PARTMENT OF EMPLOYMENT

HS010674

The Flixborough disaster

Report of the Court of Inquiry

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Appointment of persons to hold investigation

The Secretary of State, in exercise of his powers under Section 84 of the Factories Act 1961, hereby directs a formal investigation to be held under the said Section 84 into the accident which occurred on the 1st June 1974 at the factory of Nypro (UK) Limited at Flixborough.

The Secretary of State, in further exercise of his said powers under the said Section 84, hereby appoints:

ROGER JOCELYN PARKER, ESQ,
QC, (*Chairman*)

JOSEPH ALBERT POPE, ESQ,
DSc, PhD, WhSch, CEng, FIMechE, (*Deputy Chairman*)

JOHN FRANK DAVIDSON, ESQ,
MA, PhD, ScD, FRS, CEng, FIChemE, MIMechE

WILLIAM JAMES SIMPSON, ESQ
to hold the said investigation.

The Secretary of State further appoints BERNARD MICHAEL O'REILLY, ESQ, FRIC, to be Secretary of the said investigation.

Michael Foot
Secretary of State for Employment
27th June 1974

Contents

Abbreviations

AISI	American Iron and Steel Institute
BS	British Standard
BTU	British Thermal Unit
C	Column
DSM	Dutch State Mines
H	Hanger
HMFI	Her Majesty's Factory Inspectorate
NCB	National Coal Board
NRV	Non Return Valve
PCT	Process Control Technician
R	Reactor
S (four fig. number)	Separator
S	Section
SMRE	Safety in Mines Research Establishment
swg	standard wire gauge
V	Valve
X	Mixer

List of Plates

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To the Rt Hon Michael Foot, MP, *Secretary of State for Employment*

Introduction

1 At about 4.53 pm on Saturday 1st June 1974 the Flixborough Works of Nypro (UK) Limited (Nypro) were virtually demolished by an explosion of warlike dimensions. Of those working on the site at the time, 28 were killed and 36 others suffered injuries. If the explosion had occurred on an ordinary working day, many more people would have been on the site, and the number of casualties would have been much greater. Outside the Works injuries and damage were widespread but no-one was killed. Fifty-three people were recorded as casualties by the casualty bureau which was set up by the police; hundreds more suffered relatively minor injuries which were not recorded. Property damage extended over a wide area, and a preliminary survey showed that 1,821 houses and 167 shops and factories had suffered to a greater or lesser degree.

2 On 27th June 1974 you appointed us under Section 84 of the Factories Act 1961 to hold a formal investigation to establish the causes and circumstances of the disaster and to point out any lessons which we might consider were immediately to be learned therefrom.

3 As required by Sub-Section 3 of Section 84 of the Act we held our investigation in open Court. The hearings occupied a total of 70 days. They opened at the Wortley Hotel in Scunthorpe on 9th September 1974. After five days, mostly devoted to the taking of eye-witness evidence, the hearings were adjourned to London. In all we heard evidence from 173 witnesses.

4 Prior to our appointment, investigations and an interim report had already been made by Her Majesty's Factory Inspectorate (HMFI), and investigations were being pursued by many other persons and bodies. Between our appointment and the opening of the investigation at Scunthorpe the following steps were taken: (a) we held a preliminary meeting for the purposes of giving directions as to representation and procedure; (b) the task of assembling the evidence for presentation at the hearing was undertaken by the Treasury Solicitor; (c) Messrs Cremer and Warner, Consulting Chemical Engineers of 140 Buckingham Palace Road, London SW1W 9SQ, were appointed at our request as technical advisers to the Court and instructed to seek and provide to the Treasury Solicitor evidence as to the causes of the disaster; (d) the work of recovering, identifying and examining wreckage, and of conducting relevant tests, was undertaken by the Safety in Mines Research Establishment at Sheffield (SMRE) to whom we wish to pay a special tribute; (e) further investigations were pursued by other persons and bodies.

5 During the course of our investigations a large number of scientific reports were submitted to us and circulated to all parties. A complete list of such reports appears in Appendix VI. Many of such reports although of great value to the ascertainment of the cause of the disaster have no wider application. Others contain scientific information of a more general kind which, we believe, would be of value to publish. We have marked the papers which fall into this category with an asterisk.

Scope of the investigation

6 The evidence before us established conclusively at an early stage two basic facts.

(a) that the cause of the disaster was the ignition and rapid acceleration of deflagration, possibly to the point of detonation, of a massive vapour cloud formed by the escape of cyclohexane under at least a pressure of 8·8 kg/cm^2 and a temperature of 155°C.

(b) that such escape had emanated from a section of the plant known as Section 25A. This section was devoted to the production of cyclohexanone and cyclohexanol by the oxidation of cyclohexane with air in the presence of a catalyst.

7 Our detailed investigations were, therefore, primarily directed to ascertaining the cause of such escape, and we were only concerned in detail with other sections of the plant to the extent that they were or might have been connected with the operations of Section 25A, the escape therefrom, or the ignition

of the vapour cloud resulting from the escape. We shall, however, refer to the other sections of the plant additionally to the extent that they form part of the general background of relevant circumstances or lead us to the conclusion that we should point to some lesson or lessons immediately to be learned.

8 With regard to lessons to be learned, we were at all times conscious that you desired us to establish the causes and circumstances of the disaster as speedily as possible and to point out only immediate lessons arising therefrom. Therefore we did not consider that we should investigate, or seek to make recommendations upon, such general matters as the proper policy with regard to safety, siting, layout and construction of plants such as that at Flixborough.

To have done so would have involved the taking of an immense amount of evidence concerning the practice at similar plants throughout the world and concerning the social, environmental and economic consequences of possible changes in current policy, practice or safety requirements. This would have delayed this Report by many months. We were reinforced in our view as to the limits of our task by the fact that, on the 27th June 1974 when you announced our appointment, you also announced that you would be setting up an expert committee to examine the hazards of large scale plants and the ways in which people working in them and living nearby could be safeguarded. The detailed terms of reference of this committee were published on the 4th November 1974 and were:

"To identify types of installations excluding nuclear installations which have the potential to present major hazards to employees or the public or the environment and to advise on measures for control appropriate to the nature and degree of hazard over the establishment, siting, layout, design, operation, maintenance and development of such installations as well as overall development both industrial and non-industrial in the vicinity of such installations."

