard Chevassus au Louis talked about the multidimensional problem and did so in a very interesting way. However, with regard to the interface between science and decision-making, decision-makers focus mostly on plausibility, while others focus mostly on acceptability.

How do we do this in practical terms? What structure, approach or process should we use, when we try to bring together two different visions of the same truth? Analysis is all well and good, but in practical terms how do we move forward?

Bernard CHEVASSUS au LOUIS

Thank you for raising those very important questions. In the traditional risk assessment paradigm, we have the evaluation phase, the management phase and the communication phase – so there are three different steps – and according to the guidance manual from 1983, there should be functional separation between all three steps. Decision-makers in France have transposed this and call it a structural separation. This has not been written down, but that is another story.

We need to address the requirement to reconcile assessment and management, as well as management and communication, in this process. However, where we need to dig deeper is in terms of strategy. We need strategies and to have no regrets. There is a wide spectrum of uncertainty, so there are things that we absolutely must do. What about learning strategies? Will the decisions that we make today help us collect the relevant information? We know that there are interesting things under the Precautionary Principle. For example, we should not dissociate acquisition of knowledge in the decision-making process. On the contrary, we need to work on the two different fronts at the same time, because this will cause a shift in the level of uncertainty. We do not have enough time to go into detail here, but I think that we need to totally revisit the whole principle.

Pierre Benoit Joly has addressed the issue of how to assess management, but how do we manage assessment? There are therefore new aspects that we need to add to the whole risk assessment system when trying to factor in uncertainty. We need a new paradigm shift.

Simon GALAS, Centre National de la Recherche Scientifique (CNRS), Montpellier University

Going back to what was said on endocrine disruptors, which change our working assumptions slightly, I would like to go beyond this and the low doses of endocrine and ask about the trans-generational impact? Are things already being done? Is this a consensus for research programmes? Has this aspect already been taken into account or do we need to wait longer?

Participant

I take your point, but I do not think that there is anybody in this room who can answer that question. We are not just talking about endocrine disruptors, and it is not just there where we are starting to observe a trans-generational impact. As regards how it works, we need to take a look at it. However, there are two substances that show either paternal and/or maternal transmission. This is absolutely a problem that we need to address. There is a recent highly documented publication on the impact of paternal transmission, which was published last year by Sylvain Cordier.

David GEE, European Environment Agency, Copenhagen

I have two specific questions. Firstly, I enjoyed Bernard's framework and I wonder whether it would be helpful to add explicitly the issue of the distribution of impacts across groups, regions and generations to the three dimensions of severity, reversibility and acceptability. That then brings in very much the politics and the economics to the area of social sciences and it is the distribution of impacts that itself has a big impact on the process of evaluating and dealing with risks.

Secondly, I would like to thank Denis for reintroducing Bradford Hill's famous nine features or criteria for moving from association to causation. However, a problem that he pointed out was the asymmetrical nature of these things. In other words, if the nine features are present, you can move with some confidence from association to causation; if they are absent, you cannot move with confidence to say that there is no causation. They are asymmetrical criteria. Bradford Hill pointed that out then and the gap between the symmetries has widened considerably now because of our knowledge of complexity and multi-causality. If we take consistency, for example, if there is consistency across research results, it is a robust piece of evidence that helps you to move from association to causation; if you do not have consistency, it is not very reliable at all to use the absence of consistency as a reason for denying causality. I think that this point about the asymmetry, which has widened since 1965, is rarely brought out when dealing with these things, and I would like your view on that.

Bernard CHEVASSUS au LOUIS

Very briefly, you have perhaps two possibilities. Firstly, you can consider it to be part of the severity or you might introduce a new parameter, such as equity or something similar.

Denis BARD

This is a difficult question. Again, Bradford Hill spoke about viewpoints, not criteria, and I think that that is key. It is a matter of judgment. I am not sure that I fully share your point on asymmetry. In any case, just to caricature things, for instance in the case of leukemia and ionising radiation, we have a set of positive arguments, and this is still a matter of judgment for a group. It is not about one single epidemiologist in the calm of his office saying that he has a sufficient set of positive arguments that conclude that there is a causal link. I think that that is the first important point.

The other point we need to consider in this example of leukemia and ionising radiation is one of the most documented. In the field of environmental health risks, the picture is generally much more complicated and it is necessary at some point to say that there is probably, or possibly, a causal link. However, this is still a real matter of debate in the broad field of science since Culpener in the 1930s up to now, when you have brilliant US epidemiologists, such as Kenneth Rothman, who argued about the problems of causality in such a way that they were considered by others as supporting an anarchistic theory of knowledge. There is therefore room for discussion on causality in epidemiology.

Paul FRIMAT

It is now a pleasure for me to introduce the second part of the morning session, where we will concentrate on political sciences and human sciences, next to the epidemiological and mathematical approaches that we saw in the first part. The organisers of the symposium asked Robert Hoppe, from the University of Twente in the Netherlands, to talk about scientific uncertainty and the political structure of risks. Robert will therefore be our first speaker in this session.

The Political Structure of Risks

Robert HOPPE

University of Twente, the Netherlands

I. Background

1. Key Thinkers on the Politics of Risk and Uncertainty

a. *William Beveridge*

Thank you for that introduction and for inviting me to speak here today. I have to say that this is by some way the largest audience that I have addressed in the last couple of years – I am much more used to smaller seminars and conferences.

