The Assessment and Control of Major Hazards

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Discussion of Papers

Notes

1. Q/C means question or comment

2. R/A means reply or answer

3. When a speaker made more than one independent point, they are prefixed (a), (b) etc.

4. The name of the speaker is given in brackets, his application is in the list of delegates. Authors names are given in the preprints.

5. Unless stated in brackets reply given by presenter of paper.

SESSION 1 (morning)
PAPER 1

Q/C **(R.C. Griffiths)** In view of great uncertainty re estimation of hazard range, what will be the strategy of HSE in determining areas in which the public should be given information under the the new CIMAH regulations?

R/A We have to declare distances for various different purposes: firstly a consultation zone for planning purposes drawn widely so that sensitive or vulnerable developments just outside the zone are not at risk; secondly an emergency planning area which could not well be any less than the consultation zone; I personally favour making the two identical; whether there is any difference between the two zones depends on future discussions and would be decided by the county emergency planning officers.

Q/C **(Prof. J A Havens)** (a) How do you envisage the development of a policy or procedure for quantifying the exclusion zones which should be observed around installations handling large quantities of toxic materials? (b) Will the probability of the possible accident scenarios affect the process?

R/A We have proceeded on the basis of a protection concept. It is not based on quantification of risk at the present time - what is the most serious accident on the site that is reasonably possible? For example, for an LPG site, it would be the BLEVE of one vessel, even if several are there, or the unconfined explosion of the contents of one vessel. The boundary of the zone is at the distance where the consequence of the incident would be only a small percentage of deaths or serious injury, of the order of an LD05. When we started this concept a few years ago it was possible to estimate the consequences of an incident, but not the probability without multiplying the uncertainty. A consequence-based approach has another distinct merit when talking to local communities, being easier to relate to human experience than a mathematical probability (say) 10^{-5}, which then has to be further explained by comparison with other risks.

A toxic risk would also be described as a concentration or dose corresponding to a small percentage of severe injuries or deaths and a fair number of people hospitalised, but not many serious permanent injuries.

We have made calculations of risk in specific instances and have found that the biggest contribution to risk arises from fractured pipeworks, and not from vessels although the contents of the vessels are greater than that of the pipework. We found that it is not normally practicable to isolate pipework quickly in an emergency and have looked at remote isolation devices, automatic pressure actuated if necessary, to reduce the response time. Thus by quantifying the risks, one can better understand where to direct efforts to improve. Note isolation times (mins): manual 30, remote manual 5, auto 1.

Q/C **(P.J. Lynskey)** Under the CIMAH regulations 11 & 12, local authorities have to prepare off-site emergency plans based upon events defined by the companies; people possibly affected have to be informed to a range defined by HSE. We already have consultation zones for toxic storage in some areas of 1 km. As the authorities only have 6 months to review this information and develop their plans, what time scale is anticipated in producing guidelines on the principles to be adopted in estimating hazard ranges and informing people?

R/A I share your concern and we must get guidelines out quickly. A document is being prepared for publication, but is awaiting authorization. My initial view is that information to the public must go out to the boundary of the consultation zone. It is very important that emergency plans do not totally revolve around the worst accident; typically this would happen once or twice in the whole lifetime of the plant. The central core of the emergency plan should relate to the first step i.e. the smaller off-site accident - - with provision to escalate the arrangements.

Q/C **(M.H. Walter)** Following from the keynote paper, would Mr Barrell comment on the Regulatory position on transient accumulation of hazardous materials as in for example rail sidings. Will consultation zones be applicable?

R/A They are not covered by the regulations which I have described. A subcommittee has been formed to look at this problem to make recommendations within two years. These will give guidance for transportation hazards which will parallel those for fixed installations. In the context of major hazards, the type of situation you describe is not normally controlled.

Q/C **(OTTO GARA)** Setting acceptable risk criteria for LPG facilities. Leaving aside considerations of "what is reasonably practicable" what risk criteria (quantified) are seen as appropriate by HSE for the 3 types of land uses (A, B & C).

R/A Starting with the Royal Society report, examples of risks of death are given as unacceptable at one per thousand per year and as trivial at one per million per year, the HSE would agree with this. It is a band of risk, not a single target - - taking account of local circumstances. It is reasonable to expect a

higher standard of safety for a new plant than an existing plant. Also, take account of the societal risk as well as the individual risk, where a large number of people live near a plant.

Q/C **(J L HAWKSLEY)** You mentioned the draft HSE guidelines for determining the extent of emergencies to plan for. My recollection was that the guidelines did not address sufficiently the problem of what events reasonably to allow for. This seems to be a problem with some local authorities - they want to know the worst possible (e.g. vessel rupture) and plan for that, however unlikely, rather than consider the lesser but more likely events (e.g. pipe failure). Will your revised guidelines give more specific advice?

R/A I am not directly involved in providing that guidance and it is not addressed in that document. It will have to be sorted out individually with the various county emergency planning officers.

Q/C **(K L DICKENSON)** When considering the risk from an installation, is any account given to the benefit to be obtained from that installation?

R/A How to do that is rather difficult. The relevant transcripts from the Sizewell enquiry show that, that approach has not been very successful. It is difficult enough to quantify a risk, it is very difficult to quantify benefits. Not only commercial benefits in a narrow sense, others to be considered are employment opportunities, etc. So far the cost of the effort of carrying out an analysis of the benefits has not paid out in terms of any improved insight.

PAPER 2 (Replies by K WALSTON)

Q/C **(J L HAWKSLEY)** Is it possible to reduce corrosion allowances?

R/A Yes, but the corrosion probe does not itself truly reflect the corrosion rate of the equipment. It is more useful in controlling the rate of injection of those chemicals which are being added to reduce the corrosion rate of the equipment.

Q/C **(ARE MJAAVATTEN)** (a) How effectively will the monitoring system detect all kinds of corrosion? (b) Can localized corrosion still go undetected?

R/A Normallly by inserting a tests coupon of the same material as the pipe, you will see the same degree of corrosion on the coupon as on the pipe. However this will only be representative of conditions at that particular point. There may be effects of velocity, turbulence or condensation which are highly localised elsewhere, so you should not rely exclusively on the results from the probe or coupon to draw conclusions about the rest of the pipe, but also make ultrasonic or other non destructive measurements of the thickness of the pipe wall.

