HAZARDS XI

New Directions in Process Safety

16 – 18 April 1991, Manchester

DISCUSSION OF PAPERS

Notes

1. Q/C means question or comment (delegate's name).
2. R/A means response or answer by presenter (in capital letters) irrespective of other authors.
3. All presenters and delegates (with affiliation) appear in delegate list (obtainable from Tony Thompson, Organiser or from IChemE, Rugby).

SESSION 1 (CHAIRMAN: Dr N. GIBSON)

Paper No. 3 :MOONEY

Q/C (T. Kletz). Many explosions have occurred in reactors used to make nitro-compounds. Could amino-compounds be made by an alternative route that does not involve hydrogenation of nitro-compounds?

R/A That is one for the chemists. Maybe the best engineering approach is to minimise inventories within as well as between process stages and more frequently consider continuous rather than batch processes.

Q/C (B.J. Tyler). This incident would not have been noted above zero on the Bradford Disaster Scale (see Paper 1), yet it was obviously very serious. Does this suggest that a more useful scale would take other damage factors into account or consider also the potential, as well as the actual, damage?

R/A Although serious and tragic the effect of this incident were relatively limited and it could not be described as a 'disaster', except in as far as any such incident is disastrous for those concerned.
Financial loss could be included in a 'disaster index' but the obvious emotional problem is how many lives equal how many £million of damage. Seveso also does not register on the proposed Bradford index.

Q/C (A. Rushton). Referring to page 390, Figure 6, was the pressure recorded at t = 40 mins (when heating oils were activated) unusual? If so, could experience of expected values have been used to forewarn operators?

R/A No. This pressure was normal and remained so for at least another 90 minutes. By t = 150 minutes the pressure was clearly abnormal but was not seen by the operators as explained on page 183. This is another lesson from the incident.

Q/C (H.A. Duxbury). I appreciate that the relief valve was not designed to handle the runaway reaction which occurred since you were not aware of it. However, you said that there was a relief system to handle off-gas. A bit of generally good advice for reactors is to set the relief device to open at as low a pressure as is practicable consistent with normal operation rather than at the design pressure — particularly in the case of two-phase applications — because the vessel can be emptying while the pressure is rising but below the design pressure. The reaction may also go more slowly at the lower pressure/temperature thus allowing more time. This may possibly allow the vent to cope with a runaway though not designed for it. What was your relief device set pressure in comparison to the operating and design pressures?

R/A The reactor design pressure was 5.1 barg at 343 degC.
The operating pressure was 3 barg at 170/40 deg, determined by the use of nitrogen over-pressure for batch transfers, rather than the near-atmospheric reaction conditions.
The relief valve was set at 5 barg.

I fully agree that relief devices should operate at the lowest practical pressure. However, the problems of containing any consequent release of toxics will also have to be considered.

Q/C (H.A. Duxbury). My advice should not be taken to imply that a full study need not be done. A thorough study should be done, and if a relief device is installed it should preferably be set to open at the lowest practicable pressure.

Paper No. 4: HJERTAGER

Q/C (J.L. Cronin). (a) Can the author comment on the effect of louvres and grating deck on calculation of the vent parameter in offshore modules?
(b) Can the author comment on explosion overpressure in non-homogeneous gas mixtures which are over-rich (above MEL) in localised areas?

R/A (a) If one has louvres or grating deck, as the outer boundaries of the module, the vent parameter must be calculated using the open free flow area as the relevant venting area.
(b) The mass of fuel injected into a module is perhaps the most important parameter in estimating the peak overpressure in non-homogeneous clouds. We find that in addition to this, the direction of the leak is also important. The maximum peak explosion pressure is found when the mass of fuel is equivalent to the stoichiometric mass for a homogeneous cloud in the volume. In this situation the pressure can be as high as for a homogeneous stoichiometric cloud. If the fuel mass was rich the corresponding peak pressures were smaller. (see *J Haz. Mat*, 19, pp 139-153, 1988).

Q/C (F.S. Melville). Shell and British Gas in their gas explosion models made reference to the so-called 'external explosion' and claim it can significantly influence the overpressure in a venting explosion. Have you encountered the phenomenon in your experimental work?

R/A I am aware of the controversy related to the influence and importance of the external explosion on the internal explosion pressure. My experience from large scale experiments is that if the internal volume is filled with obstructions, the influence is small. However, if the internal volume is empty the influence is larger.

Q/C (B. Waldie). In the early part of your talk you noted a significant effect of smaller obstacles with sharp edges enhancing flame speeds. To take these finer details into account in the numerical mode will presumably increase drastically the computing time/power required. Please comment.

R/A The numerical resolution in the 'Piper Alpha' calculations I referred to in my paper is 1 m. However, the Porosity/distributed resistance (PDR) method makes it possible to take account of obstructions that are smaller than 1 m. The PDS method may in principle take account of **all** relevant details in the module, provided the subgrid resistance formulae are available.

Q/C (H.J. Pasman). Regarding the unexpected small difference between homogeneous stoichiometric fuel-air mixtures and heterogeneous ones with respect to violence of explosion, did you notice any difference in delay times or in general in the pressure time curves for

these explosions? What level of detail could be covered in modelling the internals of the Piper Alpha type module and to what extent does this limit the predictions?