The membership of the committee was published on the 9th January 1975 and it began its work immediately thereafter.

9 However, it emerged during the course of our investigation that there is urgent need for a reconsideration of certain aspects of current policy and practice. Notwithstanding the limited nature of our task, we have therefore thought it right, in that section of this Report which deals with lessons to be learned, to recommend urgent review of such aspects.

Site and general history of Flixborough Works

10 The Works (see Plate 1) are situated on flat low lying land on the east bank of the River Trent about six miles to the south of the point at which that river joins the Humber. They occupy an area of some 60 acres. The nearest villages are Flixborough itself and Amcotts on the opposite bank of the river (both about half-a-mile distant) and Burton-upon-Stather (about two miles distant). The town of Scunthorpe is approximately three miles away.

11 The immediate neighbourhood of the works consists largely of farms and has a very low population density; the perimeter of the works is surrounded by fields. The siting of the works was fortunate. Had they been in a densely populated area there is no doubt that death and serious injury would have been on a much greater scale and that the property damage, even if more localised, would have been more serious. The facts that 72 out of 79 houses in Flixborough, 73 out of 77 houses in Amcotts, 644 out of 756 houses in Burton-upon-Stather and 786 houses in Scunthorpe were damaged to a greater or lesser degree speak for themselves.

12 The site was originally occupied in 1938 by a Company called Nitrogen Fertilisers Limited, a subsidiary of Fisons Limited and used for the manufacture of ammonium sulphate. In 1964 Nypro was formed, owned jointly by Dutch State Mines (DSM) and Fisons Limited. It acquired the site from Nitrogen Fertilisers Limited. Between 1964 and 1967 plant was built for the production of caprolactam, which is a basic raw material for the production of Nylon 6, by a process, the first step in which was the production of cyclohexanone via the hydrogenation of phenol. The works were commissioned and production commenced in 1967. They were then and remained until the disaster the only works in the United Kingdom producing caprolactam.

13 In 1967 Nypro was reconstituted with DSM owning 45%, the National Coal Board 45% and Fisons Limited 10%. Almost at once design began for additional plant to increase the capacity of the works from 20,000 tons of caprolactam per annum to 70,000 tons per annum. This additional plant, referred to as Phase 2, was completed at a cost of some £15m in 1972. Its distinguishing feature for present purposes is that in it the cyclohexanone necessary for the production of caprolactam was produced by the oxidation of cyclohexane instead of via the hydrogenation of phenol. In 1972 DSM

acquired the holding of Fisons Limited and from then until after the disaster Nypro was owned as to 55% by DSM and as to the remaining 45% by the NCB.

14 From the safety point of view, the oxidation process introduced a new dimension. Cyclohexane, which is in many of its properties comparable with petrol, had to be stored. More importantly, large quantities of cyclohexane had to be circulated through the reactors under a working pressure of about 8·8 kg/cm^2 and a temperature of 155°C. Any escape from the plant was therefore potentially dangerous.

Layout of the works

15 The layout of the entire works as it was at the date of the disaster is shown in Fig. 1 and Plate 1. At the time when Phase 2 was begun the main office block and laboratories were already in existence. As will be seen from Fig. 1 and Plate 1 the addition (Phase 2) involved the hazardous Section 25A being built in close proximity to these buildings. Both were completely destroyed in the disaster. Following accepted practice, the control room was in even closer proximity to Section 25A, and was also destroyed. None of these buildings was designed or constructed to protect the occupants from the effects of an explosion; however, following a recommendation by HMFI, shatter-proof windows were installed in the control room before the accident.

16 In the event there were no personnel in the main office block at the time of the explosion so that there was neither loss of life nor injury caused by its position. There were personnel in the laboratory but all escaped from it. In this case also its position did not therefore result in loss of life or injury. The situation with regard to the control room was very different. Of the 28 killed 18 were in the control room at the time. No-one escaped. Had it been differently sited or constructed or both, loss of life might have been avoided, and records might have survived which would have greatly assisted the task of this inquiry.

17 In stating the above we desire only to record the facts. One can do no more than say that lives might have been saved. Had certain measures been taken those who died might have suffered before death overtook them. If, for example, the control room had been of specially reinforced construction and had been protected by blast walls, its occupants might well have been alive after the explosion only to be roasted or asphyxiated in the fierce fires which raged thereafter. The siting and construction of control rooms is a complex problem to which we shall revert later in the Report.

18 At this stage in our Report the only other matters to which we draw attention in connection with layout are:

(a) the hydrogen plant which was a possible source of ignition,

(b) the nitrogen plant and the storage tanks from which came the nitrogen essential to the safe operation of several sections of the plant, particularly 25A, and

(c) Section 26 which was the only other section of the plant to use high pressure nitrogen.

Organisation of the Company

19 For present purposes the material period for examination is from the beginning of 1974 until the date of the disaster. This period is complicated by two matters. The first is that the works engineer left the Company early in the year and had not been replaced at the time of the disaster. The second is that there was a re-organisation in process due to become fully effective on 1st July 1974.

The Board of Directors

20 This consisted of six directors. The Managing Director was and is Mr R E Selman. He is a qualified chemical engineer (HTS Delft) with much experience of cyclohexane plants and with detailed knowledge of the Flixborough plant and its operation. He was appointed an Executive Director of Nypro in 1971 and became its Managing Director in 1973.

Essential Management

21 The organisation was headed by a General Works Manager Mr J H Beckers. He is an experienced chemical engineer with a qualification from MTS Heerlen. He was directly responsible to the Managing Director.

22 Seven executives were directly responsible to Mr Beckers. They were the three plant managers covering respectively area one and utilities, area two and area three; the Technical Manager; the Chief Chemist; the Instrument Engineer; the Works Engineer.