PAPER 3

Q/C **(D A JONES)** The risk factor varies as it is a combination of: (i) a defined hazard and (ii) its probability. In your paper you have indicated that 32 kW/m^2 was a selected hazard consequence level, this has a range of 1 proximity distance. Obviously there is only a single point on the pipeline which can interact with a building at 1 proximity distance, therefore the value of Pr is very small; say 1m/km. Your example uses a Pr value of 2 x Prox distance: this implies that a hazard consequence level of about 25 kW/m^2 is used. i.e. at square root 2 times proximity distance. If for discussions sake you select 5 kW/m^2 as the preferred hazard consequence level then this would have a range of four times proximity distance. A consequence of this is that the interaction factor will be 2 x root 15 ϕrox distances - which for your example would be 2xrootx15x77=596 m/km. How does this range of interaction Pr, affect your conclusion?

R/A The value of 32 kW/m^2 was used by the Institution to rate pipelines numerically one with another. It does not represent a safe position. If you take lower figures you will need to analyse the corresponding scenarios to arrive at the interaction distance and probability for each specific case.

PAPER 4

Q/C **(J LUNDLEY)** (a) A slide was presented showing the various devices which were down a pipeline before the "intelligent pig was inserted". What was the magnetic cleaning pig - could you describe its function? (b) Is the pipeline inspected before going into service to give a base line to clean out this debris?

R/A (a) It is designed to collect ferrous debris in the pipeline which might interfere with the operation of the magnetic inspection tool, e.g. welding rod ends. (b) It is not used in the commissioning phase before putting the pipeline into service.

SESSION 1 (afternoon)

Papers 5 (Ramskill) and 6 (Moodie)

Q/C **(R GRAY)** Why should one heat flux before venting in the 80% fill IT tank trial be 68% higher before the PRV opens than after venting has commenced? (You have discounted nucleate to film boiling as the explanation).

R/A **(K MOODIE)** Stratification will be more pronounced in the larger tanks and allow a separate layer of hotter liquid to develop above the bulk liquid. We should need to introduce a convection calculation into our model to take account of the stratification; this would be a departure from our simple starting point, but it could be done.

Q/C **(A J WILDAY)** Could you please clarify that when talking about two-phase flow you mean droplet carryover and not bulk flow of saturated liquid into the vent. If that were to happen, you

would need a larger valve to handle the quantity of vapour generated.

R/A Yes, our first guess was that droplet carry-over was occurring -- don't know for sure and this aspect will be investigated further.

Q/C **(C D PLUMMER)** Have the experiments or mathematical model (Paper 5) been extended to cover larger tankage containing mixtures of hydrocarbons and extension also to 'roll-over' situations?

R/A We are working at present on rewriting the model subroutine to deal with liquid properties of a hydrocarbon mixture such as petrol, and to take account of the differing rates of boil-off of the separate components.

Q/C **(P BARRETT)** Paper states that all relief valves failed safe (i.e. open) with one exception. What was the fundamental difference in relief valve design between the one that failed shut and the rest that failed open?

R/A No information was available on the design of the failed valve.

Q/C **(K Dickinson)** Can the computer model described in Paper No 5 be capable of dealing with a substance that is initially in the solid state, liquifaction only taking place at an elevated temperature?

R/A ENGULF cannot handle solid contents inside the tank. A complete rewriting of the program would be needed in order to be able to model this.

Paper 7

Q/C **(N.T.I ADAMS - written contribution)**

It is a generally accepted view that, a fracture mechanics assessment, depending on the material toughness and the calculated or assumed stress state used, could show either leak-before-break or sudden rupture as plausible. Although the SRD paper reflects this general view, the LPGITA believe there are a number of points which invalidate their conclusions and that in addition certain sources of information require further clarification.

The first page and the first sentence of REVIEW OF PRESSURE VESSEL FAILURE EXPERIENCE may be thought to imply that LPG vessels are not conventional, although they have always been built to the prevailing standard of the time. In addition, the operating environments associated with LPG vessels is relatively benign.

The second paragraph of Section 2 refers to minimum thickness vessels, presumably to code requirements. However, since fabricators often use available material thicker than the code minimum required, are you sure this is correct? It is not obvious what is meant by pressure parameters.

Although the stress concentration theory has been developed and pursued by SRD for many years it does not appear to have been

recognised as an acceptible method elsewhere. In fact, it is true to say that all fracture theories based on stress concentrations have been passed over in favour of the more scientifically based crack tip stress intensity theories.

Three methods of analysis have been used to arrive at defect sizes, either critical or tolerable. However, it does appear that the analyses lack consistency with regard to the stresses used. For example, in Section 7.1, shakedown is introduced, but not apparently in 7.2 and 7.3. Furthermore, a weld fillet stress concentration factor is introduced in 7.2 but not apparently in 7.1 or 7.3. Finally, the effects of residual weld stresses, significant in a non-stress relieved vessel, do not appear to be used consistently. It is also not evident that the crack sizes are quoted for identical sites, e.g. weld metal or parent plate by a saddle. Consequently, a comparison of crack sizes, based on different stress regimes in different material conditions cannot be made and would appear to invalidate the results of Table 1.

With regard to details presented in 7.1; Fig 3 (B) does not show any experimental points. As for Fig. 5 it could be argued that a straight line through the data points in inappropriate. It is suggested that bulging effects as expressed in eqn. 8 can be neglected, but how does this formula compare with the well recognised results of Folias of Erdogan and Ratwani. Do those quoted in Table 1 for non-stress relieved vessels relate to operating or test conditions?

In Section 7.2 there is reference to considerable scatter in Charpy results and the use of lower bound values in the PD 6493 analysis. Have the same lower bound values been used in Section 7.1 to derive S?

The fourth paragraph of Section 7.2 states that no fracture toughness data e.g. K_{IC} or COD is available, yet in Section 7.3 reference sources are quoted giving a range of values.

Quoting the text "Several fracture mechanics theories are available" - the document list 12 methods - .... What is being refered to here?

The Rolfe, Novak and Barsom correlation is quoted, yet no reference is given.

In Section 7.3 a thickness to K_{IC} relationship is quoted. Historically, one will find a great deal of evidence to show that plane strain fracture occurs at as low as, half this thickness in many materials. This relationship was defined for Code testing purposes to endeavour to unequivocally measure valid K_{IC} values and should not be used to define an expected fracture mode.

In Section 7.3 Dugdale and Cotton are given individually as ref. 13, but this is not the case. Burdekin and Dawes are not referenced.

In Section 8 a fatigue assessment is made based on assumed defect size whose initial length is equal to the vessel thickness and which is assumed to double in length. Yet this is in a range where, some results suggest the vessel could have already failed. In any case, is it not true to say that the leak would be detected? A more meaningful fatigue assessment may be either to define a size of partial penetration defect, that could have survived the proof test or been missed by inspection, and compute the number of cycles to penetrate the vessel wall.

