

PERSPECTIVES ON 'THE LENS OF RISK' INTERVIEW SERIES **INTERVIEW WITH NICK PIDGEON**

Bob Heyman and Patrick Brown

ABSTRACT

This article is the first in a series which will appear in 2012 in the special mega-issue 'Health Care Through the Lens of Risk'. It provides a quasi-verbatim transcript of an interview with Nick Pidgeon, one of the main contributors to the social science component of the Royal Society Risk Report (1992). The interview contains a fascinating insider account of the debate about risk between engineers and social scientists who produced the report. It also offers some important reflections on the fissure which has opened up between risk sociology and research concerned with global and local system safety.

INTRODUCTION

At the suggestion of the editor of *Health, Risk & Society*, Andy Alaszewski, Patrick Brown and Bob Heyman have undertaken a series of interviews with a sample of leading risk social scientists, including Andy Alaszewski, Judy Green, Nick Pidgeon, Ortwin Renn, Paul Slovic, Peter Taylor-Gooby and Joost Van Loon.

The interviews will be presented across the series of special issues on *Health Care Through the Lens of Risk* which will appear in 2012. They cover views about: the meaning, if any, of the term 'risk'; the significance of the Royal Society (2002) *Risk* report; the history of risk social science; and the impact of risk thinking on the wider society and on government policy. All of these topics will not necessarily be covered in every interview. Instead the most interesting material will be selected. The chosen interview content will be presented more or less verbatim, with a few contextual clarifications and interviewer comments added either in square brackets or footnotes, with '...' used to mark small omissions. The interviewees were given the opportunity to lightly edit the text and to add references which are presented in footnotes. The order of presentation has been varied from the actual interview sequence which was variable and free-flowing.

The interview series will start with Nick Pidgeon who was first author of the chapter on 'risk perception' in the 1992 Royal Society *Risk* report. The interview contains a fascinating insider account of the debate about risk between engineers and social scientists. It also offers some important reflections on the fissure which has opened up between risk sociology and research concerned with global and local system safety.

THE INTERVIEW

Nick Pidgeon with Bob Heyman, 27/10/11

How do you see your own contribution to risk social science?

Nick: I started ... doing work on the causes of major accidents in the 1980s, from a human and organisational perspective, [around the question] 'What does it mean for

physical systems, or more accurately socio-technical systems, to fail?'. And it was an extremely influential period for me personally, working alongside an engineer, David Blockley, at Bristol and the organisational sociologist Barry Turner who was then at Exeter. The project suited me because I had always resisted being categorised into a single discipline, and it also taught me to listen carefully to the risk languages being used in different disciplinary perspectives. And risk is always a bit like that as well. So I worked on organisational safety for a number of years, but then gradually drifted into the risk communication and public perception domain. In methodological terms, I'd always done mixed methods research too, as I am always very comfortable with that approach myself, although sometimes it's a bit difficult to write it up. Many of my empirical projects now have both a quantitative aspect and a qualitative aspect because each tends to illuminate different aspects of the phenomena under study ... sometimes without actually writing all of it up as a whole, because it's actually very difficult to write up mixed methods work for the peer-reviewed literature. So you find lying behind many of my papers are projects which have got more than one data stream coming in, helping to inform the overall theoretical analysis. But sometimes they don't always end up in the same empirical paper together ... A very good example here would be our recent work on the 'reluctant acceptance' of nuclear power.^{i, ii}

Bob: That thing about mixed methods and the difficulty of getting published - have you any further comments on that? So what you are saying is you felt some pressure to divide up, and do a qualitative paper and a quantitative paper?

Nick: It's easier to do that. It's not a pressure, it is just conceptually easier.

Bob: Oh, conceptually easier,

Nick: Well, it can be conceptually and in terms of what the journals want.

Bob: That's what I thought you meant.

