

Mr R G Courtney

Thank you for your letter of the 19th of October enclosing a copy of the Expert Panel Report. I have now read the Report and prepared this response to the observations it contains.

But before taking up the Panel's comments I would like to thank you and Dr Menzies for your parts in the organization of this assessment of the work of Nigel Clayton and myself.

I must confess that when the Panel was first mooted I feared it might be a repetition of the first review of our work by a panel who, as I quite fortuitously discovered, had secretly prejudged the issue and who I consequently refused to accept.

When I was told that Kelly was to be Chairman the Expert Panel I expressed my relief since he had refereed my work before. I was more than somewhat perturbed therefore to hear later that Kelly had other commitments and a different Chairman was to be appointed. I feared that history might be repeating itself.

I now realize that these fears were quite unnecessary and that the Panel brought no prejudices to its task other than those we all possess as a result of our lifetime's experiences. I felt totally at ease at my meeting with the Panel who let me express my views with complete freedom and who argued in a perfectly reasonable and acceptable fashion.

Although, as was to be expected, we did not agree on the central issues, I left the meeting feeling that I had been dealing with men of integrity who would prepare a honest report without fear of favour to any party.

I am pleased that the Report pulls no punches. It criticises ruthlessly what is seen as error and praises generously where it feels this is justified. How much more satisfactory than some mealy mouthed anodyne that sits on the fence for fear of getting it wrong and says nothing of any significance at all.

And because, quite rightly, the Expert Panel have been incisive in their judgement of our work I am sure they will not mind if I am equally trenchant in its defence.

I have taken the sections in a slightly different order from that in the Expert Panel Report. More specifically, I comment on the probability section first since the concepts involved are more obvious than is the case for the other sections.

USE OF PROBABILITY METHODS IN ENGINEERING

In the second paragraph on page 9 of the Expert Panel Report the distinguished experts claim that I am,

"wrong in that the numbers of 2's $\rightarrow 1/6$ when $N \rightarrow$ infinity and does not tend to zero".

If I really had claimed that for N spins of a dice the number of 2's that come up tends to zero and does not tend to $1/6$ I would have not merely been wrong. I would have been grossly incompetent.

What I actually wrote was this:-

"... however many trials I make there is no guarantee that the percentage of 2's will be exactly $1/6$."

So that things will be crystal clear and to eliminate any possible misunderstanding, let me elaborate precisely what I mean by this statement.

If I spin the dice six hundred times there is no guarantee that I will get exactly one hundred 2's (one hundred being, of course, one sixth of six hundred as I'm sure the Expert Panel will concede). I might get ninety eight 2's or ninety seven 2's or one hundred and three 2's for example. I might even get one hundred 2's but, as I've said, there is no guarantee.

If I spin the dice six million times there is no guarantee that I will get exactly one million 2's. Of course it is possible, but it isn't very likely. It is considerably less likely than my chance of getting one hundred 2's when I spun the dice six hundred times.

If I spin the dice six billion... but I can't imagine that I need to elaborate any further. Surely, the next sentence of my note will now be perfectly clear. It continues on from the previous sentence given above as follows:-

"On the contrary, if I make $6N$ trials where N is a very large integer, even though the fraction of 2's could be $1/6$, the probability of this is small and tends to zero as N tends to infinity."

Weren't the Expert Panel curious as to why I should want to make $6N$ trials where N is an integer rather than simply N trials? Isn't the reason perfectly plain, namely, unless the number of trials is divisible by 6 then the number of 2's can never be $1/6$ th?

Besides being accused of being wrong, I was also accused of being repetitive. It seems to me I was not repetitive enough. Perhaps I should have assumed that people's short term memory wasn't sufficient for them to carry over the word exactly from one sentence to the next and I should have repeated it. If I had been writing for my mother (aged 95) I would have done.

As for the accusation of being trivial I fear that, on the contrary, I might have been too profound.