I will talk about risk and uncertainty and the difference between politics and analysis of risk and certainty. I think that the previous speakers did an excellent job in painting a picture of the analysis of uncertainty and risk assessment, so I will look at that very quickly and will therefore be talking mainly about the politics of risk and uncertainty. I will do so partly through the authority of two very famous political thinkers and policy analysts. One is William Beveridge, who most people will know as one of the founding fathers of the European welfare state, although he did so particularly for Great Britain in the last years of the Second World War. This distinguished policy analyst and political figure differentiated between power, which he defines as the ability to give orders to other men and force by sanctions – man has power when he can mould events by an exercise of will – and influence, which is changing the actions of others by persuasion – an appeal to reason. It is obvious when talking about the analysis of risks and uncertainty that you are in the field of influence, were you to follow Mr Beveridge.

b. *Bertrand de Jouvenel*

William Beveridge is not the only one ~~make~~ to set out this kind of difference. Bertrand de Jouvenel, who is probably well known to most of you, in his theory of pure politics also made that kind of distinction, although he stressed particularly the nature of power as being the central ingredient in any type of politics. However, he said that the working of words upon action is the basic political action, which actually means that he thinks that persuasion, which is words after all, is also one way of doing politics. Nevertheless, he says that politics is essentially a matter of collective will formation, which itself is a matter of instigation and response. The instigation/response relationship is the core of politics and it means that politicians always want to spark off contributory actions by others, and contributory actions, occasionally, are not just support, but also indifference – you can do what you want, I will not oppose it. That would be another form of instigation. He also stresses what he calls ‘that capital feature of the political animal’, namely the propensity to comply, and that is also something that needs to be kept in mind as a very important part of politics.

c. *Aaron Wildavsky and Hecklo*

Following these two great stars in their fields, there are other political scientists who have reproduced these kinds of things – and I was trained as a political scientist and not a medical

doctor or epidemiologist, although I turned to policy studies later and I am now in a group that looks at science technology and policy studies, so the relationship between knowledge and power and between knowledge and politics is my topic. These people include Aaron Wildavsky, the famous American political and policy scientist, who talks about the differentiation between cogitation, which is basically analysis; and interaction, which is about power relationships; and Hecklo, one of Wildavsky's co-authors of ~~in~~—a couple of well-known books, talks about puzzling and powering, which is something I like because I think that power is not something that you have or exercise, but a relationship that means that you work with it; it is a verb. Like thinking or know~~ing~~ledge, it is not necessarily something that you have or a body of knowledge; it is dynamic and something that you do: you puzzle.

d. *Bent Flyvbjerg*

I have worked previously with this distinction where I have said that there is something like *judgment*, as the deliberate ~~of~~ design ~~of~~ evaluation of policies; and *will formation*, ~~and as or~~ decision-making. ~~Regarding~~ ~~and~~ implementation, ~~and~~ decisions are mediati~~nged~~ between thought and action and between policy preparation and policy implementation. More recently, the Dane, Bent Flyvbjerg, ~~from, I believe, the University of Aalborg~~, has written a book on rationality and power, which I also think is very enlightening.

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I am therefore not alone in making this distinction, although I realise that there are those, especially in the sociology of science or the sociology of technology, who now adhere to a kind of seamless web model of politics and science where they do not make that distinction any more. I think that it is still worthwhile to make the distinction and focus exactly on the transactions and the boundary between the two. That is therefore what I will do in the rest of my talk.

II. The Analysis of Uncertainty and Risk

1. The Scientific and Analytical Context

The previous speakers made it very clear that if you want to make politics rational – and that is what you want to do if you are talking about uncertainty and risk – you will try to separate the rationality part from the political part in the process architecture of uncertainty analysis and risk assessment and risk analysis. Basically, what the previous speakers were saying was that you establish the context and identify the risks, you then analyse them in terms of likelihood and consequences, combine them, either by sheer multiplication or other ways of judgment, and you then assess the risk and prescribe particular treatments or measures. You then, of course, start monitoring and reviewing them, preferably by Bayesian statistics analysis, and adapt your theories later.

The question, in fact, is what do we know about uncertainty and risk? First of all, we know that there is this difference between an analytical and a political context. The analytical context is the scientific way of constructing risk and uncertainty. We therefore talk about rationality and there is a discourse of sound science and the practices of sound science. We talk about probability calculation and false and positive negatives and the ratio between them. We look at frequency distributions, particularly historically constructed frequency distributions, and we see learning as a game of skill and capacity-building and gradual error elimination. I think that that is a fair summary of the scientific and analytical context, and I will briefly ~~explore~~ it by looking at it from van Asselt's typology of scientific constructions, although it basically brings up all the things that the previous speakers have been talking ~~about~~about.

2.The Political Context

However, there is also a political context, which is a context of practitioners and people who have experiential knowledge – stakeholders and politicians, as well as their staff, who are usually bureaucrats, who think in terms of power and power relations. They think in terms of having to make tough choices under time pressure and of acceptability and accountability - to a Parliament, for instance. They do not necessarily argue in terms of probability calculus, but in terms of plausibility reasoning and plausibility heuristics, which is a much looser type of reasoning than the strict logical argumentation in probability calculus. They also think in terms of ex-ante expectations - not necessarily looking back, but looking forward, through scenarios and design. Learning is a matter of coping capacity and somehow making risks and uncertainty governable and controllable, or at least giving it the semblance of controllability. Error prevention is much more important than error correction or elimination, because if you have a couple of hundred or perhaps thousands of deaths, you are wrong as a politician. You will have made wrong decisions and will be held accountable for them. They therefore want to prevent that.

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3.2. The Political Structure of Risk and Uncertainty

I will explore this political context, which is about narrative and storytelling, through Ravetz's typology of the narrative, in terms of the political structure of risk. Again, very briefly, on the typology of sources of risk, I will just reproduce what van Asselt, who is now a member of the Dutch Scientific Council for Government Policy, has written in her dissertation on this. She says that there is basically uncertainty due to variability. This has a number of causes, which produce a number of problems with models, data and so on, which produces unreliability and structural uncertainty, and that basically enters into the uncertainty, due to a lack of knowledge and is filtered into policymaking and decision-making processes. This filtering means that there are particular types of uncertainty that are political. There is uncertainty about goals when there is inherent uncertainty in the models and there is political uncertainty, as we have seen, because there are all kinds of judgments that need to be made, either through political agreement or negotiations that enter into the political decision-making structure, which are frequently unrecognised, even by the politicians themselves. There is yield uncertainty in the sense that the costs and benefits are unclear because the models are not specified sufficiently and there is action uncertainty because the models frequently do not cover all the systematic possibilities of action or action alternatives.