In conclusion, from the information presented in the text, it appears that

a fracture mechanics assessment has been made using three different methods, two giving critical crack sizes and one providing tolerable crack sizes, that do not necessarily support a conclusion that unstress-relieved vessels may not exhibit leak-before-break behaviour.

Finally, we would like to reiterate the author's last sentence in the intro to his paper that, "However information to hand is neither sufficiently detailed in application nor nationally spread in implementation for any firm conclusions to be drawn about the whole UK LPG industry storage problems on the basis of any analysis of one vessel."

R/A **(T A Smith - written contribution)**

1. He had the impression that I did not consider an LPG vessel to be a conventional vessel.

This is not so; I do count such vessels among the national stock of conventional vessels.

2. On the subject of Fractures Mechanics there were alternatives to the Stress Concentration Theory (SCT).

I make this point clearly in the paper. Other procedures however require specific fracture mechanics (FM) properties which were not available for the compound under review. They would therefore have to be assumed or empirically derived from usual established material properties, or be assessed from tests on actual vessel plate used and heat affected zone material. Calculations using different FM approaches have been done for several types of component geometries space wide plate tests. The SCT results have been found to be well within the scatter band of values for crack sizes resulting from the calculations. Support for the SCT has lately come to hand from American where they have provided their Crack Plane Equilibrium Theory (CPET) which is based on essentially the same philosophy as the SCT and uses the same material properties.

3. Were the same stress levels used in the three approaches?

The SCT uses the far field membrane stress rather than the local total applied stress required by the two other approaches and the validity for this can be seen in the good agreement in Table 1 between the "without bending" figures and the 30×10^{-3} COD value - a reasonable value for solid steel at ambient temperatures. The "with bending" values are calculated using the applied stress used in the other calculations.

How residual stresses in an unstress-relieved component should be considered in stress analysis is still a subject for discussion.

4. Was the same Charpy value used in all calculations?
Yes, although as stated it is of far less significance in the SCT since it is only applied to the power of 1/3, i.e $3\sqrt{\ }$, which irons out the scatter in Charpy values. The other approaches require a specific Charpy value for correlation purposes.

Paper 8

Q/C **(J A HAVENS)** For the 1.2 second, axisymmetric simulation cited, what was the computer CPU time?

R/A **(T MAINI)** About 4 hours, on a Pax 11-780 computer.

Q/C **(D NEVILLE)** Dr Cuperus predicted a dynamic load 6 times static for the asymmetric fluid release case. Your results indicate a figure of 1.8. Can you explain this discrepancy?

R/A I think we have a more realistic fluid model. Dr Cuperus made several assumptions that simplified his fluid mechanics and that led to the large differences.

Q/C **(ARE MJAAVATTEN)** How does the width of the crack influence the maximum expected pressures?

R/A We carried out a series of studies with different sizes and also where the crack opens with time; it clearly affects the answers. This example had an opening of ten degrees.

SESSION 3

PAPER 9

Q/C **(R S F PULLEN)** Please comment on the effects of jet releases of toxic gases from say, pressure vessels, and the interaction of the directions of the jets and the prevailing wind. Could these be added to your model?

R/A To the extent that you could use conditional jet entrainment theory to start the calculation you could then use our model to follow the downwind dispersion of the heavy gas cloud from a known concentration. We have not done anything on that, but is reasonable. We have demonstrated the applicability of this model to the prediction of concentrations decreasing down to the flammability levels. It should not be assumed that it can also be applied to concentrations decreasing to toxic levels. We are looking into recent tests on dispersion of ammonia and nitrogen tetroxide.

Q/C **(L. DOELP)** How long can a release occur and qualify as an instantaneous release?

R/A By instantaneous release, I mean a release of gas. A release of LNG (as in the tests at Maplin and Burro) is of the order of a minute or more. The gas cloud formation resulting from a pool of boiling liquid lasts even longer. To avoid misunderstanding, the model which we have recommended to the U.S. coastguard is not limited to instantaneous or to continuous releases but in fact can be used to simulate any release for which you can provide the time schedule of the gas formation. The field experiments at Burro and Thorney Island were simulated as transient gas cloud formation problems, whereas the Maplin trials were simulated as continuous, which I think is reasonable.

Q/C **(J McQUAID)** I would just add that a review of heavy gas dispersion with particular reference to the provision of information for explosion modelling has been carried out by SRD for the European Commision. The report by C J Wheatley and D J Webber will be published shortly as a EUR report by the

Commission.

PAPER 10

Q/C **(J A HAVENS)** Your comment about the desirability of comparing concentration against time instead of distance is taken, but such a comparison is difficult when the cloud is transient or continuous, rather than instantaneously formed.

Q/C **(R J CARPENTER)** How do you propose to use a model calibrated against area average concentration to predict the hazard range for flammable gases.

R/A This is being considered but at this stage has not yet been reached.

Q/C **(FRANK ROPER)** Following a previous comment, it would be very useful for combustion workers if the analysis of dense gas dispersion could include, if possible, data on concentration fluctuations as well as mean concentrations. Data on variance and length scales of concentration fluctuations would be of great help in predicting the combustion behaviour of such clouds. This behaviour depends on the degree of mixedness as well as the mean concentration.

R/A In the present state of technology of combustion gas clouds, it is only possible to relate the total amount of gas in the cloud to the TNT equivalent. So long as that is the case, a box model description assuming a uniform distribution in the cloud, will suffice. We have not yet got to the stage of simulating combustion conditions based on a detailed description of concentrations distributed within the cloud. There is a huge number of measurements of concentration available in the detailed reports of the experiments (referred to by Professor Havens) —— not yet been examined in detail yet.

Q/C **(H A DUXBURY)** Professor Haven's comments, in the previous paper, related to concentrations of the order of the flammability limits. Do your validations relate to lower concentrations, relevant to toxicity problems?

R/A The concentrations illustrated in the slides were down to 0.1 per *cent, dictated by the lower limit of resolution of the gas* sensors used.

PAPER 11

Q/C **(J W HEMPSTEAD)** Where there is a lack of data for a particular chemical, is it possible to estimate the minimum lethal dose from the threshold limit value (TLV)?

R/A You could get a better idea from the ceiling values than from threshold limit values. I would argue however that TLV's are a bit more useful than the LD50 data, because more facts have been taken into account. The short term exposure limit or ceiling value is better way than TLV's, because TLV is based on 8 hour

working day exposure for 5 days per week i.e. a chronic hazard, whereas we are here concerned with acute effects.

Q/C **(N F SCILLY)** There is a trend over the last decade, to relate biological response to various injury causing conditions (fire, explosion & toxic) using Probit Transformations. I personally think that this can be misleading at the low & high ends of the relationships. Could you comment, please?

R/A I don't believe we can do that yet because of the imprecision of the data available. It might be possible where the toxicity is very well defined for example with chlorine, but not much else.