23 There was, additionally, a Safety & Training Manager, Mr E Brenner, whose precise position in the management structure appeared to be somewhat uncertain but who regarded himself as responsible to the Personnel Manager albeit that he had a right of access directly to the Managing Director.

24 Up to the time when he left the Company the Works Engineer was a Mr Riggall who was a chartered mechanical engineer. Thereafter, although active steps were being taken to replace Mr Riggall, there was no effective works engineer although a co-ordinating function was exercised by a Mr Boynton whose permanent position was that of services engineer. He holds an ONC in electrical engineering and an NCB (Class 1) engineering certificate. His experience before going to Nypro in 1966 had consisted of two years as a maintenance and repair workshop foreman and six years as an electrical supervisor. He received some maintenance and management training from Nypro, was employed by them from 1966 to 1970 as electrical engineer and from 1970 to the date of the disaster as services engineer. He was in our view not qualified to act as a co-ordinator of the engineering department of a plant such as Flixborough and should not have been asked to assume this responsibility even for a short while.

25 Of the plant managers it is necessary only to mention Area 2 (which covered Section 25A) and Area 1 (which also embraced Utilities). The Plant Manager for Area 1 was Mr C L Bell, a chartered chemical engineer of considerable experience. Area 2 was, up to 1st May 1974, in the charge of Mr P H Cliff, a chartered engineer (Institute of Fuel) who had as chief deputy or assistant Mr R Everton who holds an HND in Chemistry. From 1st May onwards Mr Cliff was engaged partly in assisting Mr Everton, who took over from him on that date, and partly in preparing himself for the new post of production manager in charge of all areas to whom the plant managers would be directly responsible. This post he was due to assume fully on 1st July 1974.

The Engineering Department

26 Responsible to the Works Engineer until he left were a Planning Engineer, Mr Wigfall, the Area 3 Engineer, Mr Brough, the Services Engineer, Mr B T Boynton and the Areas 1 and 2 Engineer, Mr A B Blackman. For his Area 2 duties, so far as immediately material, Mr Blackman had under him, as assistant Area 2 Engineer, Mr R Culpin and Mr C G Frow, the section supervisor for Section 25A and three other sections. None of these were professionally qualified mechanical engineers although all had some technical qualifications and practical experience. Mr Blackman in particular is in our opinion a thoroughly reliable practical man albeit, as will appear later, he was at material times under an excessive work load which led him into error.

27 We have set out the position in some detail because it is of importance in relation to what occurred later. The engineering structure was, and was recognised by the Company to be, weak. By reason of this the Company in 1974 called in Mr J F Hughes of the National Coal Board to advise on re-organisation. Subsequent to this call for assistance the position worsened as a result of the departure of Mr Riggall. From then on there was no mechanical engineer on site of sufficient qualification, status or authority to deal with complex or novel engineering problems and insist on necessary measures being taken.

This too was recognised by Nypro, for on Mr Riggall's departure, Mr Boynton and the other engineers were told that if they had problems they could call on Mr Hughes for assistance. Mr Hughes was only sporadically on the site but it was possible to communicate with him, albeit this might involve some delay, because he had many other heavy responsibilities as Assistant Chief Engineer (Operations) of National Smokeless Fuels Limited, a subsidiary of the National Coal Board with an annual turnover of approximately £95m. This weakness in the engineering structure was rendered the more serious by reason of the fact that the General Works Manager and the Managing Director were both chemical engineers with no mechanical engineering training or qualifications.

Section 25a—The process and layout

General description of the process (see Fig. 2)

28 The oxidation was carried out in a chain of six reactors (R2521 to R2526) each set 14 inches below its predecessor in the train so that the flow from reactor to reactor was by gravity. In operation the reactors contained cyclohexane to the appropriate levels at a working pressure of approximately 8·8 kg/cm^{2}g and a temperature of 155°C. The reaction was caused by the introduction of air and catalyst into the heated and pressurised cyclohexane.

29 The product of the reaction consisted of a process fluid still containing approximately 94% of

cyclohexane; the other 6% was primarily cyclohexanone and cyclohexanol with unwanted by-products.

30 In subsequent stages of Section 25A, the process fluid was separated by distillation into two parts, namely:

(i) cyclohexane which was recycled to the inlet where it mixed with fresh feed and re-entered the train of reactors;

(ii) cyclohexanone and cyclohexanol, which passed to other sections of the plant for conversion to caprolactam.

The Section of the plant where this is carried out and the flow path for the various fluids is shown diagrammatically in Fig. 2.

Detailed description

31 We set out here, albeit under the heading detailed description, only such details as are necessary for an understanding of what follows. A plan and elevation of the relevant parts of Section 25A are represented in diagram form in Fig. 3. There were originally six reactors, but at the time of the disaster No. 5 (R2525) had been replaced by the "dog leg" 20 inch by-pass shown in Fig. 3.

This plan and elevation also show the position of an 8 inch pipe connecting the separating vessels S2538 and S2539. This 8 inch pipe lay directly behind the train of reactors, and was an important feature in our investigations. Its position in the flow sheet is indicated in Fig. 2.

32 Steam heating, both for initial warm up and as required to supplement the heat generated by oxidation once the process had been begun, was provided through a heat exchanger (vessel C2544), (Fig. 2). The cyclohexane which was fed into this heat exchanger came from three sources:

(a) from storage (obtained from outside supply);

(b) by recycling from the distillation section;

(c) by condensation of cyclohexane vapour from the off-gas line from the train of reactors.

33 The flow of steam to the heat exchanger and thus the temperature of the cyclohexane was controlled by a valve indicated in Fig. 2. This valve could be either operated manually from the control room or set in the control room to operate automatically to maintain any given temperature in the cyclohexane emerging from the heat exchanger and entering Reactor 1 (R2521). This control valve could, however, be by-passed by opening a block valve on the by-pass line indicated in Fig. 2. The by-pass was needed during start up to provide sufficient steam to reach operating temperature within a reasonable time. When oxidation began, the control valve, either alone or with the block valve slightly open, was adequate to maintain the operating temperature.