Nick: Yes. I am not sure that I would put *Health Risk Society* in that bracket because I think they are pretty comfortable as is the *Journal of Risk Research*. But with the journal *Risk Analysis*, you would struggle to get good qualitative studies in, and overall there are very few mixed methods [papers] anywhere. It's really a length problem, it's often as simple as that - to get all aspects of mixed methods work properly integrated into a concise paper is tricky. One paper I am particularly proud of was on foot and mouth disease in 2004ⁱⁱⁱ, which was subsequently highlighted by Alan Bryman the social sciences methodologist in his review of mixed methods research. That was after the outbreak appeared suddenly, and then we completed a survey very quickly, not the best survey in the world, but it was good enough, in Norwich (which was not near the outbreak) and one of the severe outbreak areas in Cornwall. And then we followed it up with focus groups in both locations afterwards, and did write quite a nice paper putting both parts of that study together. So you can - sometimes it works. It was neatly contained the whole thing. So it can be done.

Bob: Was there any particular reason for your shift from looking at systems to looking at public perceptions, other than it just happened?

Nick: I think a pragmatic response would be there were funding opportunities. There were more funding opportunities because there was the post-Seveso^{iv} research agenda - there was European funding available as a result for risk communication and emergency response type projects which I got involved in very early, in the 90s that would be. And those would have been my first proper research projects to be involved in - funded research projects. And then there were several HSE^v projects which were more on the public perception side, and were HSE's response to the social sciences research agenda set out in the Royal Society report.

Bob: ... Is that partly, being very cynical ..., because the powers-that-be who dish out the money would be more happy about it - 'And we have got to understand how the public perceive it, perhaps to educate them or whatever', whereas looking at the systems actually -

Nick: I think that's a very fair comment. I don't think it is a cynical comment ... There are a set of questions in the original Royal Society report that are being asked, some of which are more relevant to policy than others. The system stuff is also there, it's just, again, who wants to sponsor it? In fact a number of UK researchers did go on to study high technology risk systems and high reliability organisations further... I am thinking of my colleagues Rhona Flin and Kathryn Mearns at Aberdeen ... who would be a good example conducting really good work in the offshore oil industry in the 90s. Latterly, the funding opportunities in the systems field shifted, with growing interest in 'medical error' in healthcare following the UK Department of Health 'Organisation With A Memory' report in 2000. And I have never personally felt drawn to working in hospitals - too many governance procedures to go through before you get near the study. So a number of people moved in that direction, I think, and that's the way the field gets blown about, almost, over time.

One new thing that has emerged which is different from when, I think, the 1992 Royal Society report was done, because it always talks about very conventional risks, is the area of emerging technologies. So, that's things like nanotechnology^{vi}, and currently in the climate arena, it would be geo-engineering^{vii}. In biotechnology also, there is synthetic biology as an extension of GM. And that raises all sorts of different and difficult questions. And I think, for me, that's so interesting - how do you study a risk perception of something that doesn't yet exist, that is merely imaginary ...? Do you then construct the risk object by doing the research, if you see what I mean?^{viii}

How do you understand the term 'risk'?

Nick: My background is in mathematics originally when I was at school. And [at] university I then did a joint degree in mathematics and psychology at Keele. And I guess, as a result, I ended up in a space that's really called science and technology studies. This field does see risk as a constructed phenomenon - but, as David Blockley taught me at Bristol, you can appreciate that from an engineering point of view too, as well as through the more traditional sociology treatments, strangely enough. People who work on uncertainty in engineering, if they are smart enough and read some of the philosophy of science, will happily say 'Engineering models aren't real - there is a whole raft of judgements that go into them - and acceptable risk isn't something that you can calculate from a purely scientific perspective'. It has a science and engineering component. yes, but also a judgement and value component ... But I also personally resist an entirely relativist view of risk as well. So I mean, whether I irritate people or not, I'm often, I mean it's the 'death and furniture' argument, which in risk studies is probably not so well known. There was a vigorous debate in British social psychology in the mid-90s, when discourse analysis had just started to become very influential ... And for the positivists within social psychology, the final line of defence was - this is a piece of furniture and that these things exist, or that when people die you can't [sensibly deny that]. And then colleagues at Loughborough wrote a slightly ironic article titled 'death and furniture'^{ix}, arguing [that] even such things could be seen as socially constructed. Now, there is a sense in

which that's true - objects or phenomena are always seen through a social lens. But I think there is a sense in which risk has real consequences in the world too^x, so you have to respect the possibility of death or harm as well. And so I am often found arguing that there is a two-faced nature to risk ... The social amplification model is a bit like this that ... where it [risk] is seen through a whole set of socially constructed phenomena which are all interesting and complex and contextually shaped. This is why psychology alone doesn't answer all of the most interesting questions which one might pose. You have to have sociology and geography and other disciplines involved, but you have also to come back to that fact that you ... are dealing with phenomena that potentially can kill people.