R/A The results I refer to in my paper are related to non-homogeneous clouds inside a 50 m^3 tube. In these experiments we injected a given amount of fuel, through various leak nozzles. After the amount of fuel was injected we ignited the mixture close to the leak point. We did change the ignition delay times and the pressure decayed for most of the tests due to escape of the mixture through the open end of the tube.

SESSION 2 (CHAIRMAN: Mr R.L. ROGERS)

Paper No. 5: Withers and LEES

Q/C (H.A. Duxbury). I question the validity of wartime experience on the effect of warning. In wartime people had pre-prepared refuges to go to. In peacetime they (ie the public) do not.

R/A What the wartime experience shows is that warning can have a marked effect on the level of casualties. As Professor Duxbury says, the application of this to the present day is not straightforward. There would seem to be three main factors which are relevant. The first is whether the person exposed receives warning in time to take action. The second is whether there is effective evasive action which can be taken. And the third is whether the person is knowledgeable enough to take such action. The V-1 was unusual in that the warning delivered to the target area, namely the sound of the motor cutting out, was clear and effective. The population expected air attacks and knew what to do. Many homes had shelters and even a dining room table provided quite effective protection. We have given in the paper a factor of 2 as covering the two extreme cases and have used a figure of about 2 as our base case but in most hazard assessment scenarios I would expect that the credit which should be allowed for warning would be relatively small so that the casualty estimate for the case of perfect warning and evasive action would need to be multiplied by a factor which is nearer 2 than 1.

Q/C (N. Riley). You mentioned the importance of warning in deriving your model. Has the model been applied to the SCUD missile attacks during the Gulf conflict where warning was probably of a higher standard than during World War 2 and fatalities were reported as very low except for the missile that fell on the barracks?

R/A I did not mention the number of fatalities for V-1s and V-2s. The V-2s caused some 4 deaths per rocket on average, the V-1s about half that number. This includes a small number of high-fatality incidents. So the number of deaths in other incidents was small; in some cases there were no deaths. So the experience with the SCUDS was more or less what I would have expected. We have had it in mind to obtain more information on the SCUD attacks in the Gulf War, but so far have refrained, as those concerned will have had other things on their mind.

Paper No. 7: JOHNSON and Wickens

Q/C (H.J. Pasman). What is the effect of oxygen enrichment on laminar flame velocity of the mixes and how does this fit into the scaling?

R/A Oxygen enrichment will increase burning velocity. However, in the high flame speeds observed in these experiments, the combustion is controlled by the **turbulent** burning velocity, and it is this that is scale dependant and which we are attempting to re-adjust using oxygen enrichment.

Q/C (A. Duxbury). I don't think I have grasped the significance, for a real plant, of the finding that the high speed flame propagation could go round a 90° bend in a 'congested' duct. Could you elaborate?

R/A The most important aspect to remember is that the type of flame propagation we are studying in these experiments is only sustained by certain densities of obstacles and seems to maintain its speed by shock interaction with the obstacles and flame. Our study was to determine these conditions which would not sustain this type of flame propagation and one of the aspects covered was the ability to sustain through a change in direction. We showed that the mode of flame propagation is not significantly affected by a change in direction of the congested region.

Q/C (F.P. Nichols). Please comment on the criterion distinguishing materials which detonate from those which do not.

R/A Depends on the reactivity of the chemicals involved — with the more reactive gases (showing high flame speeds) more likely to give detonation. Process of transition to detonation is not well understood and more work needs to be conducted to be able to identify those situations in which specific fuel types can undergo transition.

Q/C (T.A. Kletz). Have you studied the effects of sharp edges on the obstructions? (ref B.H. Hjertager's paper, p 26)

R/A Not in the small scale experiments. The type of flame propagation here may not be as sensitive to sharp edges on obstacles as found in the flame acceleration experiments.

Paper No. 8: Diener and JOHNSON

Q/C (A. Rushton). (a) In the water experiments were the ambient temperatures and humidities recorded?
(b) In the chlorine tests, could it not have been steam (not chlorine) that formed above the capture tanks?

R/A (a) Yes.
(b) The colour of the vapour above the capture tanks was yellowish-green in colour, indicating chlorine gas.

Q/C (B. Waldie). Water appears to produce a dense cloud of fine drops which may perhaps be so concentrated as to give coagulation by drop/drop collisions, leading to fallout of larger drops produced. Have any calculations of coagulations been done?

R/A Coagulation of water droplets is being postulated as one of the reasons why water behaved differently from other liquids.

The RELEASE model does not take into account drop/drop collisions and therefore does not account for coagulation. This is one of the acknowledged deficiencies in the model. So far, no calculations have been made with regard to drop/drop coagulation.

Q/C When orifice size changed, was mass flowrate held constant?

R/A The release rate (lbs/sec) follows the classical orifice type equation where flow is proportional to the square root of pressure, the square root of liquid density, and the release area. Under conditions of constant pressure, the mass flux (mass/unit area) remained constant even when the orifice diameter (0.125, 0.25, and 0.50-inch) was changed.