34 When, or shortly before, the desired temperature was reached, the valve would be put on automatic setting, to maintain the desired temperature. The block valve would then be closed to a point where it would provide any necessary additional steam which might be required. The degree of closure would vary with operating conditions. The fail safe position of the automatic valve was closed.

35 The heated cyclohexane passed from the heat exchanger to the first of the reactors and thence onwards down the train. Separate air and catalyst streams were introduced at the bottom of the reactors.

36 During the oxidation process, part of the cyclohexane evaporated. This served to keep down the temperature by taking away the heat of reaction. The evaporated cyclohexane, together with nitrogen and residual oxygen from the entering air, passed through an off-gas line into the heat exchanger, Fig. 2. From thence, the off-gas passed to the atmosphere via the cooling scrubber, absorber and flare stack. Cyclohexane was condensed and absorbed in the cooling scrubber and absorber, so that the final stack gas was mainly nitrogen with minute quantities of process fluid.

37 The off-gas line, being open to all reactors, served to equalise the pressure in the reactors, and contained a number of operational control and safety devices.

38 In the first place there were safety valves set to open at 11 kg/cm^2 which would release excessive pressure in the reactors to the flare stack. Secondly the off-gas line incorporated a pressure control valve leading from Absorber C2522 indicated in Fig. 2. This valve could be set in the control room to maintain any given pressure in the reactors by automatically controlling the flow to the flare stack. It could also be operated manually from the control room or completely over-ridden by closing one or both the block valves on either side of it (see Fig. 2). Once the block valves had been closed, which was

done on the plant and not in the control room, the pressure in the system could continue to rise until either the safety valves operated or an operator on the site manually opened the block valves again.

39 Thirdly, there were also incorporated into the off-gas line two oxygen analysers, which, in the event of the oxygen content in the off-gases exceeding 4% — above which level the off-gases might become combustible and ultimately explosive — would both shut off the air supply and cause the reactor system to be purged automatically with nitrogen from the nitrogen storage. This was fed into the off gas-line by a high pressure nitrogen line shown in Fig. 2, at a pressure of 12 kg/cm^2.

40 Finally the system provided a shut-off of air supply and an automatic nitrogen purge in the event of the level of the liquid nitrogen in the high pressure nitrogen storage tank falling below a pre-set point. This automatic safety device could, however, be simply and effectively over-ridden by setting to zero the timer for fixing the duration of the purge. The timer adjusting device was readily accessible and easy to operate and was situated behind the Section 25A panel in the control room. This same timer dealt with the nitrogen purge for excess oxygen in the off-gas line referred to in paragraph 39. We shall revert later in this Report to the question of nitrogen supply.

41 The train of reactors was connected by short horizontal lengths of 28 inch internal diameter stainless steel pipes and stainless steel expansion bellows, the minimum internal diameter of which was about 28 inches. In operation, these large diameter pipes were only partially filled with liquid and thus served in addition to the off-gas line to help equalise the pressures in the reactors.

42 The fluid emerging from the last reactor passed into an after reactor (S2529, see Fig. 2) which was merely a container in which the reaction could be completed, and thence to the neutralisation train, the purpose of which was to remove from the fluid corrosive acids formed in the reaction process. This was effected by the injection into the process fluid of a solution of caustic soda to a specified maximum level of chloride. Exceeding such level could lead to corrosion in the pipelines.

43 On passing from the after reactor the pressure in the system was increased to 15 kg/cm^2 by a pump (one of two in parallel) and the fluid then passed through a chain of four mixers and three separators. The mixers were for adding caustic soda solution, and the separators for separating the aqueous and organic phases. Two of the mixers (X2529 and X2531), and two of the separators (S2538 and S2539) are shown in Fig. 3.

44 In the separators, the neutralised product (the organic phase), being lighter, separated and floated on top of the caustic soda solution (the aqueous phase).

45 The separators S2538 and S2539 were connected by an 8 inch internal diameter stainless steel pipeline (shown to scale in Fig. 3). The pipe was supported on two hangers H and contained (a) two mixers X, (b) a non-return valve NRV, and (c) a manually operated stop valve V.

46 For present purposes, it is necessary only to state that the processes following neutralisation consisted of:

(a) a reduction in the pressure of the neutralised fluid from 15 kg/cm^2 to about 5 kg/cm^2 in a flash vessel;

(b) the recovery from the neutralised process fluid of most of its cyclohexane content in the distillation train of three columns;

(c) the purification of the remaining fluid and the pumping of the resultant cyclohexanone and cyclohexanol to Section 25B for onward processing to caprolactam.

Start up procedures

47 Certain aspects only of start up procedures are immediately relevant.

(a) After a major shutdown the plant would be charged with water, pressurised with nitrogen to 4 kg/cm^2 and tested for leaks. It would then be pressurised to 9 kg/cm^2 and further tested for leaks. When all leaks found had been dealt with, it would be depressurised.

(b) Following testing, the system would be charged with cyclohexane to appropriate levels and warm-up started. At the conclusion of the warm-up process, cyclohexane at normal operating pressure and temperature would be recycling at a relatively low rate through the reactors, distillation columns, cooling scrubber and heat exchanger.

(c) The warm-up procedure consisted of pressurising with nitrogen to about 4 kg/cm^2 with the block valves on the off-gas line closed. This was done because the automatic pressure control valve did not produce complete shut-off, and if the block valves were open there would be a loss of nitrogen to the flare stack.

(d) After pressurisation with nitrogen to 4 kg/cm^2, further pressurisation still with the block valves closed was effected by heating the cyclohexane which was put on a relatively low rate of circulation.

(e) During the heating up, the block valve on the steam by-pass was opened in order to provide a sufficient flow of steam to the heat exchanger as mentioned in Paragraph 34.