Q/C **(G PURDY)** Drawing on the previous question from Dr. Scilly, could you comment on the variation in human susceptibility to toxics, and particularly the uncertainty at LC_{03} - LC_{05} levels.

R/A Firstly there is very little information in this range of LC levels. I would point out that the variation in humans is much greater than in a single species of animal, because of the genetic, environmental, and disease resistance factors, etc. Extrapolating from data on chronic chemical exposure, ten-fold variations are quite common and thirty-fold variations are not unknown. I would expect that larger variation is much more likely with sustained exposure than with very brief exposure. *Ten-fold variability would be reasonable for brief exposure.*

Q/C **(J A HAVENS)** Would you explain your statement that "Based on an LD50 toxicity criterion, Bhopal couldn't (or shouldn't) have happened?

R/A Methylisocyanate is a chemical with a pretty modest LD50. It is not one which would give us special concern. Many other chemicals are manufactured in this country with similar values. In that sense, one would not have expected a major disaster with thousands of people being killed. It was beyond the conception of toxicologists that that sort of chemical would have killed so many people. Other factors obviously played their part, ground level of the plant emission, ill-health of population, susceptibility to infection, delay in treatment, etc.,

Q/C **(J LINDLEY)** Is there any point in stuffing large quantities of chemicals into a rabbit to determine an LD_{50} when there is no way in practice that such quantities could be injected?

R/A The answer is definitely no! There is absolutely no point in it because it has no relevance in practice. The defence of the toxicologist is that that is what legislation insists on, and we are trying all we can to stop it.

PAPER 12

Q/C **(G PURDY)** You mentioned your onsite plan relies on wind direction. Could you comment on the behaviour of dense gases when released in a complex terrain such as a Chemical works which

will travel out in all directions and indeed travel against the wind influenced by building wake effects.

R/A It is not practicable to draw fresh air from clean sources, which would have to be a great distance away to ensure a clean supply to the refuge building. We ended up with a combination of buildings, reasonably well sealed, and provision of gas masks to deal with the lower level of concentration in the buildings. When a particular refuge is downwind of the incident, the people there are gathered in one place and are in telephone contact with the control centre which gives them a breathing space. Knowledge of the prevailing wind direction can be helpful in directing people to a more suitable refuge, but the wake effect of buildings can take people by surprise, because chlorine can be split into two directions by the first line of buildings. In one particular case, the gas stood against the wind for a distance of about 20 metres.

Q/C **(J LINDLEY)** For a large site handling toxic substances, what do you think of the proposal for siting large loudspeakers facing outwards on site boundaries which can be individually activated, bearing in mind that the toxic substance and sound do both tend to travel downwind?

R/A Loudspeakers have only limited range. If it were possible to modify air-raid warning sirens, that would be a suitable device to be installed, provided that people are previously instructed what to do when they hear the sirens. A loudspeaker message may be heard but not understood.

Q/C **(R PULVEN)** For a large site the use of zoned sirens may offer a suitable method of warning specific groups of people under threat from a hazard such as a toxic gas cloud without alarming the whole local population. One problem remains and that is how to give the all clear after the passing or dilution of a toxic gas cloud, when probably the people are all indoors and will then need to get outside and to air their houses. How have you approached this problem?

R/A Send a man with radio and breathing apparatus and a box of Drager tubes with which to sample the atmosphere. Also instruct the police. As soon as the air is reported clear, the police vehicle equipped with loudhailer or traffic policemen is a very appropriate way to go around telling people to open their doors & windows and to come outside, or even knocking on doors individually to inform them. The operator with his box of Drager tubes, must be responsible for giving the advice that the air is clear.

Q/C **(C D PLUMMER)** Please comment on training of populace external to site, as siren suddenly going off would cause panic.

R/A We are setting out on an appalling public relations exercise. People have a very good perception of the hazard and no perception at all of the risk, particularly if you are providing the hazard and they are deriving no perceived benefit. This exercise is therefore so difficult that it will be heavily under-

played, when the full nature of the risk could so frighten people as to be counter productive. If the advice is to go indoors and close all doors and windows, that is what people would prefer to do rather than be evacuated. In an area with many similar risks in close proximity, it is useless for each factory to make separately its own approach to the public. It is then essential for the local authority to inform the public about the events that may happen, the sort of action they should take to protect themselves, and the county emergency services available to back-up.

Q/C **(E BULLOCK)** During his reply to an earlier question when talking about toxic releases and the best methods of warning the public, Mr Lynskey said that "it is a new baby which has suddenly been dropped on us; that we are in a learning stage and do not know the best way to deal with it". Toxic materials have been around for years and there has always been the possibility of an accidental release. More thought should have been given to this problem long before now; surely the CIMAH regulations do not alter the way in which you would warn the public; or would manufacturers prefer to keep the public in the dark, and trust to luck?

R/A I should have made it clear that I wholeheartedly agree with the idea that people should be informed. We certainly have communicated with our own parish council and similar people, as to the problems we have. We are about to do something wider, and we are doing it for everybody - including those who will find it difficult to accept the information, although the facts have been there all the time. It is inevitable on some occasions that we shall do it clumsily because we have not done it so widely before, and that will make problems too. We have had these problems for a long time and there is nothing new about emergency procedures. We should, and we have not, told people what to do downwind. Some criticism therefore is perfectly right. But I don't want to give the impression that we should keep quiet about these risks. The idea that people should be told is correct, and we wholeheartedly support it.

PAPER 13

Q/C **(T E FOYN)** Why do you disregard the late pressure peak in figure 7 when comparing the measured overpressures to the mode prediction?

R/A That peak is caused by the interaction of the flame and the wire ring support and is not related to the overpressure under investigation.

Q/C **(A RINNAN)** For what size of cloud is the model valid?

R/A The hydrodynamics concerned in a large explosion are independent of size. The main problems with scaling are associated with combustion. If you know the values of the burning velocity, this model will give the pressures whatever the scale.

Q/C **(K DICKENSON)** Author states that 'Height of cloud is sole

determinant of peak overpressure in far field.' This implies that peak pressure is independent of total quantity of material involved. Is this the correct deduction?

R/A Yes it is. One of the main points of this paper is to show that the peak far field overpressure is determined by the period of fast flame propagation and not the total cloud volume. Also, in the case of Flixborough therefore it is to be expected that the far field overpressure could be determined by the flame's propagation through the congested regions of plant, where the turbulence generated would produce the high flame speeds of the explosion. TNT equivalance methods based on total cloud inventory therefore will not always provide realistic estimates of far field overpressure

SESSION 2 (afternoon)

PAPER 14

Q/C **(M S HUGH)** Does the largely axial projection of the Pemex incident end cap fragments cause the authors to change (or update) their recommended probability distribution (Fig.8)?