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Comment [H1]: At this point in my talk I presented two PowerPoint slides (##7-8) with Van Asselt's typologies; without them the argument becomes not understandable. Is it possible to reproduce the slides here?

What we usually see in political decision-making is a focus on one, two or three decisions, which only incrementally differ from the status quo or the existing situation. This is basically a matter of coping with uncertainty in a political way. There is also a connection between the two, which I will not go into now, and we know that there is a connection between the different sources of uncertainty and particular scientific methods for dealing with them, such as hedging methods, formal scenario analysis methods, probability-based methods, Bayesian statistics-based methods and so on, which we heard all about in the previous talks.

Comment [H2]: Here I showed slides ##9-10.

III. The Narrative Aspect of Uncertainty and Risk

1. Ravetz's Views on Interpretive Policy Analysis

If we move from the analysis to the politics of uncertainty and risk, as I have already said, we are moving out of the field of calculation and into the field of the narrative and storytelling. Politicians and stakeholders, as well as policy analysts in translating scientific data to politicians in such a way that they can understand, somehow have to transform data, models, frequency distributions into stories. What, then, are these stories?

There is a lot of theory on this and it has basically been thematised in the policy sciences under the title of interpretive policy analysis. I will not go into this deeply, because I would then have to cover a lot of theoretical material, but will just use one particular typology of the major characters and a typical cast in narratives on risk, which has been produced by Jerry Ravetz, who has written about this on several occasions. While it is quite complex, it is still worthwhile looking at [it](#).

Comment [H3]: Here in my talk I presented the Ravetz Typology of types of narratives in risk governance (slide #11); could it be reproduced here?

Firstly, he says that in the particular roles that are prominent in any risk narrative, there is an *insider and an outsider perspective* and a perspective which says that the incumbent role of the policy actor is to act on behalf or as *part of a collective* or that the policy actor more or less acts *alone or in isolation*. Moving through this typology, if you are an insider actor acting as part of or on behalf of a collective, your political role is that of a *risk regulator*, which means that you are usually an administrator. Scientists have a particular role to play here as monitors, inspectors or technical experts. In terms of Funtowicz and Ravetz's theory on different ways of doing science, the idea is that you just do normal applied science and therefore act on the basis of received scientific wisdom.

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Looking at the insider role, but where you are basically acting on your own, you are a *risk imposer*. This could be, for instance, the nuclear industry or a GMO producer. You act as an entrepreneur and the scientist's role then differs and shifts towards advocacy and expertise and being a consultant, adviser or research expert who acts within the research policies of these usually commercial enterprises. The scientific rules then change. It is partly normal applied science, but it also becomes part of professional consultancy, dealing with slightly more complex issues.

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Looking at the outsider role, you may be a total *risk-rejector*. For instance, you live under the flight trajectories of Schiphol or Charles de Gaulle airports and you do not like it. The action type is a campaigner and here too scientists play a role. They are critical scientists or conceptual or value clarifiers, and sometimes they are called in as discursive mediators and have a role to play there. Again, the scientific rules shift to a higher level of complexity. It remains partly inside the boundaries of normal professional consultancy, which is still considered normal; although, there may also be a move into the post-normal sphere of doing science. The same goes for the outsider in the isolated situation. You *are a risk endurer and, culturally, and* a survivor, and there is a particular role for science here as well, which is entirely post-normal science.

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2. The Different Problems in the Risk Field

a. Structured problems

If you look at the evidence, I believe that there are different types of problems in the risk field – although it is not only in the risk field – because any problem is a conjunction of two things. It is a conjunction of consent on values and certainty on a particular knowledge base. Knowing that there is a problem means that you need particular knowledge and facts, which are compared with particular normative standards. This is a very interesting area. The concept of a problem straddles the fact/value distinction, which is so crucial to any type of knowledge and science, and basically combines spheres that cannot be combined epistemologically, because we are always told that they have to be kept distinct. However, politics deals with problems and is largely a problem-processing process.

In terms of task fields and political epistemology, there are different situations. Firstly, there is the case where you have high certainty on knowledge and high consent on values. This is what I call 'structured problems' – there is no problem with the problems. The idea here is that you can delegate the problem to a professional community, which by way of analysis and

Comment [H4]: At this point in my presentation I showed slide #12. This one may be skipped here, but slide #13 is truly indispensable for a good grasp of my argument. Could it be reproduced here?!

instruction learning, learns how to tame the problem. Pre-natal screening of pregnant women, at least in the Netherlands, for example, is considered to be a ~~'tamed domesticated'~~ or fully structured problem.

b. *Where the knowledge base is uncertain*

You then have a kind of in-between case where there is consensus on norms and values, but the knowledge base is contested and uncertain. You do not know everything or perhaps you do not know a lot. This means that you have to negotiate about the risks and the distribution of the risk and who is responsible for what and who is to shoulder particular risky burdens. Although you can also have problem-diffy~~en~~red research in order to reduce uncertainty, if that is possible. Definitions of medically required care and hospital budgets, as well as tackling obesity, would qualify, I think, as this type of problem.

c. *Where you know what to do, but there is low consent on values*

There is another in-between situation which is different from the previous one, where you have low consent on values, although you know exactly what you need to do. With abortion, for example, through the ages people have known how to provoke an abortion. The only issue is whether it can be done in an assisted way by a medical doctor. The same goes for euthanasia and, now, preventive embryo selection. Here, accommodation strategies or conflict management strategies are the politically prudent way of dealing with ~~these kinds of~~ problems.

d. *Unstructured problems*

You then have the totally ~~'wicked'~~ or unstructured types of problems, where there is a kind of chaotic, variety-selection type of learning, which is purely evolutionary driven, or garbage-can driven, as others would say.

What you see, therefore, is that from a political ~~perspective~~idea, there are very different task fields and political environments where you have to process particular problems. Sometimes you can delegate things to a professional community, as in the structured case, but in other cases you have to do a very agonistic type of wild politics, as in the case of unstructured problems, where agenda setting and priority setting and the fight about the definition of the problem is still going on. We may have some cases of this here in mobile telephony and endocrine disruptors, judging from people's responses.