(f) As the warm-up proceeded, the temperature and pressure both increased; as they approached the desired values, the steam by-pass valve was partly closed — as noted in Paragraph 34 — and the automatic valve (in parallel with the by-pass valve) gave automatic control of the temperature and, therefore, fixed the pressure in the system.

(g) When both pressure and temperature were right, the block valves on the off-gas line were opened and the system was put on automatic pressure control. Air was then admitted to the reactors, one by one, to initiate the oxidation process.

48 What we have stated in Paragraph (f) above represents an ideal situation, with the system being brought up to correct temperature and pressure without ever exceeding operating pressure or any necessity for venting the off-gases to reduce any excess pressure. It should be appreciated, however, that once the block valves were closed, pressure would continue to rise as the temperature increased, and that an excess over working pressure would inevitably occur if either:

(a) the nitrogen initially in the system was in excess, so that even the correct operating temperature would lead to over pressure, or

(b) the temperature rose above the operating value (155°C), giving over pressure even with the correct nitrogen content, or

(c) nitrogen leaked into the system.

49 With the plant as designed there was no danger involved in this, albeit the pressure control valve was isolated by the block valves, because the pressure could not exceed the safety valve setting of 11 kg/cm^2.

50 This being so, at least one process control technician adopted the practice during start-up of allowing the pressure to build up beyond working pressure, to about 9·1 or 9·2 kg/cm^2. He then caused the block valves to be opened after which he vented the excess pressure by the manual operation of the control valve until the pressure was reduced below the desired level. The block valves were again closed. Subsequent increases in pressure were dealt with by reventing if necessary in the same manner until both correct working temperature and pressure were obtained. He spoke of this operation being repeated a number of occasions during a single warm-up process. Others found it either unnecessary to vent, or vented earlier so as never to exceed operating pressure by more than about 0·1 kg/cm^2.

51 We desire to repeat that there is no danger in this with the plant as originally designed. Even if the decision to vent was made when the pressure had already risen to 9·2 kg/cm^2, or even above that, it would have been unlikely to have risen seriously during the time it would take for an operator to proceed from the control room to the block valves to open them to enable venting to take place. In any event the safety valve would have released any serious overpressure.

Events from 28th March to 1st April

52 This short period is of crucial importance for what happened within it vitally affected the integrity of the plant from the safety point of view.

53 In the evening of 27th March, cyclohexane was discovered to be leaking from Reactor No. 5 (R2525) which like the other reactors, was constructed of $\frac{1}{2}$ inch mild steel plate with $\frac{1}{8}$ inch stainless steel bonded to it. On investigation a vertical crack was found in the mild steel outer layer of the reactor shell (Plate 7) and cyclohexane was emerging over a small part of this crack thus indicating that the inner stainless steel layer was also defective. The Chief Superintendent on duty telephoned the Plant Manager for Area 2 and it was agreed between them that the plant should be shut down, depressurised and cooled when a full inspection might take place.

54 The following morning, the 28th March, the Plant Manager inspected the crack, and found that it extended for some 6 feet. This was clearly a serious state of affairs and the same morning a meeting was held to decide what should be done. It was attended by Mr Beckers the General Works Manager,

Mr Bell the Area 1 Plant Manager, Mr Boynton the Deputy Works Engineer and Services Engineer (for part of the meeting), Mr Cliff the Area 2 Plant Manager, Mr Blackman the Engineer for Areas 1 and 2, and Mr A H T Halderit a DSM commissioning engineer; all of those present gave evidence before us.

55 At this meeting, it was decided that No. 5 Reactor should be removed for inspection, that it would be possible for the oxidation to be carried on with the remaining five reactors, that a by-pass assembly should be constructed to connect No. 4 to No. 6 Reactor and that, when fitted, the plant should be restarted.

56 We were unable on the evidence to ascertain from whom the suggestion to by-pass Reactor No. 5 had emanated but this is not of great importance. What was made abundantly clear to us, however, was that:

(i) no-one at the meeting, save Mr Blackman, was seriously concerned about the wisdom of restarting without both (a) ascertaining the cause of the crack to Reactor No. 5 and (b) stripping and inspecting the other five reactors to ascertain whether any of them exhibited similar faults, albeit not yet sufficiently developed to cause actual leakage;

(ii) no-one appears to have appreciated that the connection of No. 4 Reactor to No. 6 Reactor involved any major technical problems or was anything other than a routine plumbing job, and the possible design problems and design alternatives were not discussed. Even the fact that the inlet and outlet of the by-pass pipe were at different levels was not appreciated at the meeting;

(iii) the emphasis at the meeting was directed to getting the oxidation process on stream again with the minimum possible delay.

57 We entirely absolve all persons from any suggestion that their desire to resume production caused them knowingly to embark on a hazardous course in disregard of the safety of those operating the Works. We have no doubt, however, that it was this desire which led them to overlook the fact that it was potentially hazardous to resume production without examining the remaining reactors and ascertaining the cause of the failure of the fifth reactor. We have equally no doubt that the failure to appreciate that the connection of Reactor No. 4 to Reactor No. 6 involved engineering problems was largely due to the same desire. In Mr Blackman's case we consider that two further factors operated to make him overlook the difficulty of connecting Reactors 4 and 6. Firstly there was his grave concern over the cause of the failure of Reactor No. 5; secondly there was the novel and difficult problem of removing Reactor No. 5. These were his main preoccupations both at the meeting and thereafter. In our judgment it was these preoccupations which led him to fail to take appropriate action in connection with the construction, testing, fitting and supporting of the by-pass assembly.

58 Had there been present at the time a properly qualified works engineer with sufficient status and authority to impose his views, he would, we consider, have insisted that there should be no start-up until the other reactors had been thoroughly inspected and the cause of the failure of Reactor No. 5 ascertained. Mr Hughes told us that, had he been asked, he would have taken this course.