Q/C (A. Duxbury). (a) Were any of the initial temperatures above the superheat limit temperature (or rapid phase transition temperature at atmospheric pressure) for the liquids concerned? If so, was it a relevant fact?
(b) You drew attention to the unexpectedly high proportions of liquid capture with water at the higher superheat values (in contrast with other liquids). I saw a further difference — at the low superheat end. The water curve diverged from the theoretical curve immediately, whereas other fluids followed the theory at the left hand side of the diagram. Can you comment?

R/A (a) We have not compared the experimental temperatures to the superheat limit temperature. This is something that will be looked at in the future.
(b) Water was the only liquid released that approached 100 per cent capture at the boiling point. The maximum capture for all other liquids was less than 65 per cent. The other liquid capture curves were normalised to 100 per cent capture at the boiling point. It is difficult under these conditions to say just exactly how the liquids actually behaved, although water does appear to have a much faster deviation from the theoretical line at low superheat values. Why? I do not know at present.

Q/C *(J. Lindley). Have you carried out any tests on the release of LPG or thought about the* consequences of your work upon the storage of LPG? Industry practice is to lag the vessels with vermiculite concrete, for example, or to protect vessels from pool fires with water sprays. It is apparent, though, that if propane or propylene is released under pressure from, say, a pipework fracture it is likely to fully vaporise, with no rain-out, no pool formation and therefore no pool fire. So is it a waste of time providing such vessels with passive protection with the associated problems of under-lagging corrosion? It could be different with C_4's, owing to the lower superheat.

R/A In the late 1970's we performed some simple release experiments using an LPG containing about 95 mole per cent propane. The liquid was released from ambient (pressurised) storage through a 1-inch opening. The release was horizontal and approximately 3 feet above the ground. No liquid was observed to collect or impact the ground. A thermocouple was placed in the release stream about 3 to 5 feet downstream of the release point. Temperatures of -95°F were measured. Some experiments were conducted in which the release was directed towards the ground. In these experiments, liquid pooled on the ground. Other experiments

have been conducted with ammonia stored at ambient temperatures and the results were similar.

Ambient temperature releases of LPG containing large amounts of C_3's would not be expected to produce liquid pools unless the released stream impacted the ground or surrounding equipment. When ambient temperatures are very low, the tendency to produce a liquid pool will increase. In the U.S.A., LPG tanks are not protected from pool fires, but from torch type fires. The preferred method of providing passive protection in the U.S.A. is to grade the area around the LPG tanks away from the tanks and to provide water spray protection to prevent BLEVEs due to torch type fires.

Releases of liquid from C_4's could easily form pools of liquid. The amount of liquid reaching the ground would strongly depend on the storage temperature. Our research shows that there is a narrow temperature angle in which a release changes from a liquid/large drop steam that reaches the ground to complete aerosol production and no liquid rain-out.

A very few releases of iso-butane have been conducted. The results showed that ambient temperature iso-butane releases could produce liquid pools. No quantitative measurement of rain-out was made.

Q/C (K. Palmer). (a) Design of discharge arrangement is such as to produce laminar flow of liquid; this will encourage long throw of liquid.

(b) Does the model take account of latent heat of vaporisation. For water, LH is relatively high.

R/A (a) True, the design intent was to eliminate or minimise as many aerosol producing factors (except the effect of temperature) as possible.

(b) Yes, the latent heat of vaporisation is taken into account in the calculation of the amount of vapour produced through the liquid flashing process. Thus, two liquids having identical properties with the exception of latent heat of vaporisation would not be expected to have the same capture versus superheat curves.

Q/C (M.J. Pasman). Does the model account for the temperature drop in the expanding vapour due to the gain in kinetic energy (energy partition principal)? Aerosol may be formed due to this cooling (the effect would be small).

R/A The RELEASE model accounts for kinetic energy changes occurring during the depressurisation of the released liquid/vapour. The thermodynamic model would account for any temperature changes occurring due to the pressure changes if equilibrium is assumed. The model uses mass and momentum balances in its formulation, but a mechanical energy balance is not explicitly performed in the model.

Q/C (R.D. Turney). Can you explain the results with no superheat where not all of the liquid was collected. Was this due to the pressure and mechanical/aerodynamic production of aerosols?

R/A At zero superheat, the liquid is essentially at atmospheric pressure. The driving force for the release is the liquid head above the release point and any nitrogen pad pressure applied. The orifice shape (circular) used in the experiments would not cause a large amount of mechanical shear as witnessed by the smooth shape of the exit stream near the release point.

However, as the liquid stream moves further from the release point, it begins to break up into large drops due to aerodynamic forces and natural stream instabilities. As air is entrained into the stream, the relative velocity of the liquid and gases results in further drop breakup. The lowering of the partial pressure of the released vapour due to air entrainment results in a drive by the liquid to restore equilibrium. This results in liquid evaporation and liquid subcooling. A substantial amount of liquid could be converted to vapour through this mechanism. It is also possible that not all of the liquid reaching the capture surface was collected or reacted with the water solutions. This would also result in liquid captures less than 100 per cent.

Q/C (S. Revenberg). What is the influence of air entrainment on the liquid captured? Can you give an estimation of the amount of energy of this entrainment which causes extra evaporation?