I must say, I do applaud the Expert Panel's commitment to intellectual freedom of expression in proposing that someone who believes that in a long run of dice throws the number of times that 2 comes up tends to zero, should be allowed 15 weeks to write up his ideas on possible failure of a nuclear reactor. I fear I would be far less liberal. I would ask him along to my office and say very kindly,

"Look here Frank, the management have been having a little talk. We feel that you've been in research non-stop for 36 years and really deserve, a jolly good rest so that you can pursue your hobbies and spend some time with your 14 grandchildren. We don't have any voluntary premature retirement vacancies at present but we do have discretion and we feel your case is rather special. How about it? Interested?"

And if I had been a member of the Expert Panel and asked to question someone who believed that in a long run of dice throws the number of times that 2 comes up tends to zero I would have been quite fascinated. What could he possibly think the other five numbers on the dice would tend to. After all, someone who believes that materials are held together from the outside and not from the inside might believe anything, might he not! Perhaps he would have an obsession with lucky prime 3 and think that in a long series of throws three would dominate number 2 and drive him out office. Perhaps he is a religious nut who believes that a long series of throws would summon up the devil and result in a interminable sequence of 666's, the sign of the beast.

I'm very grateful to the Panel Secretary for not drawing the Panel's attention to this this total misunderstanding (which I'm sure he must have spotted) since it serves to illustrate a important psychological principle.

The error is unlikely to have arisen from simple carelessness. The Expert Panel's Report is only 13 pages long and will obviously have been read very thoroughly by all three distinguished experts who have all signed it at the bottom.

The Fellow of the Royal Society has a commendable admiration for accuracy to nine places of decimals (see paragraph 2 on page 5 of the Expert Panel Report) so the probability of him missing an inaccuracy must be very low.

The Senior Official from the Nuclear Industry Inspectorate which is charged with the gravest imaginable responsibility for protecting the public from a nuclear catastrophe will no doubt have meticulously checked and double checked every syllable. So the probability of him missing an inaccuracy must be extremely low.

The University Professor.....? Well it's true that University Professors do have a reputation for being absent minded but I seem to remember a saying from when I was at college which ran,

"Every student wants to prove the professor wrong"

and since in my experience they very rarely managed it. I will assume that the probability of him missing an inaccuracy is fairly low.

What about interactions? It can safely be assumed that the three distinguished experts are all sufficiently independent not to succumb to the herd instinct so I can multiply the probabilities together and claim that the the probability of the group of three missing an inaccuracy is,

fairly, very, extremely low.

Since this is no higher, say, than the probability of three responsible professional nuclear engineers carrying out an experiment on a reactor which leads to its catastrophic failure, it is a probability that can be neglected.

But since the inaccuracy cannot be ascribed to carelessness to what can it be ascribed?

It can be ascribed to the psychological tendency to see what we expect to see rather than see what is actually there. A good example of this is given in Fig.1. It is a quite genuine cover, proof read at various stages in its production by the Print Unit of Sheffield University and yet the error was missed by all who saw it until it was too late and it was widely distributed. It is a fine example of conceptually driven processing at work, dictating what is 'seen', in this case at the expense of what is really there. People often see what they expect to see, the stronger the expectation the more marked the effect. When judging work professing quite unacceptable beliefs then the expectation of finding gross errors must be very high indeed.

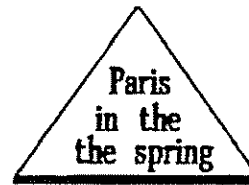
Now the corollary of seeing what is not there is not seeing what is there. As an example of this inverse perceptual error consider the following popular psychological demonstration.



Figure 1

If the piece of text within the triangle in Figure 2 is cut out, pasted on a piece of card and shown to a series of experimental subjects, the more literate the better, then the following effect is generally observed.

The subject is first shown the card for a very short interval, a fraction of a second, say. After the card has been hidden the subject is asked to tell you what was written on the card. A large percentage will reply,



"Paris in the spring."

Figure 2

The subject is then shown the card several more times for gradually increasing periods and asked each time to say what is written on the card. When he starts to get impatient he is asked if he is quite sure the card says "Paris in the spring". Many subjects will say that they are quite sure. With the most suitable subjects it is even possible to eventually let them take the card into their hand and keep on telling them that it doesn't say "Paris in the spring" to a point where they get very cross. They may even become violent. When it is eventually pointed out that the card reads,

"Paris in the the spring."

the subjects with a good sense of humour are quite amused by their own perceptual frailty, whilst the ones without become extremely disagreeable and walk away muttering something about "having been tricked".