R/A The recommendations are based on more than one hundred vessel failures and more than two hundred fragments; such recommendations would not be much influenced by a single incident. There were quite a lot of fragments projected normal to the axis.

Q/C ***(C PIEDESSEN)*** Comment on 'end-tubs' Mexico incident:
The preferential axial direction is confirmed. However most of them were sent south of the site, probably because of the piping connections being at the North side of the cylinders.

Q/C **(K W LAMB)** (a) Are there any significant effects on missile range arising from mild steel vessels; American practice is to use quenched and tempered high tensile steels? (b) how confidently can results from transport incidents be applied to static LPG installations?

R/A (a) We have not taken into account the effect of stress relief. It may be that this would affect the way in which the vessel fragmentates. Part of the problem with the statistics is that in transport incidents many of the reported fragments originate from the chassis and other parts of the vehicle while the vessel itself splits into only three or four major fragments. (b) We try to be as consistent as possible in interpretation of the results and we believe that there is no major difference between transport and static vessels.

Q/C **(M F SCILLY)** Have you any information to differentiate between the possible differences in fragment range for vessels made from high tensile and normal steels?

R/A Generally, there is not enough information on this aspect at the present time. It would be useful to try and pursue this further.

Q/C **(K DICKENSON)** Given that storage vessel fragments lead to a significant fraction of the far field hazards in Bleva type incidents, what steps have been taken to modify the design of such vessels so as to minimise the risks?

R/A The Association of American Railroads conducted a substantial programme of work in the 1970's into behaviour of oil tank cars and some of their data is included in our survey. Their work did consider tank modifications to reduce the risk of rocketing and reduce fragment range. They found that the only method which would have any significant effect on oil tank cars was to tie the ends of the vessel together with an internal structure. However, in order to resist the enormous thrust generated by flashing liquefied gases, the weight of the tank car would be increased and the capacity reduced to the extent that this did not appear to be a feasible method. It would also do nothing to mitigate against the release of the vessel contents and so measures to prevent the accidents occuring were concentrated on. (reference is AAR-R146, RA-12-2-23, Dec 1972)

PAPER 15

Q/C **(I A WATSON)** Contrasting the effects of the Agadir earthquake (1960) in a non-earthquake protected area and the 1980 earthquake of a similar magnitude near San Francisco centred near the Lawrence Livermore Nuclear Laboratories. In the latter the damage to the earthquake protected structures was relatively minor although extensive.

R/A In San Francisco in 1980 you were some way from the epicentre. In Agadir in 1960 the earthquake happened right under the town square. It is common that in this circumstance and where towns are built without any earthquake resistance in mind, the damage is devastating.

Q/C **(M J PIKAAR)** Is it possible to predict in any reliable way the onset of an imminent earthquake?

R/A No - not at present. The locations can be predicted from geological information where the majority of big earthquakes will occur. We cannot yet say when.

Q/C **(J LINDLEY)** Some time ago whilst researching storage tank incidents, I came across a few very large storage tank failures in Japan in which major scan failures occurred giving tidal waves of oil which swept over the bunds. The failures were said to be caused by foundation failure. Could these have been caused by seismic activity?

R/A There has not been an earthquake of magnitude say 7 right underneath a critical facility, or a petrochemical facility, in the last few years, in order to test how new equipment and techniques could withstand it. In particular, computers and disc drives could all crash. There was an example of this last year in North Wales. I cannot answer your specific question but earthquakes have been blamed for many things!

PAPER 17

Q/C **(C D PLUMMER)** As your models were based on something similar to distillation columns they seem rather tall and slender, did you base diameter and height and wall thickness on practical designs?.

R/A Yes - we did.

Q/C **(C D PLUMMER)** As it is normal to layout a plant in logical process sequence, this may place two high hazard units adjacent, your program would reposition these. Have you in your program addressed yourself to increased piping costs that this would produce.

R/A We tried to keep to a logical process flow. That was one of the constraints put on the program but the economics of pipework were not included.

Q/C **(D NEVILLE)** (a) Were all the overpressure waves sharp fronted shocks or did some have significant positive risk times? (b) The relationships between pressure and reflected pressures on cylindrical structures are well known for sharp fronted shocks. Did you compare your results with predictions and if so *what was the agreement?*

R/A We were unable to obtain either funds or an engineer expert enough to undertake the comparison of our results with predictions.

SESSION 3 (Morning)

PAPER 18

Q/C **(B W ROBINSON)** Hazop studies can be more effective in identifying top events than Phil Holden indicated. Certainly the top event such as fatality from a toxic column of explosive from flammable change. Hazop can identify mechanisms leading to particular top events that lone workers may miss and also smaller scale top events which may nevertheless cause fatalities. Hazop *stands for Hazard and Operability Studies which are generally* applied at an advanced stage of the project. Hazard studies based on a specially developed set of guide avoids which are less well known than those for P & I diagrams can be carried out at a preliminary stage of a project aimed mainly at more significant hazards and make a significant contribution at this stage.

Q/C **(M BOCKAMP)** Cafos of Dr Lihou will make the connection between Hazop and fault tree analysis. Though Cafos was designed to analyse processes, it was made suitable to analyse electrical systems. A problem was to find an electrical engineer that was familiar with Hazop. If Mr Holden is able to find such an engineer familiar with Hazop, Cafos can certainly be adapted to his problem.

Q/C **(J LINDLEY - written contribution)**

I would like to make several comments upon the contents of Mr White's paper, upon a couple of points arising in his presentation and finally to ask a specific question.

The FAR figure of 4 (fatalities/10^8 man hours) for the chemical industry quoted in the paper was taken from work reported by Trevor Kletz who quoted this figure for the UK Chemical Industry for, I believe, a 10 year period 1952-1961. More recent statistics indicate that, for the period post Flixborough, the FAR has fallen to approximately two for the chemical and allied industries.

For the 1960's, the ratio of background risks (ie ordinary accidents such as falling from ladders etc) to specific risks associated with the chemicals and processes involved was about 1:1 as quoted by Mr White in his paper. However for the 1970's, this ratio has changed to 4 or 5:1 such that the chemical risks have become far less significant, as shown by P L Holden in the previous paper, quoting ICI data reported by J T Illidge. This reflects ICI's success in reducing chemical process risks by improved plant design etc. In applying criteria of acceptability opposite risks to life, ICI consider the man (or group of people) at greatest risk. This is usually the plant/process operator.