Bob: Yes. So there is something there even if people's behaviour is determined by how they view it and how they feel about it.

Nick: Yes well, and then again, ... Paul Slovic is fond of saying ... that perceptions can be very real in their consequences. And, again, that's quite a straightforward and well known psychological and sociological idea from many, many years back, which I think is true isn't it? If you see people buying salt in China as a result of Fukushima, it may well be a perception and entirely wrong to suggest there was radiation exposure in Beijing^{xi}. But if people believed it was going to protect them, they would go and buy the salt. Whether they were using it or not is a different matter.

The second point is the limitation of classical probability as a model for every 'risk'. I always think of Andy Stirling's work on defining ambiguity and ignorance here. But ... even in the 1992 Royal Society report, I made the distinction between more measurable forms of risk that are more closely tied to either well-understood risk-generating systems or observed within long-run empirical evidence, and risks where the evidence is more uncertain or sparse. So take the risk of being run over in Cardiff. While every street is different of course - we won't get into an argument about that - the risk of dying in the coming year as an average pedestrian user, is obtainable through empirical frequency probability. So, that's kind of one end of it. And then you move through to uncertainty, which in effect involves a greater degree of subjective judgement about phenomena that might, say, only happen once, but you might still have information that bears on that problem that helps you to make decisions about that problem; so that's a Bayesian model of risk. Finally, you have what's thought of now as more ill-structured uncertainty ... the domain of ignorance, where either probabilities or consequences or both, i.e. the structure of the model, are fundamentally undetermined ... It's not risk, but it's a deep form of uncertainty and ignorance so there is a kind of a spectrum there. Andy Stirling talks about a two-by-two model, and you have got uncertainty either more, or less, well-defined and consequences either more, or less, well-defined. And, of course, risk is in only one quadrant where you have well-defined systems, and ignorance is its antithesis, with ambiguity and uncertainty as intermediate states. So I like that set of ideas, although intellectually you can trace this back to David Collingridge in his book *Social Control of Technology* from 1982, where he talks about the dilemma of control in decision-making under ignorance. And I have always seen those distinctions as critical, which is why I covered them in the original Royal Society chapter ...

Bob: Would you disagree with the Bayesian view? ... If it's uncertainty that you can quantify, like with the road case, you would call that risk. But it's not actually different to uncertainty. It's just that you have got inductive evidence you can use it to quantify it?

Nick: There is a very good book called *Comparative Statistical Inference* by Vic Barnett^{xii} which I read years ago when at university which tried to lay this out in terms of mathematical theory, and there he made some fairly strong distinctions. So you have classical probability, relative frequency probability, Bayesian probability and uncertainty. And I think there is a leap between relative frequency probability where you really do – you have been able to observe some things under relatively repeated situations, and you know the new situation is like the old situation, even though you don't know what is actually creating the chance phenomenon *per se*. You know it's just a load of random crazy driving errors around the city of Cardiff generating the accidents, but they all add up to the same thing year on year. But uncertainty, Bayesian uncertainty, is different again because it's fundamentally a psychological phenomenon, it's about a degree of belief. There are philosophical debates about how different they are. There is a sense in which relative frequency probability is also a degree of belief, so Bayesians would say they are the same fundamental concept. Equally, the classical people would say there is nothing more than relative frequency ... Those ideas of course bridged my original intellectual interests in both mathematics and psychology.

Do you know Charles Perrow's work on normal accidents^{xiii}? He's an organisational sociologist – an American sociologist - and his view of risk is [that] it's inherent in the system. My colleague Barry Turner had also made the same fundamental observation in 1978 in his earlier book *Man-Made Disasters*. Both pointed to deep forms of ignorance that you couldn't easily predict. Perrow claimed that you couldn't predict what would eventually go wrong at the Three Mile Island nuclear power station *per se* because of the complexity and the coupled nature of the interacting system elements. So he argues that some systems are, by their very properties, prone to unanticipated accidents that you would not even be able to envisage in a Bayesian sense. And his argument is also, of course, an argument against just taking a purely statistical, fatalities-based approach to risk. It's much more about trying to understand the properties of the risk generating systems and organisations. And I think one of the lessons from Fukushima is that we are going to have to think through more of those types of blind spots in our models of high-risk systems. So it's ignorance again, and that's got a very real set of implications for policy. The German policy response [to Fukushima] has been to take a very precautionary view on its significance. But the British and American response has been, 'No we will try and learn from this, and will carry on [with nuclear]'.