R/A Air entrainment lowers the amount of liquid captured in two ways. First, the ambient air may be at a higher temperature than the liquid. If so, heat transfer occurs and results in evaporation. Second, the air entrained with the vapour lowers the partial pressure of the released vapour stream. As the partial pressure is reduced, the liquid evaporates in an attempt to reach an equilibrium with the vapour/air mixture. This also lowers the amount of liquid captured.

We have not made an estimate as to the amount of energy that is transmitted by either mechanism. The very low stream temperatures, compared to the boiling point, for chlorine, methylamine, and cyclohexane, indicate that a large amount of sensible heat is transferred. No doubt, considerable amounts of latent heat are also involved.

Q/C (T.A. Kletz). Are there any plans for studying leaks **above** the liquid level? In studying the effects of leaks it is important to know how much liquid is entrained (we often assume it is half the theoretical flash).

R/A There are no plans in this experimental programme to study leaks above the liquid level.

Q/C (A. Rushton). You ignored one of the methylamine results. If you don't ignore it your data can be fitted by a straight line rather than by the curves of cyclohexane and chlorine and of the RELEASE model.

R/A The data point was not ignored, it was suspected of being in error. The data for superheats below 15°C does not follow the straight line drawn through the other data points. A curve of the shape predicted by the RELEASE model fits the experimental data quite well.

Paper No. 9 : CARTRIGHT and Ebadat

Q/C (R.L. Rogers). The authors have determined that with the antistatic (woven conducting threads) big bag they used, an igniting discharge was obtained when none of the conducting threads were earthed but that they did not get ignition if 3 of the 4 sides were earthed. Have they determined whether igniting discharges occur with only one or two sides earthed?

R/A In one experiment, the conductive threads on all four sides of an antistatic FIBS were isolated from each other and from the earthing loop but the earthing loop of the FIBC was

earthed. It was not possible to ignite the gas emitting probe with discharges from the walls of the bag with isolated threads.

Additional comment:

The use of FIBC's in hazardous areas is in its infancy. There are few general rules to specify their acceptability. Currently each application needs to be assessed individually as the possibility of an ignition occurring depends on many factors including nature of the powder charging rate as well as the type of fabric used in the construction of the FIBC. For example we have determined that it is safe to use an 'antistatic' big bag constructed with a specific volume of conducting threads without earthing the conducting threads in situations of low charging with medium resistivity powder. Such bags are however unlikely to be safe for the case described in the paper of charging with polyester pellets.

Q/C (N. Gibson). (a) If a bag has a partial earthing failure your data showed no ignition. Would contamination modify this conclusion?

(b) In the absence of flammable vapours would ignition of sensitive dust clouds be obtained from the non-conducting bag? Is there a critical M.I.E.?

R/A (a) Partial earthing failure together with external contamination with a conductive contaminant could exceptionally lead to increased discharge energy. This is considered unlikely since: (i) extensive earthing failure is required, (ii) the contaminant would need to exclusively cover unearthed threads, (iii) the bag tested had conductive adjacent threads earthed at top **and** bottom. Double breaks along multiple adjacent threads would therefore be needed for the fault condition to occur.

(b) This question has not been answered by the investigation to date. Further information on discharge incendivity to dust clouds is available in RS.5958.

Q/C (T.A. Kletz). Could a discharge occur from the structure to an earthed point and ignite any dust cloud that is present?

In a conducting bag there are earthed points to which should discharges could take place.

R/A The investigation has not addressed this question.

Q/C To what extent can conducting fibre breakage lose interconnection/earthing and yet remain safe?

R/A Single fibre breakage would not lead to isolated conducting threads since each thread is earthed at each end.

The work also indicates that even if several adjacent threads all broke in 2 places, discharges incendive in propane/air would not occur.

SESSION 3 (CHAIRMAN: Dr J. BURGOYNE)

Paper No. 11: Lazari and BURLEY

Q/C (M. Braithwaite). Re Figure 15 Strain versus charge mass plot.

There is a deviation between prediction and experiment at low masses of explosives (less than 50 g). It is difficult to get small masses of secondary explosives to 'completely' detonate (due to run up, losses, etc). This might account for this deviation.

R/A Agreed (see note in body of paper).

Q/C (P.V. Rutledge). Will the vessel fail at the nozzle weld or the bolts?
R/A The failure will be at the weld. The bolts and flange are well overdesigned.

Paper No. 12: HURST

Q/C (D. Newton). Does the vessel failure data presented encompass both metallic and plastic vessels? Is data available which is specific to plastic tanks?
What is the HSE's view of the use of plastic storage vessels? Is sufficient design and data and practical experience available to ensure that their use is acceptable from a health and safety point of view?
R/A The data is primarily for metallic tanks. There is no specific plastic tank data.
I cannot comment on the 'use view' of plastic storage vessels.

Q/C (N. Madison). How does the methodology cater for missile penetration and knock-on effects?
R/A Knock-on effects are classified under 'domino'. Sabotage is included as an underlying cause of failure.