3. The Prevalence of Politics Over Analysis and Risk Policies

I think that all this means that in a very surreptitious way, covertly or overtly, politics usually trumps analysis and risk policies. It weighs more heavily. You could basically say that the political framing of problems and types of policy politics hangs together, where there is a correspondence between them, and they trigger particular ways of boundary arrangements between science and politics and science and policy, and these boundary arrangements trigger allowed or proper roles for science and scientists and appropriate methods for uncertainty and risk analysis. This means that the political process in general generally prevails, even though it might be very difficult to detect where exactly it trumps the analytical part, because it is basically in this congruence dynamic between the political framing of problems and the types of policy politics and the way that politics is creeping into particular policy domains of risk analysis and risk assessment.

There are particular cases of this and ~~--~~possibly contrary to what Jasanoff would actually do herself ~~--~~; I would say that her book, *Designs on Nature*, ~~in a way~~ betrays this dynamic between analysis and politics. Firstly, she says that there are culturally stable narratives that

Comment [H5]: At this point I showed slide #14, which is also crucial to the argument and should be reproduced here if people are to see the gist of the argument.

trigger problem framing and policy politics in the field of bioethics – and I use cultural theory terminology here to talk about these issues. She uses the term *'monsters'* to describe entities that threaten disorder by crossing the settled boundaries of nature and society. Here, she is talking about assisted reproduction, stem cell research and genetically modified crops and food, but she could also be talking about cyborgs and enhancement medicine, such as bionic ears and eyes and so on, which cross the border between technology and human beings, which Latour also talks about. She says that in the United States the idea is that you embrace these kinds of hybrid constructions and you have a lot of decentralised norms. Whereas, in the United Kingdom, there is a kind of controlled admission or assimilation of these monsters, but you have them in a centralised way. In the Federal Republic of Germany, it is all forbidden, simply because they see it as being too analogous with Nazi-style euthanasia and racial cleansing problems. There, there is therefore also a law-like centralised norm.

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These culturally stable narratives lead to different boundary arrangements and risk strategies. In the United States, innovation and risk is market-regulated, where there is a kind of winner-take-all settlement of controversy. There is usually exposed judicial accountability and sound science at the bar, as she would call it, as well as a strong opposition against the Precautionary Principle – they ~~should dsimply do~~ not want it and see it as contravening trade and the economy, as well as science.

In the UK, innovation is much more expert-regulated. Controversies are consensually settled and there is much more ex ante Parliamentary and administrative accountability, with science-based expertise, but in an independent but trust-based way. There, they embrace quite a broad notion of the Precautionary Principle. In Germany, it is different again.

What we see, therefore, is that national and cultural differences and political regime shifts and differences lead to different forms of framing the risk problem and dealing politically and procedurally with risk and risk analysis. (The are ~~also~~lots of other cases, which I will not go into here.)

IV. Bringing About Better Governance

1. Handbooks and Guidelines of Little Help

I will conclude my talk by asking what can realistically be done about better risk governance. People usually think in terms of better guidelines, Government rules, handbooks and methods, and more transparency is one of the slogans. Usually, this means standardisation. I think that this helps a little bit, but it will not go very far, partly because I have been involved in an effort on this for the Dutch Natural Environmental Assessment Agency and have written about the different types of problems and tried to make people aware that these different types of problems require different types of risk assessment and so on. It turns out that they are not using it at all. I had a dissertation written on it and basically there was a negative outcome (De Vries, 2008). This is not just the case for Holland; there is also the case of the Environmental Protection Agency (EPA) in America. Handbooks and so on do not really work. This also means that the idea of enhancing an ethic of reflexivity, where basically you have contingent guidelines, with different guidelines for different situations, may help a little, but it is not essential.

2. Usefulness of Fast Enhanced Trial and Error Learning

However, I do not believe that that means that methods can do nothing. The improvement of methods is possible, for instance by what I would call fast enhanced trial and error learning. Trial and error learning is the basic policy way of doing things, and we already saw it in

Lindblom in the 1950s and 1960s. However, with his co-author Woodhouse, at Rensselaer Polytechnic, he improved the whole idea by saying that if you start out with the Precautionary Principle, but generally relax it when knowledge is accumulated and leads to new insights, you can have an enhanced trial and error learning process. However, this needs to be organised well because it does not happen by itself, automatically.

3. Insights in Psychology and Working with Dialectical Heuristics

I also believe that there are a lot of insights on risk in psychology that are still unused, and these should be used in political decision-making. Additionally, If you take the four ideal typical risk roles: the risk-regulator; the risk-engineer; the risk-rejecter and the risk-imposer, you can work with the dialectical heuristics, with the book by Mason and Mitroff called *Strategic Assumption Surfacing and Testing*, already in 1994, and use these dialectics between these four roles [distinguished by Ravet](#), and map all the differences that exist much more specifically.

4. The Key Requirement for More Dialogical and Horizontal Accountability Processes

However, first and foremost, I believe that more checks and balances are needed with stakeholders, as well as with concerned and informed citizens, so that more dialogical and horizontal accountability processes are created, which basically means that we need to improve the politics and procedures of dealing with risk. Of course, substance is also very important and you cannot have procedural decisions without any substantive knowledge, and in that sense methods and analysis remain very important. However, we have underdeveloped the political and procedural way of dealing with these issues and they have become more and more important. It is about whether or not you believe in a risk society or not.

Paul FRIMAT

This is indeed a broad-ranging subject and political science comes into play in terms of risk assessment. At the end of the day, we need to mull over the need to bring together all the stakeholders, as you rightly said in your conclusion, so that everyone can be empowered. As a doctor, I keep saying that people need to be in the driving seat in terms of their own health and it is therefore important not to sweep the issues under the carpet of scientific complexity. We need to explain clearly to people, who are having a hard time interpreting the situation, that their interpretation is biased. That is a starting point for dialogue.