What do you think of the Royal Society definition of risk? How do you view the impact and significance of the Royal Society (1992) Risk monograph?

Nick: If you look at my chapter in the Royal Society report, there is a little aside about common mode failures^{xiv}, something that had been known for a long time in the risk world. Under such circumstances you have to be quite careful that the standard combination laws are not making inappropriate assumptions about independence of events ...

Bob: Sometimes as well, I mean, the assumption of independence is ... linked to the quantification of risk, as you said.

Nick: Yes and I think that [quantification] is what the Royal Society wanted at the time. And, as a result, they could have done without the social science chapters really, which was the history, because they weren't very happy with some of the

things we wrote. There had been a 'risk group' meeting regularly at the London School of Economics, who were asked to review and write the social sciences material, which is what we did. I took on chapter five on risk perception and communication, and Chris Hood took on chapter six on risk management. Having been involved subsequently in other Royal Society reports, and now knowing the process they normally use, this report was not written like that. Normally they would get a committee of experts together, perhaps over a year. They would meet to discuss the broad principles and text, and then certain members would go away with the secretariat and write various bits. Ultimately, however, a report is an agreed statement of the Society, sanctioned by the Council as an authoritative statement from 'science' so to speak. In this case we did not arrive at an agreed report of the Society because when they got our two social science chapters ... there was objection to the way I had developed the objective/subjective risk debate. And, in the first draft, I had developed the argument of Fischhoff's book '*Acceptable Risk*'^{xv}, which is that risk assessment involves judgement all the way along the line, and therefore there is no real thing, there is no such thing as a real risk, it is -

Bob: Depending on the judgements and -

Nick: Yes science and values, science plus engineering judgement plus values. And I was fairly confident that this set of arguments would stand up. But that does lead to the unfortunate conclusion (from the Society's standpoint at the time) that risk itself is a subjective phenomenon. And they did not take to that, and objections were raised when it went out to peer review. In turn, our group were confident that the arguments were sound and in line with the very best thinking in the risk community of the time. The Society argued that this claim either had to be changed, or they would not endorse it, because risk to them was a real thing. And so they were thinking from the conventional engineering science perspective, and there was also objection by referees to a fairly innocuous table I had inserted based upon the work of Charles Vlek which made the simple point that there are multiple definitions of risk in the literature and in risk practice ... And, of course, the Society wanted a single definition. And a report like this was not going to be useful if it didn't have a single definition of risk! So if you look right at the start there they define risk as -

Bob: That's the one I have got there, yes^{xvi}.

Nick: Yes. So then, that went to Council, and there are several levels of report. Level I report or whatever ... the highest categorisation of report is a statement agreed by Council that's said to be true as the Council sees it at that point in time, and not the opinion of the individual who might have written it. If they can't agree on the report, that it's a true statement of the Society, then it becomes a Grade II report which is what this [the Risk report] eventually became. So then the individual chapters are described at the start as the individual views of the individual authors. It had the odd effect of, elevating one's name onto the chapter - whereas in a normal Society report, irrespective of your level of input, you would merely be listed in the Appendix as a committee member. So that whole argument had some interesting career implications ...

Bob: Yes not bad I suppose?

Nick: Not bad. Well, it was different ... At the time I could actually attribute the work to myself, and use it as a REF^{xvii} publication whereas I wouldn't otherwise have been able to do that [in a consensual Royal Society report] ... In preparing for my chapter, as quite a junior lecturer at Birkbeck College at that time, I had written to many of the well-known people in the field, people who subsequently became very good colleagues such as Paul Slovic and Roger Kasperson, Ortwin Renn, Tim O'Riordan

and of course Brian Wynne at Lancaster. I explained that a revision of the earlier Royal Society report was underway^{xviii}, and that they now wanted an expanded chapter on risk perception and communication. So what were they working on currently? What were the key debates? To my surprise I received letters back from almost everyone I wrote to, which then helped enormously to structure the intellectual content of Chapter 5. I still have these letters somewhere in a box, and so that was a part our defence. I said, 'Look, I'm sorry. I have spoken to every international [scholar] in the field I know of, and this is what they came up with', and this chapter is just reflecting some of the things they have said ...