Q/C (R.C. Gray). Page 166 — Seeing that unexpected chemical reactions have been separately considered, what were the underlying reasons for inadequate vent/pressure relief systems (capacity of PRV, vent piping, **code** sizing method)?
R/A Appendix 2, page 166 is a breakdown of direct causes, in this case overpressure. The loss of containment accidents involving inadequate vent/pressure relief systems included (1) blow down system full of liquid, (2) invalid design assumptions, (3) failure of a safety valve, (4) pressure relief system blocked or jammed.

Q/C (J. Lindley). Following on from Trevor Kletz's comment on the underlying causes of vessel failure, I would like to make a point relating to Table 4 in Mr Hurst's paper. He lists various causes of vessel failure including overpressure, temperature, corrosion and impact. Operating error is also in the list, but surely this is a sub-set of overpressure, temperature, etc.
R/A Operating error is listed for the situations where it is a direct cause of the loss of containment. The underlying causes are listed separately.

Q/C (A.Z. Keller). (a) How does one deal with planning applications for new plants or extension of new plants on a green field site?
(b) How practical is it to use a simple 'generic' risk assessment method modified with 'shaping' parameters or factors obtained from site safety audits?
R/A (a) Applications for new plants are considered by the Major Hazards Assessment Unit. They might use RISKAT risk estimates to consider the location of such a plant.
(b) The practicality of the methods proposed will be tested by field trials of the methods once fully developed.

Paper No. 13: Neville and WHITE

Q/C (R. Gray). Your water test indicated an immediate stress in the shell to floor weld at a lower head of water due to initial flattening of floor buckles. Might rapid cooldown of the tank floor seriously increase this stress level? LPG tanks today are cooled by slowly running -40° propane on to the tank floor at a rate governed by tank pressure control.

R/A If the rate of cooldown is such that the tank floor is fully contracted before the tank wall then it may be possible to reproduce the high stresses at the shell-to-base weld toe. However, there are many reasons for avoiding rapid cooldown in low temperature tanks and in commissioning all such tanks I would recommend that the cooldown be undertaken in a slow and controlled manner.

Q/C (T. Kletz). Would you recommend that large low pressure tanks for storing liquids such as petrol or crude oil should not be fully emptied but rather that a heel equivalent to 1 m water is left in them.

R/A The filling and complete emptying of a tank with a distorted shell-to-base junction will cause cyclic stresses greater than previously thought. If these high stresses are repeated with sufficient frequency, then the fatigue performance of the shell-to-base weld may be below that required. For such tanks, it would be advisable to consider retaining a heel of liquid to reduce the cyclic stresses.

Paper No. 14: ROBERTS

Q/C (H. Pasman). Your last slide showing the new UK proposed classification scheme would certainly offer a solution in the short term, but if you think about all the problems you mentioned, would it not be more logical to classify the **hazards** rather than the **substances**?

R/A The UN classification system is still in the process of development. The main hazards are recognised but many substances present more than one hazard. The UN hazard precedence table is still not fully developed and more work is required to develop tests and criteria to adequately assess the degree of hazard. Once all the test methods and criteria are in place and the hazard procedure agreed, it should be possible to move to a more logical system.

Q/C (P. Rutledge). Does it make any difference if the bubble is air or nitrogen?

R/A The literature indicates that there is no difference. The mechanism appears to be adiabatic compression of the bubble followed by hot spot initiation. There is no difference in cavitated detonability tests.

Q/C (N. Madison). Are HSE intending to issue any interim proposals for 'new' tests (for guidance) before they are formally accepted by the UN (approx +18 months away)?

R/A The Department of Transport circulates all the UN proposals for comment before each UN meeting. HSE do not intend to issue any interim proposals. Normally, anyone proposing to set up a UN test should write to the National Contact (for Test Details) of the country of test origin. A list of these is given in the UN Recommendations: Tests and Criteria. The country of origin should be able to supply engineering drawings and sources of supply, the latest modifications to the test prescriptions, etc.

Q/C (N. Madison). Would it not be appropriate for HSE to give guidance on KNOWN problems/inaccuracies in performing the tests until more suitable tests are accepted?

R/A One of the purposes of this paper is to indicate which tests HSE use and to give some guidance on the application of the UN explosivity tests. The OECD-IGUS Energetic and Oxidising Substances Group will be publishing the full round-robin and will highlight problems in performing the tests given in the paper. HSE guidance will be combined with that from BAM, TNO and INERIS in this paper.

Q/C (L.H. Armstrong). Sensitisation of liquids by gas bubbles: Is there any evidence of a limiting size? i.e. bubbles small enough **not** to sensitise the liquid to impact?

R/A There is evidence of a limiting bubble size in BAM cavitated detonability tests on hydrogen peroxide. I would expect there to be a lower limit to the bubble size which can sensitise a liquid to impact. Initiation is usually considered to be by adiabatic bubble compression followed by hot spot initiation. Thermal explosion theory predicts a minimum size of hot spot for propagation to occur.

Paper No. 15: MERRIFIELD and Roberts

Q/C (J. Lindley). Could you indicate how Ethylene Oxide ranks in the Classification procedure and whether you can foresee any changes in future transport requirements/legislation?

R/A Ethylene oxide is currently classified as a gas (UN No. 1040) with flammable liquid and poisonous substance subsidiary risks. The UN Committee of Experts has just revised the recommendations for Class 2 (gases) and in the future ethylene oxide will be classified as a flammable gas (Division 2.1) with a toxic gas subsidiary risk (Division 2.3).