As if to demonstrate that the first perceptual error of commission was not an isolated weakness The Expert panel has provided an inverse example of omission in the last paragraph on page 8 of the Expert Panel's Report which reads as follows.

"Mr Grimer reveals his wild misconception of probability as an engineering tool.... in Document (3) p.13 'after Chernobyl the probability of failure of remaining nuclear plants was increased from zero to $1/N$ where N is the total number of nuclear plants extant.' "

The correct quotation from Document (3) p.13, reads as follows.

"Since any probability is ultimately the expression of known facts, after Chernobyl the probability of failure of remaining nuclear plants was increased.

At the most primitive level of risk analysis, Chernobyl raised the fiducial probability of catastrophic failure from zero to $1/N$ where N is the total number of nuclear plants extant".

The panel has created its own "wild misconception" by ignoring the beginning of both sentences and cobbling together the ends.

The fiducial probability doesn't refer "failure", it refers to "catastrophic failure". What the inhabitants of San Francisco might describe as "the big one". Failure comes in many shapes and sizes as I discuss at length in one of my notes. But to the best of my knowledge there has only been one real monster nuclear reactor failure which truly deserves the term catastrophic, a failure which has spewed its contents over hundreds of miles and an unknown number of future generations.

And that failure is Chernobyl.

The word catastrophic is essential as it separates Chernobyl from all lower levels. In a Richter scale of failures Chernobyl is the highest index and hopefully will remain so. Perhaps it was some sub-conscious censor that interrupted the flow of information from the retina to the Expert Panels brain and deleted the word CATASTROPHIC as being just too painful to contemplate.

Also, I was very careful to commence the sentence in Document (3) p.13 with the essential qualifying phrase,

"At the most primitive level of risk analysis."

and I will now explain precisely what this means, since the Panel have evidently failed to grasp the philosophy of probability detailed in this document.

As the adjectives "most primitive" suggest, the amount of knowledge one possesses is a minimum and is summed up in the following two statements:-

At some period of Alpha, there are N nuclear reactors none of which have failed catastrophically.

At some later period Omega, there are N nuclear reactors, one of which have failed catastrophically and N-1 which haven't.

Years, days and seconds are totally irrelevant at this most primitive level. The concept of time has been stripped down to its irreducible minimum, an Alpha and an Omega, a beginning and an ending. Any knowledge one might have as to the internal workings of the nuclear reactor are also irrelevant as is new knowledge that was obtained, not immediately after but some significant time after. The only knowledge involved is that the nuclear reactor is in one of two states, the state of not being catastrophically failed or the state of being catastrophically failed.

For this primitive analysis the probability of catastrophic failure is indeed $1/N$, i.e. the ratio of the number of reactors which have failed catastrophically to the total number of reactors. A probability has to be some chosen state space ratio. It can't possibly be anything else. The state space ratio may relate to real or imaginary data, true or false data, implicit or explicit data but it has to be: it has to exist.

Of course, everything hinges on the choice of the state space, doesn't it. One day Dr Johnson was walking down a narrow London street where the upper stories of the houses overhung the lower stories which allowed people to converse with each other across the gap. Two women were arguing hammer and tongs and Johnson observed to his companion,

"Those two women will never agree. They are arguing from different premises."

And that is the crux of the matter. Everything depends on the choice of assumptions, the choice of the state space.

And whose judgement governs the choice of these assumptions? The person or group with the biggest stick, with the power to impose assumptions. The deciders, not the advisers. The King, not the courtiers. If the only concern is with objective truth and totally uninfluenced consciously or unconsciously by self-interest, financial gain, professional advancement or human respect then the assumptions are likely to be true and providing the logic is sound the deductions will be good. Alas, how rarely are these conditions fulfilled. Indeed, increasing numbers of people even seem to have lost all belief in the existence of objective truth.