I now give the floor to Soraya Boudia, who is a Senior Lecturer at the University of Strasbourg, where she teaches sociology, amongst other things.

Risk Uncertainty and Governance of Low Doses: A Historical Perspective

Soraya BOUDIA
University of Strasbourg

I. A Changing Concept

To begin with, I would like to go back in history and talk about low doses. This is a problem that is being raised more and more for a number of health and environmental risks, but what we will see is that it is not a new problem. The issue has been discussed for the last 50 years and the interesting thing is that the issue of low doses has always been addressed by combining the two concepts of low doses and uncertainty. What are the lines along which the issue has been discussed and what kinds of answers have people come up with, and what has the scientific community and the regulatory authorities come up with in terms of solutions to the problem?

50 years down the line, let us stock-take. We need to bear in mind that the problem is far from being solved. Let us talk about the terminology first of all. Low doses is a problem that is being put forward in a number of reports, but when you look at the reality that is covered by this concept you realise that the concept has changed over time, which is only normal. However, behind the concept of low doses, you find attributes that can be very different from one group of stakeholders to another, as well as in terms of the conditions of use.

I will not try to be absolutely accurate from a scientific point of view here or completely exhaustive, but I would like to give a number of examples. The concept of low doses obviously emerged in opposition to high doses. However, it is also a problem that crops up for effects that are not directly observable and can only be seen through epidemiological studies, and it is also a problem that crops up when you try to qualify or describe so-called ordinary exposure – all the things that everybody –people like you and me – are exposed to. Additionally, we have introduced the interesting concept of very small doses. The issue of low doses has been to qualify natural exposure to radioactivity in certain areas, for example – all the natural exposures that apply to the general population.

II. The Development of Understanding of the Issue of Low Doses

1. Vast Changes from One Period to Another

The history of low doses is based on technical and scientific expertise being tested and scientific experts have grappled with the issue for a long time. They have tried to come with a number of answers, as we shall see, but it is not a problem that is easy to grasp, for various reasons. A number of scientific studies were mentioned this morning, so I will not dwell on those. From a historical point of view, the very first time that the issue of low doses was raised was in the late 1940s and in the 1950s, the problem was officially formalised, as it were.

In terms of frequency, there is a timeline. I am a self-respecting historian and if I put everything into a timeline it means that I can reduce complexity – I try to cram everything into those three periods. However, the message that I would like you to take home is the following. If we cram everything into a timeline, we will be able to see the vast changes from

one period to another. There was a period during which we stated the problem scientifically and another period when we tried to address it politically. What, then, have been the changes from one phase to another?

2. The Setting of Threshold Values in the 1930s

I hope that you will understand why I chose this particular method. I divided the timeline into three different periods, starting in the 1950s. However, before I talk more specifically about the 1950s, I would like to say a few words about the ways in which we address non-infectious health risks. Here, I am talking about health and environmental risks and how we have addressed those problems since the 1930s. We addressed them against a backdrop of what we call a threshold paradigm. In other words, we have a general vision of things and we set limits – exposure thresholds – for various substances. This approach was designed and formalised officially in the 1930s in the US. The National Conference of Governmental Industrial Hygienists, created in 1938, was the very first agency to put forward threshold values, particularly in terms of exposure in health at work. That was a starting point.

Since 1938 – and we will see to what extent this paradigm is being discussed and even being called into question and compared with other paradigms related to low doses – we can safely say that the terminology around the definition of thresholds and limit values has been brought into other areas. I will not go into detail here as you are probably more familiar with these areas than I am. Sometimes, there is competition between the various regions and countries in terms of the terminology, but I would simply like to say that this threshold-based paradigm will be discussed and called into question as we address the issue of low doses.

From the late 1930s, therefore, up to today, it has not been smooth sailing for this particular paradigm in terms of the setting of threshold values and limit values and so on, and this has obviously sparked scientific discussions. We had an example of these discussions this morning when we talked about the reasons why there is uncertainty in terms of how to set those standards, but that is also subject to political controversy. When we set standards for health at work, it all depends on whether you are exposed to the risk yourself and you will address the issue differently depending on whether you are a victim yourself.

3. Ionising Radiation in the 1950s

What is interesting here is to see to what extent we call this paradigm into question. First of all, we have the example of ionising radiation, which was also something we discussed this morning. Ionising radiation, and radioactivity in particular, takes pride of place in the history of low doses. This is an old problem. However, it is also a problem that has been documented and it is set against the backdrop of the major controversies that erupted in the 1950s on the impact of atomic tests in the atmosphere. At the time, a number of Governments tested hundreds of bombs up in the atmosphere and there was a major public discussion on the impact of this radioactivity. As part of the discussion, a number of scientists were involved and one of the scientists, Edward Lewis, was working at the California Technology Institute (CALTECH) – and a lot of this story obviously takes place in the US. Lewis was a geneticist and he tried to summarise a number of studies, in particular a number of epidemiological studies on the impact of radiation. He combined exposure to high doses to what could be called low doses.

At the end of the 1950s, the very first proposal emerged, with a proposed model for a dose/effect relationship. It is no wonder that this proposal came from a geneticist because it is in the field of genetics that the issue of thresholds and threshold values was the most

discussed. Genetics had gained ground in the late 1930s, when a lot of studies were done and many publications were issued. At the time, geneticists worked with flies, producing thousands and thousands of flies, and some of their experiments boiled down to ionising the flies, where they bombarding them with radiation. It was very difficult technically-speaking to measure the actual doses. However, regardless of the doses, they observed genetic mutations in the flies, which showed that mutations were possible. You could therefore not say that radiation had no impact and this approach was therefore taken up again in the area of radio-protection.