Q/C (J. Lindley). In the early part of your presentation, you showed a slide of the Peterborough explosion. Severe building damage was apparent, but I noted that there appeared to be a number of Portakabins in the area. Could you comment upon the effect of the explosion overpressure on these units and whether people are likely to be at less risk in them than in an industrial steel framed building, bearing in mind that people are likely to be killed in a collapsed building by the weight of the masonry?

R/A Portakabins at 50-55 m away were subjected to a blast overpressure of around 5 psi, and suffered severe damage. As far as I was able to establish, no one was in the units at the time of the explosion. The sides of the Portakabins facing the explosion were damaged most; metal cross members bent inwards and cladding material detached and deposited both on floor inside and outside the units. The roof and main frame of the units were generally intact. Persons inside the Portakabins at the time of explosion would probably have received injuries from perhaps flying glass/panels and from being knocked around generally. Persons inside industrial steel framed buildings at the same distance are, subject to the nature of the glazing and wall construction, exposed to the same sorts of hazards.

SESSION 4 (CHAIRMAN: Dr B.J. TYLER)

Paper No. 16: KLETZ

Q/C (G. Wells). Following up the Chairman's first question it is indeed useful to use key words such as 'avoid', 'modify', etc. followed by another phrase such as 'units', 'inventory', etc. We have found that the old technique of Critical Examination applied to safety studies at an early stage is very useful.

R/A For the examination of a flowsheet many engineers believe that the usual hazard and operability (hazop) guide words are suitable, although ICI recommend the use of different questions[1]. For the earlier study at the conceptual stage of design, different questions are certainly needed and I have made some suggestions elsewhere[2].

Hazop was developed out of critical examination., This is a technique designed to generate alternatives and it was adapted so that it generates deviations[3]. In an ordinary hazop of a line diagram we assume that a deviation, such as 'more temperature', is undesirable. We ask if it could occur and, if so, how we can prevent it. In the study of a conceptual design or a flowsheet we should also ask if more temperature might be desirable, that is, we are interested in alternatives as well as deviations. I agree, therefore, that the old critical examination questions should be useful at this stage of design.

1. J.L. Hawksley, *The Safety Practitioner*, Oct 1987, p 10.
2. T.A. Kletz, *Plant Design for safety—A User-friendly Approach*, Hemisphere, New York, 1991, Chapter 10
3. T.A. Kletz, *Hazop and Hazan—Notes on the Identification and Assessment of Hazards*, 3rd edition, Institution of Chemical Engineers, Chapter 7, forthcoming.

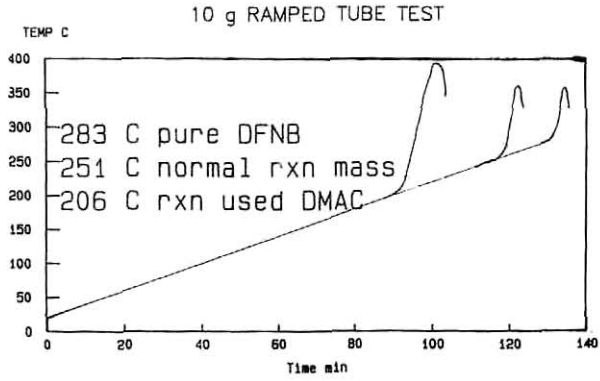
Paper No. 17: Rogers and HALLAM

Q/C (A.Z. Keller). As an academic I teach the principle that, having defined the functional requirements (including safety) of a system, one should always then strive to satisfy these requirements in the simplest way possible. Can this principle be applied with regard to inherent chemical safety?

R/A Yes.

Q/C (N. Maddison). Comment only: When considering reducing temperature (heat medium) from a High Pressure (ie HP steam), then work may need to be done on the steam to drop the temperature; dropping the pressure alone may not be sufficient.

Q/C (R.L. Rogers). Further comments with regard to the study of thermal instability are summarised in the following contribution with reference to the ramped tube test for a specific example.



Isothermal small scale tests:
— can be insensitive.

Use RAMPED screening tests:
— to search at higher temperatures;
— to test whole reaction mass;
— to test used solvent if recycling.

Search for cause
— if decrease in stability
— if yield affected.

Decompositions and side reactions
— often ENERGETIC.

Paper No. 18: WILDAY

Q/C (R.L. Rogers). Would you like to comment on how the ideas expressed in your paper link in to, or are consistent with, the new pressure vessel regulations?

R/A The Pressure Systems Regulations require that a suitable pressure relief system be provided **if equipment can be overpressurised**. The approach outlined in the paper is therefore allowable under the Regulations, as I interpret them.

Q/C (K. Palmer). Vessel surrounded by fire — it is important that the surface above level of contained liquid does not exceed 600°C, at which steel loses strength.

R/A I agree entirely. External fire is the big problem in eliminating pressure relief. BLEVEs in external fires are a problem even if pressure relief is provided.

Q/C (T. Kletz). In the past, stronger vessels have not been used because the relief and blowdown review was held late in design. When it was suggested that a vessel be made stronger, so

that an R/V was not needed, it was found that the vessel was on the critical path and was already on order. Logistics, not technology, prevented a change.