59 In the event the failure to wait until such steps had been taken did not directly cause or contribute to the disaster. Indirectly, however, we consider that it did. Had it been decided to strip and examine the other reactors and await a report on the cause of the failure of Reactor No. 5, the plant must have remained shut down for several days. The design and construction of the by-pass assembly would not then have been conducted as a rush job as in fact it was. There would have been proper time to consider what problems were involved and how they should be dealt with. Given such time we believe, although we cannot be certain, that some at least of the problems would have been recognised and steps taken which would have prevented the disaster.

60 After the decisions at the meeting had been taken, steps were put in hand to implement them. We do not propose to examine these steps in detail. To do so would be useful and necessary only for the purpose of attributing blame as between individuals. This is not our task. We shall merely state the facts that are of relevance to the disaster.

61 Although the openings to be connected were 28 inch openings, the largest pipe which could be found on site and which might be suitable for the by-pass was 20 inch diameter. Calculations were made to determine whether this pipe was large enough to take the required flow of liquid. It was also checked that the pipe was capable of withstanding the working pressure as a simple straight pipe. No calculations were made for a dog-leg pipe as the exact shape of the pipe was not appreciated at this time.

62 It having been ascertained that the pipe as a pipe would both take the flow and would stand the internal pressures involved, the assembly, shown diagrammatically in Fig. 4, was devised. The existing 28 inch pipes with inset bellows — which had previously connected Reactors 4 to 5 and 5 to 6 — were instead connected by a dog-leg pipe which thus replaced Reactor 5. This dog-leg pipe consisted of three lengths of 20 inch pipe welded together, with flanges at each end bolted to the existing flanges on the stub pipes. The new flanges thus formed bridging pieces between the existing 28 inch pipes and the new 20 inch pipe. No-one appreciated that the pressurised assembly would be subject to a turning moment imposing lateral (shear) forces on the bellows for which they are not designed. Nor did anyone appreciate that the hydraulic thrust on the bellows (some 38 tonnes at working pressure) would tend to make the pipe buckle at the mitre joints. No calculations were done to ascertain whether the bellows or pipe would withstand these strains; no reference was made to the relevant British Standard or any other accepted standard; no reference was made to the designer's guide issued by the manufacturers of the bellows; no drawing of the pipe was made, other than in chalk on the workshop floor; no pressure testing either of the pipe or the complete assembly was made before it was fitted.

63 As a result, the assembly as constructed was of completely unknown strength and failed to comply with the British Standard or the designer's guide in the following respects:

(a) The relevant British Standard is BS3351:1971, and with regard to expansion joints the relevant paragraphs are:

"4.6.2. For axial bellows, the piping shall be guided to maintain axiality of the bellows and anchored at adjacent changes in direction to prevent the bellows being subjected to axial load due to fluid pressure."

"5.4.2.1. . . . When expansion joints are used, the advice of the manufacturer should be sought with regard to the guiding, anchoring and support of the adjacent pipework."

(b) The bellows manufacturer was Teddington Bellows Limited of Pontardulais, Glamorgan. In their "Designer's Guide" it is made very clear that two bellows must not be used out of line in the same pipe without adequate support for the pipe: the instructions on this point are clear and explicit, and there are helpful diagrams. It is plain that if the engineers at Nypro had read the "Designer's Guide" they would have realised that their pipe and bellows assembly was unsafe.

64 We find that the only good thing that can be said of the assembly was that its construction was properly executed in that the dimensions were accurate and the welding and fabrication of the flanges was properly carried out. It was completed by the evening of the 29th March.

65 Whilst work on the "design" and fabrication of the 20 inch bridging pipe was being carried out, work on the removal of Reactor No. 5 was also proceeding. For this purpose an access scaffold was built, and support cradles were made for the purpose of supporting the stub pipes with inset bellows while they were detached and lifted out, the need to support the bellows during the installation of the by-pass pipe being plainly appreciated by all concerned.

66 The access scaffold was constructed, save for a few short lengths which were of steel, of aluminium standards (verticals) and ledgers (horizontals). Reactor No. 5 was lifted out at about 2 am on the 30th March. When it had been removed a further access scaffold was constructed within the original scaffold, in the space left by the removal of Reactor No. 5. The additional scaffold, which provided a platform for the fitting of the by-pass assembly, was constructed entirely of steel, not because it was desired to make it stronger but simply because the aluminium stock had all been used.

67 The second access scaffold having been completed, the stub pipes with inset bellows were lifted into position in turn in the cradle which had been used for their removal. They were bolted up and, before the cradle was removed, were supported by steel scaffold poles immediately on the reactor side of the flanges. These poles were about 7 feet long, the span between connections being about 6 feet 6 inches and they were connected to the aluminium standards of the original access scaffold.

68 When the bellows had been bolted up, the dog-leg pipe was lifted into position and it too was bolted up. During this operation the supports for the bellows were moved so that they were directly under the flanges (Fig. 4). This was necessary in order that the lower bolts could be tightened up. Finally a steel scaffold pole like those under the flanges, and of the same length, was used under each horizontal length of the dog-leg in order to support it. That under the lower leg was connected to steel standards of the second access scaffold. That under the upper leg was connected either to other steel standards or to a handrail which was itself connected to steel standards. It is not possible on the evidence to determine with certainty which was the case but we consider the former to be more probable.

The approximate position of the four poles supporting the assembly is shown in Fig. 4. No poles or other means were used either to hold the pipe down or to prevent lateral movement. The four poles that were used (as shown on Fig. 4) were intended merely to afford support during assembly to ensure that the bellows were not strained under the weight of the assembly itself. For this purpose they were probably adequate. They were wholly inadequate as supports for the assembly during working conditions. This is not surprising for no thought appears to have been given to the question of desirability of support under such conditions, save by Mr Blackman who at the "design" stage provided Mr Culpin (his assistant) with a sketch for supports bolted to the pipe flanges and firmly attached to the reactor plinth. This support was not, however, provided and Mr Blackman did not take any steps to insist upon its installation.