4. The Question of Thresholds and the Linear Dose/Effect Model

How do you protect people from ionising radiation? This was all structured as early as the 1930s and 1940s – between the two World Wars - and was based on threshold values. Following World War II and this debate sparked by the atmospheric atomic tests, and based on the work carried in the field of genetics, as early as the 1950s a number of scientists, who worked in the area of radio-protection, pointed out that even if you set thresholds, you could not say that doses below the thresholds were completely safe. As early as the late 1940s, they had proposed a significant modification. There was no longer talk of so-called permissible, reliable or tolerable doses; this said that thresholds were being set, but that there was no assurance that doses below the thresholds were safe and that there was no risk whatsoever – you simply considered the risk to be negligible.

This occurred in the 1950s and, in parallel, Lewis carried out a summary of all the previous studies. In the summary, he proposed that the relationship between the doses and the impact should become a model. He believed this basically to be a linear model and, based on the work carried out by geneticists, he said that there was basically no threshold. This approach remains the reference for regulatory authorities and was taken up by the International Commission on Radiological Protection (ICRP), and other international regulatory authorities, with the aim of finding justification for the issue of thresholds. You therefore set a threshold and, at the same time, know that you cannot be 100% sure that there is no risk under the threshold. However, if you consider that the linear dose/effect model can be applied, you therefore justify thresholds as being the most conservative measure. That is, if you use the model, you will be sure that you are taking into account the most pessimistic case scenario.

5. Work on the Justification of Risk Taking

a. Radio-protection

However, as early as that period, a number of scientists pointed out the fact that a certain number of outcomes and results simply did not justify the adoption of a linear dose/effect relationship model. There was a whole series of papers in the 1960s that discussed other potential models for summarising the data. However, the discussion was extremely controversial when it came to justification. What is it that justifies taking a risk, no matter how negligible it is? How do you justify taking a risk?

I would like to share with you excerpts from two reports that were published six years apart. How do you state the problem? Firstly, in the late 1950s, people were aware that they were taking chances, but they thought that the risk was negligible. At the same time, that does not mean a lot – how negligible is the risk, from what point of view and for whom? People tried to justify this approach based on the relationship between risk and benefit, as well as cost and benefit. The cost/benefit methodology gained a lot of ground in the 1950s and 1960s, and in the mid-1960s another idea was introduced. This was an idea whereby in the area of

decision-making, when you have to make decisions in terms of risks, then the data that you have is not enough and you have to take into account economic and social considerations in order to make that decision.

A report on radio-protection said that this was a problem that continued to be controversial and asked how you make a decision. How do you decide what it is that you need to take into account in terms of economic and social considerations? We can still use the term 'economic and social considerations' today.

What can we say about this approach to low doses in the area of radiation? Very briefly, regulatory authorities approach the problem in this particular way because they dealt with a number of problems. In particular, if you admit that there is a 'risk', that means that you need to avoid all the activities that can generate that risk. However, what does that mean? We are talking about the development of the nuclear industry and that simply could not be called into question for those experts. In other words, they needed to strike the right balance between geopolitical considerations, energy considerations and the need to protect human health – the health of populations. From this particular point of view, therefore, the nuclear case is a very specific one, although this way of stating the problem also emerges in other areas.

b. Food

The only area where the approach is slightly different is food. In the 1950s, there was a number of discussions on adding supplements to food, even in low doses. Did those additives not have an impact on human health? These were chemicals. At the time, the chemicals were the subject of a major discussion in Congress, where there were a lot of hearings. This was the only case where a proposal was put forward for a ban on anything that people wanted to add to food that did not ensure a zero level of risk. Those of you familiar with the history of risks in relation to food are probably familiar with this, but it is the only case where the zero risk factor was raised.

It was, in fact, a scientist who initiated this. He was in charge of carcinogenic substances at the Cancer Institute in the US and was an expert in occupational health and occupational cancers. As he could not impose his idea, according to which carcinogenic substances would be banned entirely at work, he thought that it might be easier to use food instead for products that were not absolutely indispensable. Food conservatives and food additives are now pretty much everywhere, but it all started at that time.

III. The Regulatory Approach

1. The Banning of Cyclamates in 1969

In terms of the regulatory approach, how has this been applied? In fact, it remained a dead letter up until 1969. It was only in 1969 that the idea was implemented and cyclamates were banned. That was a very important juncture in time and was the first time that the issue of low doses was raised.

2. A Growing Awareness of Environmental Health Concerns

However, nothing was solved; this was still an open-ended question. The controversy was renewed significantly in the 1970s in the area of radiation. Kaufman and Templin were two scientists who were atomic researchers with the Energy Commission and therefore working on US nuclear regulatory affairs and their work had been financed for a number of years, with a focus on low doses. In the early 1970s, they publically declared something that sparked controversy, when they said that if you went by existing standards for radioactive

effluence, it would lead to an additional 32,000 cancers a year in the US. Can you imagine the impact of having two scientists - and Kaufman was very well known at the time - declaring publically that existing standards were generating thousands and thousands of additional cancers? This sparked a huge controversy in the early 1970s and gained further ground, particularly since it was a very specific period in time. At that time in the 1970s, environmental concerns started to gain pride of place in public discussions, ahead of the Stockholm Conference, for example.

However, here again in the US, there was a change and a growing awareness in terms of environmental concerns. For a long time, people in the US had thought about the environment in terms of the need to conserve resources. However, health then became more and more important, and everything to do with environmental health became more and more important. This was then raised in Europe, where there was a lag and it only arrived in Europe many years later. Environmental agencies were created in the US and there was a whole community that focused on environmental health. Thanks to this new framework, therefore, the issue of low doses took on further importance. It was raised not only in the areas of radiation and food supplements and additives, but also in the area of chemicals.

3. The Work of the US Scientific Academy and Others

I would now like to speak a bit about the work of Rachel Carson, who wrote a book that introduced this discussion and played a very important role. The whole issue of low doses was raised not just for ionising radiation, but also for pesticides, which were very important in this discussion. This was therefore the area of pesticides, but also of chemicals, more generally speaking. It is very important and is something that I think we need to discuss.