R/A Yes, I agree. If environmental pressures push the industry towards eliminating relief systems, there will be a need to look at pressure relief much earlier in the design, so that vessel design pressures can be set accordingly.

Q/C (R.C. Gray). (a) Should you not put tanks below ground level, in preference to high in structure, to avoid external fire?
(b) Need to control liquid volume without PRV to prevent hydraulic fill with external fire. Gas tanks approach tanks 98% full with PRV to protect 100% full tank against overpressure *with 100% containment walls liquid-cooled.*

R/A (a) Yes, agree — whenever feasible.
(b) Yes. Often the best way of dealing with hydraulic expansion is by relief to a total containment vessel. External fire is a problem because the containment vessel could also be in the fire.

Q/C (T.L. Meyer). I do not agree with the recommendation to put a storage tank on an elevated level, because structural steel will weaken quickly when exposed to a spill fire; the structure will collapse and the tank break, contributing much more flammable liquid to the fire!
In case of elevated storage, even the use of steel insulation (e.g. by application of concrete covers) is still questionable in my opinion.

R/A I would agree, and putting the whole of a chemical plant at an elevated level was not a serious suggestion on my part. Robin Gray's suggestion of burying them all in the ground might be a better bet, but still has its problems.

Q/C (J. Lindley). Mention has already been made of the paper by Trevor Kletz on the above subject written in about 1974 in which it is suggested that the target hazard rate for an instrumental trip system should be one tenth of that for a conventional relief valve. However, in order to meet this target it is usually necessary to have a 1-0-0-2 trip initiator together with double trip valves which tends to give a rather complex and expensive trip system which will, of course, need treating and maintenance. In assessing the reliability of such a system it is essential to take into account common mode failures, which appeared on one of Jill's overheads. I discovered an example recently of a trip system which had been installed where common mode failure had been ignored. The two high pressure trip initiators were located on a vessel having a known fouling problem and this, or course, could have affected both pressure switches and had not been allowed for in the risk analysis. I would like to draw peoples' attention to an SRD document on defences against common mode failure which is mainly descriptive and very useful, namely: SRD R196 (A.J. Bourne, 1981), *Defences against common mode failure in redundancy systems. A guide for management, designers and operators.*

R/A Thank you. A useful comment!

Paper No. 19: Mercx, Weerhijm, Verhagen and PASMAN

Q/C (S. Collins). Does the presence of buildings or houses in a built-up area affect the overpressure as the shock waves propagate out from the site of the explosion?

R/A The presence of buildings does influence the blast propagation out from the explosion centre.
However there are, to my knowledge, no models available to incorporate these effects other than numerical fluid flow codes.
Existing empirical pressure damage relationships were derived based on free field blast propagation. To fill in this gap in knowledge, a joint research project between UK, Dutch and French laboratories, on the blast propagation and interaction within built-up areas was proposed to the CEC for funding; however it has not been granted yet.

Paper No. 20: HEWERDINE

Q/C (A.Z. Keller). How does one show that the two acoustic emission 'fingerprints' taken at different times are identical?

Q/C (R.C. Gray). Does your AE fingerprint on an ammonia sphere enable you to determine crack growth during the production period between tests and acceptability of result for another period of service?

R/A TO BOTH QUESTIONS (Keller and Gray)
The level of acoustic emission activity detected by each sensor during the monitoring period is graded from A (lowest) to E (highest), based on the total energy release and the rate of energy release measured by the sensor.
Fingerprints must be taken each time under identical conditions of operation/loading. In this case, significant changes in the grading associated with a sensor or sensors would indicate a change in the fingerprint, requiring further investigation to identify the cause of the change.
Such investigation will normally involve conventional NDT to locate and size any defects giving rise to the acoustic emission activity.
Evaluation of the equipment for fitness for purpose using a fracture mechanics assessment of such defects will then be necessary to determine acceptability for further service.
Conversely, absence of any significant change in the fingerprint should indicate suitability for further operation without any additional investigation.

SESSION 5 (CHAIRMAN: Professor T.A. KLETZ)

Paper No. 21: JONES

Q/C (R.C. Gray). In the light of Graham Tortorshell's work, are Fibre Optic communication systems safe in hazardous areas?

R/C There has been a considerable increase in the use of optical fibres and sensors, and they are being introduced into hazardous environments where there may be flammable gases and dusts.

It is HSE's experience to date that **low power** systems used in this context give no cause for concern, but clearly as energy levels increase problems could arise.

HSE has therefore commenced a research project to assess the practical and theoretical circumstances under which ignition could occur from optical system sources. The objective is to produce essential data for standards for optical equipment in dangerous areas.

Q/C (R.L. Rogers). Incident investigation provides root causes most of which are well known. Is any work being done to identify whether there is any difference in the level of appreciation/action or methodologies in companies with different accident/incident records?

R/A No such HSE work is being done at the moment because our database of **reported** accidents and incidents is insufficient to allow us to start analysing, with any confidence, trends with type of equipment, process or company.

Once we build up more data, HSE will then start looking for such trends, not so much for enforcement reasons, but to detect possible problem areas.