69 When the assembly had been fitted the plant was pressurised with nitrogen to about 4 kg/cm^2 and tested for leaks. A leak was found but not marked and, after depressurisation, the plant again had to be pressurised to about 4 kg/cm^2 so that the leak might be marked. This having been done the plant was again depressurised and the pipe was removed, re-welded and re-fitted. During these operations some and perhaps all of the scaffold poles supporting the assembly were removed, but we are satisfied that everyone concerned was aware of the need to support the bellows and that by one means or another adequate support was afforded throughout. On completion of re-fitting the supports were replaced as they had been on completion of the original fitting.

70 The pipe having been re-fitted, the plant was again pressurised with nitrogen to about 4 kg/cm^2 to test for leaks; no leaks having been found, the pipe was pressurised to 9 kg/cm^2 for further tests. No leaks were found. The plant was then de-pressurised, start-up procedures were carried out and the section then again brought on stream. This stage was reached by 4 am on the 1st April.

71 There was no overall control or planning of the design, construction, testing or fitting of the assembly nor was any check made that the operations had been properly carried out.

72 At this stage the position was as follows:

(a) An assembly was installed which had been the subject of no design calculations, which did not comply either with the British Standard or with the bellows manufacturer's recommendations; which was subject to a turning moment under pressure; which was wholly unrestrained in an upward direction and wholly inadequately restrained in a downward direction. As a result the bellows were subjected to shear forces for which they were not designed and the 20 inch pipe was under high and unknown stresses resulting from the end loads of 38 tonnes.

(b) This assembly, although pneumatically tested to 9 kg/cm^2 had not been tested up to the safety valve pressure of 11 kg/cm^2.

(c) Any one or more or all of the reactors still in use, while not yet leaking or leaking sufficiently for the leak to be detected, might already have been suffering from extensive cracking in the mild steel outer shell. Such cracking could have propagated and possibly caused a disastrous rupture of the vessel. Certain of those in Nypro who were concerned believed that the crack in Reactor No. 5 was a wholly unique incident in some way connected with the fact that at an earlier stage it had been sprayed with cooling water for a considerable period. This belief was confirmed a few days later as a result of metallurgical tests carried out by DSM but at the time when the plant was brought on stream the position was wholly uncertain and even up to the time of the disaster no-one was sure that one or more of the other reactors had not been affected.

73 The position with regard to pressure testing calls for comment in the light of the procedures laid down in BS 3351, paragraph 7.4.

(a) Para 7.4.1: "The testing shall be by water . . . unless otherwise agreed." As far as we are aware, hydraulic testing of the 20 inch pipe and bellows assembly was never considered.

(b) Para 7.4.2: "If hydrostatic testing is not considered practicable . . . a pneumatic test may be substituted. . . . The dangers associated with pneumatic testing . . . should be recognised and suitable precautions taken." As far as we know these dangers were not appreciated, nor were any precautions taken.

(c) Para 7.4.6.1: "Piping tested hydrostatically shall be tested to a pressure of not less than 1·3 times the design pressure adjusted to 50°C, but in no case less than 7 bar." This kind of test does not seem to have been considered. If the assembly had been tested according to the British Standard, a pressure of over 11 kg/cm^2, ie above safety valve pressure, would have been used: such a test would almost certainly have caused failure of the pipe and bellows assembly and the disaster would have been averted. The tests that were carried out were not for testing the strength of the assembly, but were leak tests.

Events from 1st April to 29th May

74 After the Section was again on stream the assembly was lagged. Between the time when it came on stream and the time when the plant was shut down on the 29th May as set out below, the assembly gave no trouble. It was never closely inspected but was casually looked at on frequent occasions by a number of witnesses. One of these witnesses observed that under pressure the pipe seemed to lift slightly off the support pipes but no-one noticed anything amiss. It must therefore be taken that, albeit there may have been some displacement of the assembly during this period, it cannot have been great enough to attract attention.

75 During the above period the only events which call for mention are that the plant, albeit twice shut down for short periods, was never depressurised or cooled and that an investigation into an unexplained usage of high pressure nitrogen was being conducted. This investigation was still uncompleted at the time of the disaster.

Events from 29th May to explosion

76 On Wednesday 29th May a leak was discovered in the bottom isolating valve on a sight glass on one of the vessels and it was decided to shut the plant down. During the course of the day this was done. The plant was depressurised to 1·5–2 kg/cm^2 and cooled.

77 During the 30th and 31st May the leak was repaired and certain other work done. Nothing calls for mention until the early hours of Saturday morning when the start-up process was begun. Before this all safety checks had been completed, the plant had been pressurised to 4 kg/cm^2 with high pressure nitrogen and tested for leaks and all leaks had been repaired.

78 At about 4 am circulation of cyclohexane was begun and a small amount of steam put on the reboiler to C2544 (Fig. 2). Leaks were soon found and circulation was stopped and heat taken off. The leaks were subsequently found to have cured themselves and at about 5 am circulation was again started and steam put on. Pressure rose much more quickly than normal and had risen to 8·5 kg/cm^2 when Reactor No. 1 had reached only 110°C with the other reactors progressively cooler. This pressure was reached within about 1 hour and had the heating up process continued it is clear that substantial venting would have been necessary before the warm-up had been completed if the pressure was to be at 8·8 kg/cm^2 at the conclusion of warm-up.

79 This did not in fact happen because when the pressure had reached 8·5 kg/cm^2 a leak was found and circulation was stopped and heat shut off. The temperature and thus the pressure were allowed to drop and by the end of the shift (7 am on Saturday) the pressure was down to approximately 4·5 kg/cm^2. The particular leak and other leaks found were not dealt with on that shift owing to the necessary spark-proof tools being in a locked shed. Nothing untoward arose from this but it is clearly not a satisfactory situation. A delay in obtaining the required tools could lead to a minor and harmless situation developing into a major and hazardous one.

80 We now come to the last shift in respect of which we were able to take evidence from the Control Room staff namely the 7 am to 3 pm shift on 1st June 1974.