Anyway, so the history then became told as a very acrimonious process. And it had actually, I mean probably was, more acrimonious than the actual authors realised at the time, because I think it probably went on in the Society - should we publish it in this form or not? In the end they decided to do it. Because, as it turned out, and as I am sure you will know, the report became one of their best-selling and most influential reports for many years. And, then, after three or four years, it was clear that the science policy people at the Royal Society were changing, and they realised they had actually got closer to the state of the art than they had imagined ... So it should, I suppose, have appeared as a full Society report with the benefit of hindsight.

Bob: But it actually contributed to what I suppose was a change in climate anyway?

Nick: Yes, absolutely, yes. And people then started to think more about the social sciences aspects. It led to a whole raft of other initiatives like the Risk and Human Behaviour Research Programme that the ESRC^{xix} subsequently commissioned. And again, the report helped the ESRC to make the case to have a programme on risk ... The other thing that it stimulated, that I was also involved with, was funding from the Health and Safety Executive for social science research. So there was work on risk evaluation within environmental economics and on workplace risk communication. But they were also brave enough to sponsor work on social amplification of risk. HSE had in effect a mini-programme, enough anyway to sponsor three projects, one of which was the seminar project that produced the book *Social Amplification of Risk*^{xx}. So it led to some government funding which would not otherwise have been there. I am pretty certain that was because the HSE risk policy people read this stuff and thought, 'The social side is very important for us and our work on societal risk - we have got to engage with it'. I don't think they would do it now. They wouldn't have the resources now to do what, in effect, was a more blue skies form of research.

How do you see the history of risk social science?

Nick: One recent development has been many sociologists becoming interested [in large-scale risk systems analysis], but most of them, I would say, are not risk researchers particularly. Let's consider the Beck argument, the whole argument about Beck. Conceptually his work is very illuminating, but nevertheless a number of us were always quite sceptical as to how well it articulated with mainstream risk research. And I think that that still seems to be the case; that it's not influential [in applied risk systems research], but it has been within sociology more widely. I mean, yes, if you take Beck's book and line it up against Turner's work, or Perrow's book^{xxi}, or Dianne Vaughan's classic account of the Challenger disaster^{xxii} ... Perrow and Vaughan are now very well-known within the risk management field, particularly in the US. Barry Turner was equally well-known in Europe, and a number of key

developments flowed naturally from all of their work, all of the safety culture and high reliability organisation work etc. ... Yet, given they were all mainstream sociologists, they are often completely ignored within mainstream 'risk sociology'. Now isn't that strange? And yet every sociologist has read Ulrich Beck's book. Why is that?

Bob: Yes, that's what I was going to ask you!

Nick: When we come to Fukushima, it's absolutely clear that Perrow's book tells us far, far more about that event and its risk significance^{xxiii} than Risk Society [Beck] will tell us. Risk Society may tell us about some of the wider responses to the event, and perhaps I frame it too narrowly around the risk management questions - how you keep the system safe, what's dangerous or not, risky or not ... So sociology has made very good inputs into the risk field, but it's also carved out a niche that it calls risk but which still struggles to articulate with the field ... For example, Beck talks about anxiety and individual biographies of risk which in theory ought to be relevant to risks from nuclear power through to the more everyday type such as health risks. There is an excellent book by Tulloch and Lupton^{xxiv} where they try to work through Beck's analysis in relation to the everyday, and find that it doesn't quite articulate with their qualitative data. We have also done work with residents near nuclear facilities in the UK. And if there is one place where 'risk society' ought to be prompting the eternally anxious subject, it's there. And yet, while anxiety clearly can be generated even by the very thought of radiation and nuclear power, we also obtained some very similar findings to Tulloch and Lupton^{xxv}.

Bob: So that, essentially, people are quite pragmatic?

Nick: Quite pragmatic when living with nuclear risks, yes, except that everybody can also tell you about an instance of when they felt rather uncomfortable, and, for some, exceptionally uncomfortable, at the thought of nuclear power. So moments of anxiety do exist.