With reference to the first paragraph on page 9 of the Expert Panel report I am well aware that many published studies show that prior to the catastrophic failure at Chernobyl the probability of failure was not zero. Also, I don't doubt that a re-assessment is likely to reduce the perceived risk. After all, public confidence in nuclear power was severely shaken by the Chernobyl catastrophe. People whose hard won expertise and very livelihoods would be put in jeopardy by the abandonment of nuclear power would have very strong incentives both conscious and subconscious to frame new assumptions and come up with better numbers. As for the statement that "Chernobyl has made nuclear energy safer" the reasonable man on the Clapham Omnibus could perhaps be forgiven for echoing words uttered by a casualty of a earlier debacle.

"They would say that wouldn't they."

Please don't misunderstand me. I well realise that Chernobyl must have had an beneficial feedback effect on safety in the nuclear industry. Many known potential causes of failure have doubtless been eliminated and so a sound objective assessment based upon the remaining known potential causes of failure will show a reduced risk of failure. The trouble is such assessments are in danger of leading to complacency since they can only take into account the known risks, they cannot by definition take into account the unknown risks. New knowledge is constantly appearing and the price of safety is a constant re-assessment of that new knowledge to determine its implications in relation to system behaviour.

When large computer networks were first set up did anyone take into account the effect of viruses on the integrity of the systems? They considered it no more than the possibility that pigs might fly. Viruses didn't exist. Our 20th century saw the creation of the very first.

When the Hartlepool reactor was designed did anyone take into account the possibility that one day the theoretical view of material strength might, quite literally be turned inside out and reveal possible failure mechanisms they hadn't considered. I doubt it.

At the top of page 10 of the Expert Panel Report the Panel have completely misunderstood what I mean by a high level event. I don't mean events which have identifiable precursors but events which haven't. Spelling mistakes may arise because there is a hole in the paper or because the ink delineating some of the letters has faded so badly that the letters cannot be distinguished from their ground. Such spelling mistakes can be forecast from their low level precursors and rules can be devised to calculate when combinations of such errors become sufficiently frequent to lead to failure of words to convey their correct meaning. But obviously I don't mean that kind of failure by a high level event do I? I mean the spelling mistakes which do not involve holes in the paper or disappearance of the ink. Failure where there is nothing wrong with the elements in themselves and nothing that can be discovered from examination of those elements. It is that tenuous substantial reality, the relation between those elements which leads to failure of the words, not the failure of the paper or the ink or the source of illumination. I made this quite clear in the third paragraph on p 23 of Document 13.

What the Expert Panel describes as "my violent rejection of probabilistic methods" was intended to make people stop and think about the implications of developing mathematical methods of structural design which are becoming lost in a mathematical fairyland, a devil's advocacy to counterbalance gross theoretical hagiology. To be perfectly fair, the Panel does recognise this somewhat belatedly in the last paragraph on page 10 but it might have been more helpful to the reader if this final paragraph had been inserted at the beginning of Section 4 rather than appended at the end. Perhaps it was added as an afterthought. Perhaps Section 4 was written before our meeting on 4 May.

On page 10 the panel writes:-

"The climax of Mr Grimer's dissertation in Document (II) is found on page 24 where he states 'By designing deterministically the engineer achieves an appropriately god-like omnipotence and omniscience in relation to his creations'"

and adds no comment, presumably because it feels that this ultimate cry of defiance against the god of chaos speaks for itself. I can almost hear the hissed accusation. "This man blasphemeth. What further need have we of evidence?"

But consider that most modern of engineers, the software engineer. Aren't his creations completely deterministic? Don't they always give the same answer? If he designs a routine to calculate 'e' to 4 places of decimals doesn't he always get the same answer? Is there any possibility, however small, that one day he will use his routine and get 'pi'. Of course I am talking about the routine in itself, not the implementation of the routine by a machine. One of the most humbling things about programming is that if the programme crashes you can only blame yourself, not your unlucky stars.

Consider also the most deterministic of modern man's creations, the computer. If when I had left college in 1953 I had seen the future and forecast the degree of determinism in the machine which would replace my hand cranked calculating

machine I would have been laughed to scorn. Wasn't it the Astronomer Royal who said that space travel was bunk. Wasn't it proved mathematically that bumble bees couldn't possibly fly. If we hadn't reached for the moon we would never have got there and if we don't aim for zero defect vehicle production in Britain we will all finish up driving Japanese cars.