When you try to address the issue of pesticides and chemicals, you borrow a number of aspects that have been stated in the discussion on ionising radiation, and it really was ionising radiation that had a structuring effect on the discussion and provided embryonic solutions to problems that emerged in other fields. Those answers were brought in in the early 1970s by a committee from the US Scientific Academy, which still exists today. The committee first of all produced a landmark report in 1972 on low doses in ionising radiation, and included other areas as well, which put forward scientific conclusions pertaining to uncertainty of data and problems with the methodology - which is something we discussed this morning, and there is uncertainty in the methodology used to collect the data - and pointed out the fact that the scientific approach cannot be the only approach when you need to make decisions in areas where those problems are raised. The scientists therefore obviously believed that further scientific studies were needed to provide a better foundation for the problem. However, even if there were more studies, it would not solve the problem of knowing what kind of decisions needed to be made and what methodologies should be used when the time came to make decisions regarding these activities. The scientists then called for reflection on the decisions taken in uncertain and risky situations, and this gave rise to a thinking process in the US, where several agencies and committees were involved, resulting in the so-called Red Book of 1983 and risk assessment as we know it today.

When you examine the work of these committees - which I have done - you really see the difficulties. They were fully aware of the fact that there were uncertainties at all levels and judgments of value, as well as a political component in the methodology that they had developed. However, what can you do in those circumstances? That is the problem. The methodology that was developed was not regarded as the final and best one, and another report was done on risk assessment in 1994 to take stock of the situation 10 years after the Red Book. Since then, the American agencies have organised workshops on the methodology.

4. Developments in the 1990s

a. Cross-border activity

In the 1990s, there were a lot of cases and the cross-border area took on more of a role. All the thinking took place at an international level and you could then observe major national differences. As was said this morning, the issue of low doses became quite prevalent and it is interesting to address the low doses issues from an effects point of view. The generic mutation and carcinogenic effects have been addressed for some time, but since the 1990s, the focus has been on the repro-toxic effect and in approaching and classifying things in three major categories. Since 1995, this idea has been developed significantly with the support of international organisations. This type of approach had existed in the United States and there had already been some reflection on it in Europe, but the real initiative was taken by the cross-border institutions.

As was discussed this morning, the epistemological studies took various approaches, with epidemiology, toxicology and mathematical modelling, and cultures can therefore be quite different. It all depends on how you address the topic and you will not produce the same type of scientific facts, even if we can agree on a number of variables.

b. The multiplication of structures of expertise and assessment

Secondly, as regards the environmental and health risks, [inaudible] the classification of carcinogenic agents, one problem that we can highlight is the multiplication of levels and structures of expertise and assessment. Over the last 30 years or more, we have seen a lot of expertise. Sometimes, this does not mitigate these uncertainties and it might even increase the degree of uncertainty because one organisation may not agree with another.

c. Continuing controversies

Thirdly, in risk assessment, there are some controversies and a few things are not clearly stated. Things are underlying, but not explicitly explained. There are economic considerations and some associations and trade unions know that. I am not criticising the fact that we take economic considerations on board, but there is something which is not explicit and decisions are justified in scientific and technical terms. However, in the expertise committees, when we are interested in what is discussed behind closed doors, the horizon is broader and the [inaudible] of course play a major role. We therefore need transparency and we should clarify the parameters on which decisions are made. It is desirable to have this transparency, but that sparks off other problems, such as political issues.

d. Different types of uncertainty

Fourthly, as we demonstrated several times this morning, uncertainty is a term that has a number of different definitions and there are different types of uncertainty. There is technical uncertainty, scientific uncertainty and political uncertainty.

Paul FRIMAT

Thank you. You talked about the concept of thresholds and whether you were a toxicologist, physician, sociologist or legal expert, and those concepts need to be discussed at length. As an expert in the area of health and work, I have discussed the concept of a threshold [inaudible] or not, although I was exposed to two. This concept of threshold is therefore both negative and positive, as it all depends on the categories of people. Setting a toxicological threshold as a purely legislative one is easy to do, but difficult to manage.

I now give the floor to Claude Henri.

Remarks for Discussion

Claude HENRY

Sciences Po/Columbia University

I. The Nature of Uncertainty

Both presentations were extremely interesting and a good source of stimulation, and as a panellist I therefore feel at ease. However, the presentations were also different from each other. It would not be opportune for me to highlight specific questions for the discussion, although there was a key issue that linked both presentations, which is also key for our symposium. You said that you were interested in the historical explanation of uncertainty, so I will start with a bit of history.

I was a physicist before I became an economist and am old enough to have had the opportunity to talk to Werner Heisenberg about the way he achieved this principle of uncertainty, in full contradiction to the traditional physics that he learned at university. At the beginning of the 1920s, two economists, Frank Knight in Chicago and Knight's colleague in Cambridge, John Maynard Keynes, were writing books. Knight's book was famous, while the other one disappeared. It was called *The Treatise of Uncertainty*. Both writers were making a clear-cut distinction. If we had been aware of those works, we would have agreed that the principle of Heisenberg could be called a risk principle. This principle started from quantum mechanics and in spite of its complexity and exotericism, the science is extremely clear: everything is done in terms of objective probabilities. You disintegrate a core kernel and you cannot say what is going to happen to the kernel. You disintegrate 800 million and on the basis of quantum mechanics you predict statistically exactly how they will behave, and the statistical distributions that you are going to observe are really in line with the forecasts.

We can say, in a way, that atoms explain risks – although that is ludicrous, of course. The cells leave more uncertainties, and if you deal with ecology or climatology, they are full of uncertainties and the probabilities announced are subjective and often deceiving. Climatology, ecology and possibly some other sciences of direct interest to you are therefore not certain, in the profound sense of these uncertainties, as distinguished by Keynes.

II. Characterising Uncertainty

Can uncertain sciences be reliable? If the answer is no, then we, AFSSET and climatologists will all find it quite difficult to make progress. However, how can we answer the question? How can we characterise an uncertain, yet reliable science? We can show that those are both sides of the same question? How can we give rigorous content to the Precautionary Principle?