It is important to enquire how accident statistics are derived. If they are based upon the total number of employees then a large fraction of these people may not be exposed to chemical risk (eg sales staff, personnel etc) and so the actual statistics upon which any target criteria are set should take this into account if it is intended that no person should be exposed to any risk greater than the average for other workers in the industry. The chemical risk may be concentrated upon only 25% of the total number of employees, for example, and the actual average risk to which the process men are exposed may, in fact, be higher than had previously been estimated by the use of the industry statistics.

Assuming that there are, on average, five main classes of chemical risk on a chemical plant (eg fire, explosion, toxicity, asphyxiation etc) we have, in ICI, traditionally set our criterion of acceptability for any specific risk equivalent to an FAR of 0.4.

Hence, using Mr White's symbols, for a continuously operated plant presenting a risk to operators 24 hours a day for 365 days/year,

$f(h) \times P(k/h) = 0.4 \times 10^{-8} \times 365 \times 24 = 3.5 \times 10^{-5}$ per year

ie not more than one fatality every 30,000 calendar years.

For a process operated only on days for 40 hours a week the equivalent figure becomes

$0.4 \times 10^{-8} \times 40 \times 50 = 0.8 \times 10^{-5}$ per year.

ie not more than one fatality every 125,000 calendar years.

Turning now to a couple of points opposite the CIMAH Regulations, Mr White and a number of other speakers have implied that industry will be carrying out hazard and risk analysis in the preparation of safety cases. However, supported by advice from the Chemical Industries Association (CIA)

background work in support of safety cases will not involve detailed fault tree and estimates of probability and risk contours unless it is seen as being crucial to the demonstration of safe operation. It is implied in the CIMAH Consultative Document that if a manufacturer was unable to meet the objectives required in the safety case using his own staff, doubts would arise about his competence to manage a major hazard plant.

Returning to Mr White's paper, is it implied in Figure 2 that fatalities are only likely to occur on the 250 major hazard plants in the UK? In our experience, fatalities are by no means limited to only major hazard installations.

R/A (R F WHITE, G H HELSBY - written contribution)

1. Dr D Griffiths questioned whether risk is F x C or whether it should include risk aversion, F x C".

Mr White: The risk equation is R = F x C. However, it has been suggested by several sources that the risk equation should be $R = F \times C^n$ to account for an aversion by society of high consequence accidents. This is a misconception because the multiple fatality statistics already reflect risk aversion ie. they are not a straight line on the F-N plot but curve sharply downwards at high values of N. If we use these statistics as a basis for a societal risk criterion then an empirical fit to such a curve may have the form $R_{CRITERION} = F \times C^n$ eg. for the purpose of inclusion in computerised calculations. However, this does not affect the definition of risk itself.

2. Dr. Griffiths also raised the question of the perception of risk.

Mr White replied that in the paper the authors were presenting a generalised and rational way of arriving at criteria based on fatality statistics which are accepted, in the present climate of opinion, by society. The purpose is to illustrate to practising engineers that data are available upon which logically derived criteria can be based. Arguments based on the perception of risk are rather more emotive and sometimes illogical and usually philosophical. As such, these aspects are outside the scope of the present paper.

3. Mr G Purdey expressed concern at using historical data as a basis for new plant ie. perpetuating the same risk with no improvement in new plant.

The author's response was that the aim should be to make the risk as low as reasonably achievable (ALARA). However, when risk is quantified we need some measure of acceptability. The purpose of a criterion is to provide some measure of the upper limit of risk which would show whether ALARA is low enough. It is not intended that the risk due to new plants should be designed to lie exactly on the criterion.

Furthermore, a cornerstone of the CIMAH legislation is the control of planning around a Major Hazard site. Therefore, although existing plant may place both individual members of the public and society at a higher risk, in the future new plant will presumably result in a lower risk to both groups from the same frequency of event and release of material. These reductions in risk would stem directly from an improvement in siting policies.

4. Mr J Lindley made the following comments and questions.

(i) Comment: There is a large number of plants which re not designated as Major Hazard, in the sense of the CIMAH legislation, which have the potential for causing substantial number of fatalities off-site.

Answer: Surely this reflects on the adequacy of the definition of a Major Hazard Plant.

(ii) Question: Why have you used 250 as the number of sites. Other speakers have referred to 500 - 1000 plus sites.

Answer: The value used in the paper was simply an illustration of how to arrive at a societal risk criterion.

(iii) Comment: The FAR used is out of date.

Answer: In the paper the purpose was to demonstrate how the FAR, whatever its level, could be used to arrive at a quantified risk to both an operator and an off-site individual. The numerical values in the paper are merely used as examples. The data quoted by Mr K Davies of 0.4 per 10^8 man hours, which is a more recent value for the special risks, only emphasises that a good on-site safety record can be used as a demonstration of adequately low off-site risks.

(iv) Comment: The risk to operators is different from that given in the paper if one considers shift work.

Answer: The above statement would be true if we consider a group of workers because then the total man hours at the plant is > 8760 but not all by the same operator. However, in the paper we were considering the risk to a particular operator in which case his exposed hours at the plant is 2000 per year whether the plant operates days only or shift work. This was done so that we could compare the risk to a particular operator with that to a particular member of the public living near to the plant.

(v) CIMAH legislation says the manufacturers alone must carry out the safety case.

This is not true. The consultative document and HSE guidance notes make it clear that the manufacturers must (a) be aware of all hazards and (b) accept responsibility for the safety case. However, a number of paragraphs in the consultative document particularly address the usefulness of consultants in providing specific areas of expertise (which a site may not have) or in giving an independent input to the safety case.

(vi) CIMAH does not require quantified risks (Chemical Industries Association policy).

It is our understanding that CIMAH does require quantified risk. At a conference organised by HSE on the Control of Industrial Major Accident Regulations at UMIST on 2/3 April 1985 it was specifically stated by HSE staff that quantified risk assessments would be an essential part of any safety case. The impression was gained that any safety case which did not address the quantification of risk for the site would not be considered adequate.

This issue has been supported on numerous occasions including a number of papers presented by HSE at this symposium.

Q/C **(J L HAWKSLEY)** I would like to comment on your use of some

data from ICI showing the improvement in safety as assessed by the reduction in fatalities due to process risks since systematic techniques of hazard assessment have been applied. That needs some qualification. Those techniques have made a significant contribution not to be underated. Over the same period, much attention has been given to broader aspects of safety management — establishing systems and engineering compliance. These have made a significant contribution. In cases for the CIMAH regulations it could be more important to demonstrate adequate safety management than to present numbers. QRA cannot say that a plant is safe — only indicate that it could be safe subject to broader considerations. Bhopal disaster is the latest testament to need for good management.