The point I was making when I "blasphemed" was that the engineer is completely in control of his creation in so far as it is his creation but obviously he is not in control of that part of the production which is not his but His. The engineer controls the structure of the beams and columns in his bridge, his creation, though he may not properly understand the underlying structure of the material from which those beams are made since this design is not his but His and generally far more mysterious and intricate than his. The same consideration applies to the environmental forces that the bridge sustains.

I could discuss in detail all the other comments the Expert Panel has made on probability, the incident of the humble candle at Brown's Ferry for example, so nearly a fulfilment of that ancient prophecy.

"Deposuit potentes de sede, et exaltavit humiles."

but there would be little point.

HIERARCHICAL MECHANICS OF FAILURE

The Expert Panel opens this section with an assertion which is incorrect. It claims.

"Mr Grimer's theory of the hierarchical mechanics of failure has its roots in his experiments on the fracture of concrete under various combinations of compressive stress."

Not so! The theory of Iterative Hierarchical Mechanics did not have its roots in the experiments on the fracture of concrete under various combinations of compressive stress.

IHM theory had its roots, appropriately enough, in experiments on the strength of clays and soil-cements which revealed a network of Logarithmic relationships between strength and density. These experiments are described in the first of the research papers, Document 3. This network of roots was stimulated to growth by the discovery of the equation of state for water (see the Appendix to Document 3).

Now it is true that in Document 3 I never actually used the word HIERARCHICAL but the hierarchical implications are perfectly clear. I wanted to use the word hierarchical but (you may find this hard to believe) my division head, who censored even internal notes, seemed to have some irrational objection to the word, and wouldn't allow it. I had to use the phrase "levels of scrutiny" instead.

If roots are to be found, a bit of digging is normally required.

IHM theory was brought from underground into the sunlight by the experiments to determine the full stress strain curve of a range of concretes.

This is the point at which it was realised that materials are held together by external compressions and not internal tensions; this is the point at which the darkness surrounding the roots was turned into the blinding light of the sun: this is the point at which.

"lux in tenebris lucet, et tenebrae eum non comprehenderunt."

The experiments which extended the technique originated by Bridgman were carried out after this realization and in the light of this realization. But for this realization it would never occurred to us to carry them out. Documentary evidence of this claim is provided by the memo attached as Appendix I.

I thank the Panel very sincerely, on behalf of my colleague, Mr Nigel Clayton to whom total credit is due, for their magnanimous recognition that the experiments "...are well designed, have produced interesting and challenging results, and have also produced a method of clear practical value for determining the strength of concrete under a variety of important circumstances." Why, might I ask, was this "method of clear practical value" not produced by the conventional viewpoint? After all, the method is simplicity itself to carry out and people have been testing brittle materials for an awful long time. Couldn't it possibly be because they were looking at the problem in the wrong way and we have found out how to look at it in the right way?

I also thank the Panel, again on behalf of Clayton, for admitting, in relation to the experimental demonstration of failure under conditions of all round compressive strain, that "There can be no doubt about the validity of this striking experiment." The reason it is so striking, of course, is because, given the conventional viewpoint, it is utterly counter intuitive and only explicable at all, by mental gymnasts as hugely skilful as the members of the Panel.

And while I'm making my thanksgiving I must also express my gratitude for the compliment that I am a "...genuine person dedicated to unconventional lines of thought in which he sincerely believes...". Like the curate's egg, the Panel's report is very good in parts.

Now I would like to take up the statement that, "There is nothing in this effect that is inconsistent with conventional ideas". The same could have been said about, say, the discovery of the phases of Venus. It must be remembered that the Ptolemeic system was not replaced by the Copernican system because of effects it couldn't cope with. The principle advantage of the Copernican system was its greater geometrical simplicity.

Because they are peculiarly apt to the claims of IHM it is appropriate to quote some lines from Koesler's, *The Sleepwalkers*, where he observes that the Copernican system.

".....was an enormous gain in simplicity and elegance. On the other hand the shifting centre of the universe to a place in the vicinity of the sun entailed an almost equal loss in plausibility."