However, we suspect that many companies, particularly the largest ones, have a lot of in-house unreported data in this area, and I would urge them to share it with others, even on a totally anonymised basis, so that control system technology can be improved where necessary.

Q/C (A.Z. Keller). Is experience and practice obtained from 'fly by wire' military and civil aircraft being incorporated into HSE considerations?

R/A Yes.

HSE is in regular contact with both MOD and CAA in the application of computer control systems for aircraft, and the development of appropriate safety standards for them. An inter-departmental Government committee maintains a broad overview of all areas of use of PES in safety related applications. This ensures that practitioners, researchers and regulators can learn from one another, and generally keep up to date with relevant developments.

Q/C (R.L. Rogers). During introduction of PES systems a major area of concern was the possible conflict between control and safety. Is this now resolved or are there still problems?

R/A I think the term 'conflict' is overly strong in this context. There was, and still is, debate on the relative contributions of control and protection systems in chemical plants. For example, if one could guarantee a 100% reliable control system, it could then be argued that a protection system (eg ESD) is not then needed.

However, control system reality has reliability nearer 70-80% level, in which case a backup protection system is needed, which itself has a high reliability of responding on demand. The debate centres on how reliable (in statistical and hence cost terms) the back up protection system has to be.

The HSE PES documents talked in terms of safety related systems, for which control systems could be felt to be a major part. New international standards in this area are now referring to **designated** safety related systems, i.e. those which are there for specific safety purposes.

Paper No. 23: WELLS and Reeves

Q/C (T.A. Kletz). Violation is often due to:
(a) A failure to convince someone that the rule was reasonable and necessary;
or (b) Turning a blind eye to previous violations.
In these circumstances blame or punishment is inappropriate.

R/A It is important to distinguish between a mistake and a deliberate violation. A mistake is associated with training, poor human-machine interface, poor supervision, etc. In a blame-free culture the aim would be to discuss matters and get them put right. A deliberate violation of instructions, such as not to wear a hard hat deserves appropriate reprimand/punishment. Obviously there are grey areas in between. The two illustrations given clearly indicate an absence of the correct safety culture within the organisation and suggest that every effort should be made to rectify this problem whilst urging proper compliance to instructions by the individual.

Paper No. 24: SANTON et al

Q/C (P. Cartwright). Where does the legal liability rest with the proposed expert system?

R/A The question of liability is under active consideration by lawyers at present. It is recognised as a possible stumbling block. Liability will certainly arise and will be shared, so far as we can see, appropriately between the programmers at Salford University, IChemE, BMHB and HSE.

Q/C (A.Z. Keller). Is the 'expert' system under development a genuine expert system or an 'enhanced' user friendly system? What shell and what type of knowledge base will be used?

A/C The system under development will be a genuine expert system in that it will recommend a solution on the basis of the rules and constraints put into it by experts. The shell has been built specifically for the project using the AI language Prologue, and has been designed for ease of input of expertise directly by experts without programming knowledge.

Q/C Referring to p 328 in the papers, was there a correlation between field of work and interest in the system, e.g. interest in runaway exotherms may correlate with an unwillingness to use an expert system?

R/A Replies to questionnaires were not analysed in this way at the time. The data is still available and such correlation will be sought when resources permit.

SESSION 6 (CHAIRMAN: Dr C. WEBB)

Papers No.s 26 and 29: COTTAM and Taylor

Q/C (C. Webb). Is it possible to assess the risk of a currently considered safe, but genetically modified, organism, having been released into the environment, becoming potentially harmful due to further mutation/evolution? If not, what regulation should apply?

R/A The assessment of the risk from a genetically modified organism takes into account the organism's genetic stability and the likelihood of it being involved in gene transfer events.

Q/C (H. Londiche). Please could you tell us how the British regulation takes into account the hazards induced by the widespreading of genetically modified plants into an open medium in order to avoid such an accident as the 'corn rust' in the U.S.A.

R/A The risks from releasing genetically modified organisms into the environment is assessed on a case-by-case basis. This process includes review by the HSE's and the DOE's Advisory Committee on Releases to the Environment. This committee includes members with a range of scientific disciplines as well as representatives from both sides of industry, local authorities and farmers.

Q/C (K. Palmer). What is the state of the technology for the disposal of organisms from fermenters?

R/A Both physical (i.e. heat) and chemical methods are currently used to inactivate the living organisms that may be present in the waste from fermentation processes. The method selected depends on the process and the organisms used.

GENERAL DISCUSSION

Q/C (D. Brown). How do you see future uniformity in equipment design to meet with biological application?

R/A Being looked into — present concern.

Q/C (C .Webb). Is it possible to fix realistic safety limits on release of, or exposure to, microbes without major incidents to base these on?

R/A (A. N. Cottam, G. Leaver, R. Cumming)
In many of the processes involved in biotechnology there are few relevant exposure limits and none that apply to living organisms. There is a lack of adequate, quantitative data on the risks to health from organisms and their products. In the absence of such data the approach taken has been based on the principles of occupational hygiene. Exposure to any living organism, its components or products should be reduced as low as reasonably practicable. There are no current proposals for occupational exposure limits for living organisms and more epidemiological evidence is required before any such limit could be accepted.