R/A There were indeed many changes which impinged upon safety during the period covered by the ICI data, for example the introduction of the Health and Safety at Work Act. While I agree that many of these were important and that one cannot separate the contribution from quantitative analysis, I do believe that there is no basis for believing that such a dramatic improvement could have been made without it. I would endorse your comment that a risk assessment can only show that there is confidence that a plant in inherently capable of safe operation and that competent management is necessary to ensure that it will be safe.

Paper 18

Q/C **(HOLLAND)** in Delft a program developed by Lihou is being used. Could we have comments on the use of this.

R/A We are most aware of the method refered to but much is learned when doing HAZOP studies i.e. plants can and should be designed for lower risk to operating personnel based on available statistics. It is much more difficult to assess the risk to society. HAZOP is not much help here.

Q/C **(B ROBINSON)** Thinks Holden has underestimated value of HAZOP — may be right for major hazards but there are many minor hazards to be considered. A different check list is required and can be very powerful in identifying the major hazards.

Q/C **(ICI)** The importance of involving plant operators and supervising staff in HAZOP should always be considered.

Paper 19

Q/C **(G H HELSBY)** The paper indicates a technique for calculating societal risks. This may not be valid in areas of very large population density. The required frequency which is calculated on the basis of the consequence - may not be achievable. For example considering maximum credible accidents it may be the required frequency would be 10^{-8} - 10^{-10}/year. This should be compared with external events such as seisure events (frequency 10^{-4} events/year) and aircraft accidents (10^{-7} events/year).

Paper 21

Q/C **(C PIETERSEN)** There is a large overestimation of damage distances in LPG example. In Mexico, heavily damaged area ± 600 m diameter, quantity of LPG involved was greater than 10,000 m^3. Damage distances shown (± 3000 m) are unrealistic

R/A The distance of ± 3 km quoted was for a single release of 8000 tonnes, and was for third degree bu ns, which can be expected at a lower heat flax than 'heavy damage.' A single release, for a Mexico –type installationn is clearly very unlikely.

Paper 23

Q/C **(R P PAPE)** Could the author tell us about variability in performance, both between individuals and for one individual on different occasions?

R/A We do know what the uncertainty bands are - purpose to get a more novel understanding of what goes on Yes we could tell you what probability of a person being vaiable in performance. We are interested in population characterisation to within an order of magnitude.

Q/C **(J LINDLEY)** It is generally assumed that learning curves become asymptotic, but did Mr Williams find any evidence that performances can reach a maximum and fall away as errors creep in owing to incorrect practices coming in, perhaps giving evidence for a need for retraining.

R/A The asymptotic nature of the curves are an artifact of the way the curves are plotted. Reliability technology is concerned with orders of magnitude - logarithmic plots. Most experiments don't last long enough to get sufficient data.

There is not enough informationn to say whether changes in practice affect the numbers.

Q/C **(B W ROBINSON)** Maintenance of trip systems is repetitive task. Experience and skill level was certainly high enough. When responsible for this type of work, recognised that craftsman responsible for testing for weeks on end became more error prone and to avoid this were rotated between different jobs.

R/A Individual effects are small in reliability terms compared with population trends. He refered to numbers given in paper relating to men who had been on same job for a long time. All the numbers available seem to show that people do not become more prone to errors with time.

SESSION 3 (afternoon)

PAPER 24

Q/C **(R E HEATH)** Exploring a similar technique in Mond, confidence limits on risk assessment have been found to be in the region of ±1½ orders of magnitude. Translated to distance uncertainty for a particular plant, this means that a given risk level could lie

anywhere within a band of 500m to 1500m from the plant. How can planning advice be given in the face of such uncertainty?

R/A I think the first point to make is that in the paper we advocate the use of best estimate approach, as opposed to a conservative approach, on the principle that the best estimate is uncertain and we could talk about the actual levels of uncertainty, but if one attempts to be deliberately conservative you are probably making the final result even less certain. So let's aim for a best estimate accepting that there are numerous assumptions and that you hope there will be a swings and roundabouts effect - done quite consciously. We find all the way through that there is a tendency to err on the side of conservative, and this has to be watched. Having done that, you produce a figure which is uncertain. Once again in the paper we suggest that the uncertainty was at least half the order of magnitude, and in fact that was really just the uncertainty on the analysis of cases. e.g. lengths of pipe, number of gaskets, the sizes of gaskets, and so on, plus uncertainty about the rates of release of material if a pipe breaks or if a gasket weeps, and those uncertainties for the things that we were guessing the half order of magnitude. There are also the uncertainties in the dispersion model, and in people's reactions plus mitigating factors. So the message is that you have got an uncertain result.

I think that going through the process you have actually improved the quality of your knowledge but then when you come to look at a proposed siting you have to bear the uncertainty in mind, and I think that you have to adopt the more or less classic 3 band approach. If you come out with a risk level which is very high the answer is unambiguous; but those boundaries can be set three or four orders of magnitude apart, and what you have got in the model is a big grey area. You should avoid trying to set very rigid and precise numerical criteria so that you get in the situation where you are just one side or just the other side of a precise line, and the thing to do is, to do the assessment, see what the answer is, and then to consider the other factors including uncertainty in making the judgement. Bear in mind that I was talking specifically in the context of advice to planning authorities, and we should explain to them, the customers, that there is this degree of uncertainty. We should try to set the results into context of other risks where there will also be decrease of uncertainty, and ensure that the decision maker, knows what the situation is.

Really what I am saying is that the exercise is worth doing because it improves the quality of knowledge, but it certainly does not make it perfect. Does that deal with the point?

Q/C **(R E HEATH)** Yes, could I come back to you and say how do you get 1 1/2 orders of magnitude.

R/A You are absolutely right; it is to a large extent judgemental. We played about obviously judging bounds on failure rate data and it may be different for pressure vessels.

Talking about 90% or 95% is neither here nor there. We have done that sort of thing. We have established subjective facts for a number of parameters and then tried to do the correct thing. Do

you end up having to subjectively make allowance for other factors.

There is one other aspect which I think people ought to bear in mind, and this is something which the Rhinemond Authority and others embarked on. On a strictly comparative basis, other things being equal and the same assumptions, you are in a better position comparing project A with project B than you are with comparing project A with some outside standard, and I am not saying that that is going to solve all the problems, but it does rank them a lot more reliably than you can rank them against an absolute value. We all accept this uncertainty, and just how far away is it is a debatable point. If you simply change a few parameters which define alternative A and alternative B and leave everything else the same, then that comparative judgement should be very real and have a lot more conscience than the absolute value.