"The Copernican system; was not a truly helio-centric one; it was a vacuo-centric system, so to speak."

"If it was to be considered merely as sky-geometry without reference to physical reality this did not matter too much. But in his text Copernicus repeatedly affirmed that that the earth REALLY moved and thereby exposed his whole system to judgement based on real, physical considerations."

And I affirm that materials are REALLY held together by external pressures exerted by a hierarchy of REAL and MATERIAL aethers and that tensile forces are negations: regions of reduced pressure in these material aethers.

The Panel is quite right in claiming that this idea extends, "to all levels and scales of matter including the molecular and atomic." To show just how megalomaniac the claim of IHM is they could have added "nuclear, sub-nuclear and any lower level of structure that might be discovered in the future". This is the whole essence of any hierarchical theory which claims to present a general view of the relation between the nth and the (n+1)th hierarchy. In the language of solo whist, the cards have been seen and Royal Abundance has been declared.

In para.3, page 5 of the report, the Panel refers to the Casimir Effect which shows that a vacuum can exert a pressure but point out, quite rightly, that it is far too small to exert the kinds of pressures needed to hold atoms together, let alone nuclei. But it does establish a principle doesn't it. Any pressure, however small is infinitely more than no pressure at all. Like the grain of mustard seed, this principle will grow, given time.

Turning the strength of materials inside out will not involve the rejection of existing experimental facts but merely their reinterpretation and result in a much better understanding. To give an analogy, if I have a negative photo of the Queen and turn it visually inside out by printing a positive, I do not lose any bits of information. The crown still sits on her head accurate to nine places of decimals, doesn't it? The advantage of the positive photograph is that whereas the person in negative was unrecognisable and might have been any old queen, the person in the positive is instantly identified as the our Queen.

RELEVANCE TO NUCLEAR SAFETY

I must begin by pointing out that I have never claimed that my doubts about large prestressed pressure vessels in general and Hartlepool in particular were anything other than initial. They arose from the radically different way in which we understand material strength.

But I have had initial doubts about structures before and have seen such doubts justified.

When I got hold of the cube tests from the Lean Concrete laid down during the construction of the M1 in the 1950's and statistically analysed them, I had doubts whether the motorway would last more than a few years.

When Sir William Glanville ordered that every copy of my internal note be overwritten in large lettering "Not for Publication" my doubts were seriously increased since he obviously felt that there was something which should be covered up and I had a great respect for his engineering judgement.

When, almost as soon as the motorway was finished, we had to dig through it to find out why it was beginning to break up and we found that instead of lean mix-concrete it consisted of what would be more accurately described as poorly-controlled-cement-stabilized-as-dug- aggregate I no longer had any doubts, I knew. I knew that the complete replacement of the whole of the road pavement down to subgrade level was only a matter of time. And so it proved.

The M1 was not the only motorway about which I had doubts either.

When we were permitted to drill soil-cement cores from the Ross-Spur Motorway and started finding a layer of neat soil sandwiched in between the two stabilized layers I had doubts that these were isolated imperfections and informed the Assistant Resident Engineer.

He, of course, had no doubts. He couldn't afford doubts since 4 miles of dual carriageway had already been finished and covered up with the hot rolled asphalt wearing surface and the consequences of having to dig it all up again was just too traumatic for him to contemplate.

So we went back and drilled a lot more holes and found nearly all of them had an unmixed layer between one half inch and two inches thick. I told the engineer that I was certain that the fault was general and he began to wonder.

So he ordered a trench to be dug across the full width of the carriageway. And he said that he could see any unmixed layer which was understandable because dry compacted Keuper Marl without any cement in it doesn't look very different from from the same material with cement. To convince him that what we said was so, we did a quasi-archeological excavation to reveal the top of the bottom layer which had been compacted by a smooth roller and was flat as a pancake, and the top of the unmixed layer which had been compacted by the rotating tines of the in-situ stabilization machine and was heavily corrugated. Then he was convinced that there was indeed an unmixed layer but he believed it was an isolated fault.