Bob: And that would be based around an incident?

Nick: It could be when people hear mediated accounts, such as the terrorist bombings in London, when Chernobyl occurred, and so it would have been within the last six months when Fukushima occurred. Then, many people living around the sites would be prompted to ask themselves the [safety] question. And strangely enough, it also happens when you get anything odd happening at a plant. So if you are having an emergency exercise and local people have not been told, then they can become concerned ... We've described it, drawing upon the work of anthropologist Joe Masco, in terms of the 'nuclear uncanny' - that nuclear technology has taken on a set of associations with world-making and world-destroying powers, and the invisible^{xxvi}. It's not just a purely psychological thing as well. It's about the way nuclear has been positioned within discourses about science and society that sets it apart. So it's a strange technology even now, and that then generates anxiety. And I have even had nuclear specialists saying exactly the same thing - that this analysis applies to them. Although they think it's great, it's safe, there is nothing wrong with it, and they have confidence in the risk management and assessments, they have had at least one experience of feeling, 'Oh dear, this is about to go wrong, and now this really does worry me'.

Do you think risk has become a key element of government policy?

Bob: would you say the UK government policy has become more risk-orientated, for better or worse, over the last 20 years or so?

Nick: I think it did for a while, because we had the Cabinet Office Risk report in 2002 which was trying to instantiate risk thinking. And I think, of course, the parallel thing going on at that time was the insertion of chief scientists in all of the UK government departments. So I think automatically, once you have senior scientists sitting at policy level ... this lends to phrasing policy in risk terms. So I think there is more technical expertise in risk. But there is also, ... there is more scientific thinking, I think, and that immediately, then immediately the model you are going to use is a risk-based model. That may or may not be entirely desirable, of course, but I think that's clearly the case. But alongside that ... there is a danger that it just becomes a wider thing about 'risk management of everything', as Mike Power^{xxvii} puts it, as applied to government. And the Cabinet Office report - good report though it is - has become proceduralised to some extent. I think that would be the way to talk about it - institutionalised in a not very reflective way at times.

ENDNOTES

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- ⁱ Pidgeon, Lorenzoni and Poortinga (2008).
ⁱⁱ Bickerstaff *et al.* (2008).
ⁱⁱⁱ Poortinga, *et al.* (2004).
^{iv} Chemical industry accident near Milan in 1976 which caused regional dioxin exposure.
^v The UK Health and Safety Executive (HSE).
^{vi} Pidgeon *et al.* (2009).
^{vii} Corner and Pidgeon (2010).
^{viii} This issue is addressed by Nick Pidgeon and colleagues in Horlick-Jones *et al.* (2007).
^{ix} Edwards *et al.* (1995).
^x The ontology of risk remains ill-defined in the social sciences and more generally. One approach to thinking about the issue raised is to distinguish an **event** such as nuclear accidents which 'really' kills from its **risk** which references prognostic limitations.
^{xi} Many other, sometimes controversial examples can be given, for instance cheese avoidance if sceptics about the dietary fat intake-high 'bad' cholesterol-coronary heart disease linkage (Hann and Peckham, 2010) are to be believed.
^{xii} Barnett (1982)
^{xiii} Perrow (1984)
^{xiv} Failures arising from the same causal factors and therefore not statistically independent – c.f. the banking fiasco of 2007.
^{xv} Fischhoff *et al.* (1981)
^{xvi} 'the probability that a particular adverse event occurs during a stated period of time, or results from a particular challenge.'
^{xvii} The UK Research Excellence Framework (REF), currently in use for evaluating academic research, known at that time as the Research Assessment Exercise (RAE).
^{xviii} The Royal Society (1983).
^{xix} The UK Economic and Social Research Council, the primary state funder of social science research.
^{xx} Pidgeon, Kasperson and Slovic (2003).
^{xxi} Perrow (1984).
^{xxii} Vaughan (1996)
^{xxiii} Pidgeon (2011).
^{xxiv} Tulloch and Lupton (2003).
^{xxv} Parkhill *et al.* (2010).
^{xxvi} A good example of culturally supported 'risk iconography' (Heyman *et al.*, 2010, pp. 55-56).
^{xxvii} Power (2004).

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