Now the Rhinemond Authority are beginning to build up a risk data bank for things that go on in the Rhinemond area, and there has been a big stepwise change from the earlier study to the present safety package. The study has in fact been run through the safety package to re-establish what the modern computer will produce, so it is in fact comparable now to this transportation issue. We are on the same package with the same type of calculation. Though not perfect, it is far more realistic and helpful, and if they can build up a feel for it which is another way of looking at it, then they have a feel for the absolute levels. If over a period of years they can build up a feeling, using a standard package, that this operation is riskier than that one, then they know where they are going and how they relate. I think we should bear in mind that we are not always comparing with absolute criteria.

Q/C The thing that we have is quite deliberately described as a tool, and it is very useful as a tool to test sensitivity to uncertainties or different assumptions and so on and we found it surprisingly robust. We have been surprised at how robust it is and that one can play around with varying assumptions to see what happens.

Q/C **(B W ROBINSON)** In practice I have found a more useful technique than confidence limits is to consider sensitivity of the result to changes (say) a factor of 3 or 10 in individual basis of data or information used. This approach has been widely used in assessment of frequency and should be applicable to consequent work. The difficulty in working and confidence limits for the final results of complex computation may result in lack of credibility of the confidence limits quoted.

R/A These figures have to be used by the decision maker who will use your judgement on the confidence limits when making his decision.

Paper 25

Q/C **(R C GRIFFITHS)** Given all the experience that there is concerning the use and production of toxic irritant gases, such as chlorine , is there an emerging consensus about the toxic response relationships that should be used, and how can the

substantial uncertainties be reduced?

R/A There is a consensus on the chlorine by not ammonia - there are differences which affect the end result. The Dicken line and the US Coast Guard are referred in the I Chem E Working Party on Chlorine report to be published shortly. A separate report on ammonia reaches somewhat different conclusions. We are homing-in on a band, more certain than before.

R/A We all have reservations because of the many different ways of doing the tests all claimed to give the LC50.

Q/C (C NUSSEY) Pointed out that the consequences of exposure depend on a number of factors

R/A Problem is that you cannot check one item alone whether it be toxicity or something else like rate of evaporation.

Paper 26

Q/C (J Davies - written contribution)

The authors assume, without discussion, that the relation between the preconditions of an accident and its outcome may be represented by fuzzy implication, i.e. that the outcome of an accident is fuzzily implied by its preconditions. I don't think this is an adequate description of the true state of affairs. To my way of thinking, there is a very large element of chance in the relationship and, so far as I can see, this is not explicitly allowed for in the authors' procedure.

Also, I'm put off by the apparent precision in the outcomes as reported in the test cases of Table 8. The outcome for a given test case is a fuzzy set, and the precision claimed by the authors is arrived at by an arbitrarily chosen defuzzification procedure. The authors' findings would have had a greater air of plausibility if the outcomes had not been defuzzified, but had been translated into linguistic terms (e.g. many killed, few killed, etc.) either manually or by some automatic method.

The authors say nothing of the shape of the fuzzy outcome sets. If the sets have longish plateaux then the knowledge base (i.e. the set of all fuzzy implications) is incomplete: if, on the other hand, the fuzzy sets are multi-modal, then the knowledge base contains inconsistencies. The former state is pretty harmless, but the latter state is dangerous.

The authors offer no method of assessing the validity of their procedure. I'm sure something could be thought up for this purpose by borrowing some ideas from the field of cross-validation.

R/A (P VAIJA - written contribution)
Indeed in our paper a discussion is missing concerning fuzziness of a relation between accident preconditions and its consequences. However, intuitively a similarity is felt between concepts of fuzziness and probability.

Accident engineering problems in which these concepts could be used are similar or coincide. On the other hand, between the concept of fuzziness and probability there exist essential differences. Probability is an objective characteristic and fuzziness is not. A grade of membership is subjective.

Mostly it is not easy to recognise what is of fuzzy and what of statistical nature. Perhaps there is one heuristic rule available. The set of input information on which fuzzy description is based does not depend directly on the law of large numbers.

Moreover a concept of fuzzy probability is available /1/ for the case when it is desirable to preserve a structure of classical statistical analysis. A theory of possibility /2/ represents a definitive departure from this conventional statistical analysis.

Several philosophical problems remain unsolved connected directly with statistical representation of probability /3/. However, an industrial accident is a truly unique event and therefore conventional statistical treatment of accident files is of little or no use. Therefore expert systems and especially fuzzy based expert systems might be an adequate formal tool for which loss prevention and reliability engineering have been waiting for.

The fuzzy set is not yet a well known concept. This is why a sort of numerical representation is used to interpret the resulting fuzzy set. We do agree that a considerable information loss is connected with such defuzzification. A probability density is specified by its mean value under certain conditions as well. May be that linguistic interpretation of results would be more adequate, see e.g. /5/.

The Table A gives full specification of answers no 1-5. A discussion concerning an interpretation of fuzzy results, including multimodality, is given in /4/. We believe that provided our knowledge of accidents is inconsistent so it must be kept as such even in expert base.
Two statements, namely statements no 64 and 66, have been activated by the question no 1 (see Table 1) The form of these statements is (see Table 7)

64 LO VF HG ME ME LO LO
66 LO VF HG ME HG LO ME

Each activated statement is considered separately. The answer given by any active statement has always a form given is Fig. 2 (point a, b,c and d). The abbreviation DVV means dependent variable value, namely property loss. GM is the grade of membership of points a,b,c and d. The fuzzy answer is a fuzzy union of the fuzzy answers of all activated statements. The questions 2 and 5 are fuzzified (see Table 9).

Table A. Full specification of results to answers.

question	activ. statem.		membership function of answer a	b	c	d
1.	64	DVV	0.00	0.00	25.29	26.00
		GM	0.00	0.10	0.10	0.00
	66	DVV	20.00	25.29	75.33	100.00
		GM	0.00	0.70	0.70	0.00
2.+	33	DVV	5.53	5.53	5.53	5.53
		GM	0.00	0.28	0.28	0.00
3.	44	DVV	0.00	0.00	20.00	26.00
		GM	0.00	0.80	0.80	0.00
	64	DVV	0.00	0.00	20.87	26.00
		GM	0.00	0.00	0.69	0.00
4.	70	DVV	20.00	20.86	96.00	100.00
		GM	0.00	0.11	0.11	0.00
5.	-					

+ this answer is the answer to the fuzzified question no 2.1.

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GENERAL DISCUSSION

Q/C **(A MOTTERSHEAD)** In hazard and operability studies and for that matter risk assessment, we make a number of theoretical assumptions based on a designers perception of how the plant will be operated. We must make sure the plant operators share the same understanding otherwise we can be deluding ourselves in the safety of our plants. This leads to the belief that operator and supervisor involvement in selected studies and audits can only benefit from that better shared understanding.