So he had another carriageway width trench dug 100 yard further down the motorway to demonstrate that the fault was isolated. But it wasn't. He had another trench dug another 100 yards further down. And another, and another, and another..... By lunchtime the trenches stretched along the motorway like defences in depth on the Somme and finally he was convinced.

And he ordered all the work along both length of carriageways to be stopped. It remained stopped for two whole weeks while everyone blamed everyone else for the shambles.

And eventually a compromise was reached, the specification for the rest of the motorway was changed from in-situ stabilization to lean- concrete mixed off-site, the finished section was botched up with another two inches of hot rolled asphalt and everyone went home and hoped for the best.

Since the Ross-Spur is known as the motorway that goes from nowhere to nowhere and carries very little traffic their hope was probably fulfilled.

Many years later working at Building Research I was a member of the joint BRS/Pilkinton Bros. Steering Committee controlling the development of Glass Reinforced Cladding Panels.

When I saw the manufacturers manipulating the interpretation of our research data I began to doubt that the development would be successful.

When the Pilkington committee chairman suddenly changed the design philosophy from ductile to brittle because the long term tests showed a disastrous drop in ultimate tensile strain (1 percent to 0.03 percent in 5 years) my doubts grew more serious.

When on American Independence Day, 1974, the Chairman flew into a rage because I refused to go along with his wishful thinking about the shape of the deterioration curve and screamed.

"I'll hound you Grimer! I'll hound you! I'll hound you!

before bursting into tears, I knew the project was heading for the rocks.

I resolved to start inspecting cladding panels after 5 years when they had lost 97 percent of their strain capacity. Sure enough, at 5 years the panel on the 170 buildings surveyed had started to crack up and had to be extensively repaired or replaced. The largest manufacturer went into liquidation and was taken over by Pilkingtons. The three next largest went bust or dropped manufacture.

So you can see why I feel a duty to follow up my doubts even though they may not be shared, even though they may be violently opposed.

If the Hartlepool reactor were to fail catastrophically and make Cleveland uninhabitable (God forbid) it wouldn't be much use my saying.

"Yes, I did always think that there were indications this might happen but I like a quiet life and I thought people would seize upon my unconventional scientific philosophy as an excuse to twist everything I said and without those theories at least being recognised as fascinating and possible correct I didn't think I stood a celluloid cat in hell's chance of anyone taking the slightest notice. So I kept quiet!"

Surely, people would upbraid me and say.

"My goodness, what moral cowardice! You were a Senior Researcher in the Structural Integrity specifically charged with anticipating potential failures in structures. This was such an important structure and the consequences of catastrophic failure so horrendous that you had a grave duty to investigate any doubt even if it meant being very unpopular indeed. Even if you hadn't been believed you might have alerted people so that if anything did start to go wrong they would be jolly careful how they handled things."

And they would be right, wouldn't they.

The Panel is quite correct in deducing that I am not in touch with the design and development of prestressed concrete pressure vessels but I don't need to be since I am concerned with general principles not specific details. Even a nurse can recognise when a surgeon is cutting off the wrong leg.

I have heard the story about the calibration error at Hartlepool and frankly, it sounds to me as though someone is being economical with the truth. Before I believed it, I would need to see the relevant records and talk freely to the people involved. I can't imagine that I would be ever be allowed to. And when information is withheld the only safe course of action is to believe the worst.

And even if the story should be wholly true it raises serious questions of how such an error could ever arise. And where one serious error has been found there is an increased likelihood of others.

I am not so naive as to think that the BBC story contained the whole truth. Nor am I so naive as to think that CEGB and the HSE are incapable of cock-ups and cover-ups. Good heavens, I've known enough of them during my four decades in government service.

I have not communicated my views to anyone in the nuclear industry because the time was not yet ripe and I was stopped dead in my tracks by a Director's edict and forbidden to develop those views. Since developing original lines of thought is damned hard work I was, frankly, quite pleased to have an excuse to forget about them for a while.

MR GRIMER'S SCIENTIFIC OUTLOOK

The panel finds my lines of thought flawed. In the light of their misunderstandings on probability this is hardly surprising. If flawed lines of thought can give rise to valid and striking experiments together with methods of clear practical value for determining the tensile strength under a variety of important circumstances then, on purely pragmatic grounds, these lines of thought must be worth pursuing, must they not?