Over the last 10 years, significant progress has been made in trying to provide an answer, both qualitatively and quantitatively. These answers try to show that the results depend on the nature of uncertainty. As we saw this morning, there is a broad range of uncertainties and it all depends on the degree of reliability of the science. It cannot be characterised only – and here I agree with Professor Hoppe – by one science politics intervening.

The British physicist and scientist, John Ziman, wrote a book which I would strongly recommend: *Real Science: What It Is and What It Means*. The book was written by an

outstanding physicist and science historian and I have extracted two sentences. Firstly, 'Give us a way to characterise the uncertain, yet reliable science.' The credibility of science depends as much on how it operates as a collective social enterprise as it does on the principles regulating the type of information that enterprises accept. We could say that AFSSET and JEC, the Intergovernmental group on climate study, meet the criteria of those structures generating an uncertain, but reliable science. The Bush administration denied this process, which delayed the problem at an international level.

In his book, Ziman reminds us of a very famous sentence by Thomas Kuhn in *The Structure of Scientific Revolutions*. Kuhn wrote: 'Normal science is a mindset that can take hold of researchers in almost anything'. Normality is what we could call a comfortable approach of the reliability of an uncertain science. However, it is too often the comfort of routine to the detriment of innovation. I do not know about the science of nature and of life, but I can tell you what it means in terms of climatology. However, when I say that I can tell you, I can only quote two major stakeholders of this science – one on the nature side and the other on the economy side. The first one is Dr Pachauri, the chairman of IPCC and the recipient of the Nobel Prize, and the second is Lord Nicholas Stern, the author of *The Stern Review on the Economics of Climate Change*.

If you met them every six months, what would they tell you? They would say that they were not happy with things and that an important point had been set aside in their last report or that there was something they did not take on board in their last paper – not because they had ignored it, but because they thought that the point was not well enough established. As this type of thing is a frequent occurrence in climatology, you realise the dynamics and the delay there might be. Politicians and public opinion moves even more slowly and you can therefore imagine how difficult the task is.

Questions and Answers

From the floor

My question is for Mr Hoppe. In my association, we try to apply a rational approach to the questions of the proper level of exposure concerning ionising radiation from mobile phones, but it is very difficult because facing us and our actions is the industry, and the industry has a tremendous amount of money. This money-making process is based on passion and they run a lot of advertisements for the cute, little mobile phone, especially at Christmas. We are therefore sometimes in despair because we want to promote a rational approach. However, we are facing the industry – and what do they say? They say that they have experts – and some of these experts work for the industry and handle the rational questions. It is therefore very difficult for the associations.

Robert HOPPE

I suppose that the problem is in defining rationality. It is indeed well known, in the field of mobile phones, as well as in nutrition and tobacco and smoking, that a lot of scientists are not only working for companies, but they are also in so-called independent agencies that make their money by creating uncertainty, in the sense that they permanently try to cast doubt on experimental outcomes of other scientists if they are not in the interests of tobacco or mobile phone companies and so on. I like what Flyvbjerg says when he states, 'Power has a rationality that rationality does not know, and rationality, on the other hand, does not have a power that power does not know'. What he is basically saying is that those in power, whether they are business interests or political majorities that define and impose their reality on others,

can also define what is reality, in the sense that they can decide which experts to believe and which not to believe. That is one of the reasons why I think that it makes sense to differentiate between powering and puzzling. Unfortunately, the two are strongly connected and that has important consequences for the way we need to deal with these kinds of issues, which is one of the reasons why I said at the end of my talk that more checks and balances were probably more important than more rational and analytical schemes and methods of puzzling.

Paul FRIMAT

It is therefore important for experts to be independent and have real independent agencies, and there is a real problem there.

Yannick BARTHE, Centre of the Sociology of Innovation

I would like to thank Soraya for her presentation and for summarising such a long period of time in just a few minutes, which is a very difficult exercise. Soraya described the scientific controversy, but radiation in the US is linked to the judicial field and has to do with repair and not risk prevention. What about the legal sphere and the judicial field and our field? Are they interconnected? I think that the legislation has moved away from the dose and dose-effect relationship to the benefit of the presumption of this causal link between radioactive fallout and the inhabitants of Nevada or Utah or other American states. A scientific debate takes place in the Court of Justice and we then feel that suddenly they are moving away from that to respond to political pressures. Could I have your view on that?

Soraya BOUDIA

You are quite right. The judiciary plays a major role in the United States. Experts are involved on both sides and there will be a discussion. However, I would be cautious regarding the repercussions of the judiciary on the way we design the topic of low doses and its evolution over time. If we examine what happened in the 1950s and the 1970s in a more detailed way, the judiciary only intervened in the second step. The public debate took place in the media and among scientific experts and the reports that came out were produced before legal proceedings took place.

How are we going to deal with the problem with such and such a plant or such and such a worker or being under such and such an environmental exposure? In that case, the judiciary will play a major role. With the scientists on the one hand and regulation on the other, provided that there is still uncertainty, there will always be room for legal proceedings. What pushed the Environmental Protection Agency to set up risk assessment was the legal proceedings, but within the EPA a number of directors have a given vision of what the regulation and expertise should be and those directors will mobilise the legal proceedings to orientate the situation. The directors therefore play a role, but in the historical analysis you have to look at the institutions from the inside, in terms of what can be said and done. The experts generate solutions and sometimes these solutions are for the politicians. In the US, the national framework has played an adversarial role and there are controversies. However, the solutions drawn up in the American framework will be exported through international bodies to countries where legal proceedings are not so essential. The type of solutions will therefore not downsize the role played by the judiciary, although we sometimes give this too much attention.

Paul FRIMAT

I think that we can say that the principle of uncertainty and risk exists and, as Mr Henry said, we need to be careful that the problem of normality does not become a routine, because it is typically French. My thanks to all our speakers.

Our next session is a bit different. We have designed it as an attempt to introduce this afternoon's five workshops and we have three different topics and four different speakers. We will talk about the various kinds of uncertainty and the mobilisation of stakeholders, and our first speaker is Henry Rothstein.