They also find that I am adept at finding loopholes for escape in debate. This is to be expected. Clayton and I are our own worst critics and probably appreciate the arguments against IHM better than anyone.

With regard to the experiments on concrete it must be reiterated that what the Panel describes as a more radical explanation, and I describe as the true explanation, came before the experiments. It was not the other way around. Without the radical/true explanation to guide us, there would have been no experiments. This doesn't prove my explanation was true but it does show that it was at least useful.

The Panel is absolutely correct to say that I have kept myself in an 'ivory tower' and not sought to expose my ideas very much outside the circle of my immediate colleagues. Radically new ideas cannot develop in the hurly burly of the market place. A desert, monastery or ivory tower is the necessary environment. Also, being a terrible coward, I tried not to stick my head above the parapet more than I had to. The day before I presented a paper to an international conference saying that I believed materials were held together by external pressures, not by internal tensions which were simply negations of those pressures, was one of the most nerve-wracked of my career. But what was I to do? I knew that if I had to stake my life on it being true or not being true I would stake my life on it being true. If this made me unfit to carry out research then my employer had a right to know just as he would have a right to know if I had caught some infectious disease.

The Panel has no justification for saying that I interpret lack of comment as agreement with my theories. That would be stupid. I do however interpret lack of comment as indicating that I am not necessarily wrong in my conviction that my theories are essentially correct. Consider for instance the very last Note which led to the showdown with my director. This details some simple arguments relating to the nature of mass and the reason for the phenomena known as time dilation. Why didn't he simply tear the arguments to pieces. I'm not an unreasonable man. I only want the truth. If my arguments are false anyone who demonstrates this will be doing me a great service. He did try, via the intermediary of my division head, to rubbish our experiments on concrete but only succeeded in making a fool of himself and demonstrate that he wasn't even capable of reading what we had written.

As for the note on Mass, it is my belief that he didn't know where to begin, and he couldn't find anyone else who did, either.

RECOMMENDATIONS

I must confess that had I not read your letter before reading the Expert Panel Report I would have been surprised to find any recommendation for further work in view of their implicit rejection that a hierarchical philosophy might be true or even useful.

Since my eventual retirement is only 2 years away I am prepared to put off writing notes on the nature of mass and the like until I am a free agent, and stick strictly to the behaviour of concrete. However I obviously have to interpret this behaviour in line with our general theoretical approach if any progress is to be made and we cannot possibly recant on the philosophy set out in the three previous publications (Documents (6),(9) and (10)). To do so would be to loose one's integrity and with it, all credibility.

I regret that I must also ask you to modify your offer in two respects. To develop the argument about which reinforcement should be tightened in a large prestressed structure to a point where it is convincing, not simply to an indifferent bystander, but also to a person who has a strong psychological incentive to refute them (cf the example of the Resident Engineer on the Ross-Spur Motorway) will require a considerable mental effort and I feel incapable of making that effort unless management are prepared to show sufficient confidence in me to remove that deadline and leave the project open ended, right up to the end of my service if necessary.

To be perfectly honest I suffer from a medical condition which means that my night's sleep is very disturbed and I am conscious that since the death of my wife some three years ago my efficiency has been seriously reduced. Being a Section Head without a section, a head without a body, is doubtless better than being a body without a head, but to do the job required I will need at least one hand. I must therefore request all necessary assistance from my valued colleague and indispensable collaborator, Mr Nigel Clayton who has always served as a correcting force, and permission to consult other colleagues when necessary.

Since the basic principle that I should develop my argument with respect to Hartlepool is accepted and in view of the gravity of the possible consequences should my worst fears turn out to be well founded, I feel these two requests are reasonable. Indeed, their refusal would tempt me to echo Superbrat and declare,

"You can't be serious!"

To save everyone's time and trouble by moving from serial to parallel operation, please consider this reply an application for permission to appeal to the Head of the Home Civil Service in the event of my requests being unacceptable. If permission to appeal is denied I will have a clear conscience knowing that I have done everything possible within the Civil Service system to draw attention to what I perceive to be a real danger of a significant failure.