The evidence with regard to the state of the plant at the commencement of this shift was unsatisfactory. Both the outgoing and incoming shift superintendents stated that pressure was in the region of 4–4·5 kg/cm^2 but the incoming superintendent gave evidence of reactor temperature higher than those which, in the previous shift, had led to a pressure of 8·5 kg/cm^2. Other witnesses considered that there was no or very little pressure in the system. We consider that the pressure was as stated by both superintendents but that the incoming superintendent was mistaken in his recollections as to temperature. Determination of the pressure at the commencement of the shift is, however, unimportant. The amount of nitrogen in the system in the previous shift had been sufficient to produce a pressure of 8·5 kg/cm^2 at low reactor temperatures (see paragraph 78 above). There had been no venting in that shift and no material leakage. Hence if a pressure no higher than 8·8 kg/cm^2 was to be achieved at an operating temperature of the order of 155°C, substantial venting was certain to be required when warm-up was resumed and this is confirmed by the fact that venting did in fact take place on one occasion during this shift. The circumstances surrounding this event are of importance.

81 Warm-up began at about 9.30 am. The block-valves on the off-gas line were closed, the temperature controller was set at 160°C and the block-valve on the steam by-pass line was opened sufficiently to ensure that there was a reasonable rate of increase in temperature. Shortly after warm-up began it was found that there was insufficient high pressure nitrogen to start oxidation and that further supplies would not arrive until midnight.

At about 11.30 am to 12.00 noon the shift superintendent was out on the plant and the Process Control Technician (PCT) noticed that pressure had risen to a point where corrective action was necessary. He first attempted to deal with the situation by reducing the steam supply but finding this to be ineffective he caused the operators to go out on to the plant and open the block-valves. He then commenced to vent. At that time the pressure had reached 9·1 or 9·2 kg/cm^2 or 0·3 or 0·4 kg/cm^2 above the pressure of 8·8 kg/cm^2 which was the pressure wanted on this day. He was in no way alarmed and there is no reason why he should have been. The relief valve was set at 11 kg/cm^2 and Mr Halderit, who knew as much about such plants as anyone, stated that he would not have been alarmed to see a pressure of 9·5 kg/cm^2. He would want to know why and he would take corrective action but that is all.

82 At the conclusion of venting, by which time the pressure was below the aimed at final pressure of 8·8 kg/cm^2, the block-valves were again closed. The position with regard to steam supply, both after venting and in the remainder of the shift, is a matter on which the evidence was somewhat confused. There is no doubt that it was intended to provide sufficient steam to continue warm-up because operating temperature had not been reached. If the intention was successful then, as warm-up continued, the pressure would once again rise. Whether it would again exceed 8·8 kg/cm^2 would be dependent upon the question whether the original venting had been so precisely judged that the amount of nitrogen remaining in the system was no more than sufficient to result in such a pressure at a time when the system had reached and settled down at the operating temperature of 155°C. We are satisfied that the venting was not and was not intended to be so precisely judged. The PCT gave clear and convincing evidence, which we accept, that he expected that one or more further ventings would be necessary before that point was reached. This is indeed what one might expect. The desire on everyone's part was to conserve nitrogen which was known to be in short supply.

The tendency, therefore, would be to undervent rather than to risk the wastage of existing nitrogen and the need to use more if the venting was excessive, ie left too little nitrogen in the system to achieve operating pressure at operating temperature.

83 We consider, however, that the most probable sequence of events is that the reduction in the steam supply prior to venting was, at the conclusion of venting, such as to provide too little steam to continue warm-up and that later in the shift the block-valve on the steam by-pass was opened sufficiently to produce the continuation of warm-up. At what time this occurred we were unable to ascertain.

84 No further venting took place during the shift and we are satisfied, despite some evidence to the contrary, that at the end of this shift the system had not reached and settled down at operating temperature. Temperature and pressure were still rising slowly, the block-valve on the steam by-pass was still slightly open and one or more further ventings were likely to be required.

85 What happened during the final shift can never be known with certainty, for the explosion not only killed all in the Control Room but also destroyed all the relevant instrumentation and records. After the disaster, however, one of the block-valves on the off gas line was found to be closed as also was the block-valve on the steam by-pass.

86 As one or more further ventings were, as we have said, likely to be required during the shift, it follows that it is also likely that pressure would again rise to approximately the same level as it had done in the previous shift. Furthermore, since the previous venting had taken place before warm-up had been completed, and the plant settled, it also follows that if and when the same pressure was again reached it would be reached at a higher temperature than had prevailed at the like pressure in the morning. How much higher it is impossible to state but it is important to bear in mind that during warm-up the temperature in Reactor No. 1 would, until the plant had settled, be higher than in No. 2 and so on down the line. At the time of the steep pressure rise in the early hours of the morning, for example, Reactor No. 1 was at 110°C, whilst No. 6 had still reached only 50°C.

If therefore the morning venting had taken place when Reactor No. 1 had been within a few degrees of operating temperature Reactors 4 and 6 would still have been much below such temperature.

87 From the foregoing it can be seen that before a subsequent venting was required Reactors 4 and 6 could well have reached considerably higher temperatures (about 155°C) than that at which they had been in the morning.

Such increases in temperature would involve no possible lack of competence, care or attention to instructions on the part of any of the Control Room staff. Their objective was to bring the plant up to operating temperature. It had not got to that point either at the venting during or at the end of the previous shift. It was therefore their plain duty to continue warm-up until the objective was reached.

An increase in pressure to the same, or even a slightly higher pressure than that achieved in the morning would also involve no possible lack of competence, care or attention to instructions on the part of any of the Control Room staff. A submission to the contrary was made to us but we reject it.

No-one suggested or in our view could suggest that the PCT who had permitted the pressure in the morning to reach 9·1 or 9·2 kg/cm^2 before taking corrective action had acted otherwise than properly. It is thus impossible to suggest that, if the Shift Superintendent or PCT in the final shift behaved in a similar manner he acted otherwise than equally properly (see also paragraph 81).

88 Before passing from the events of the last few hours before the disaster, we should for completeness mention a number of miscellaneous matters:

(a) The unusually fast rise in pressure in the early hours of 1st June remained unexplained. It was not accompanied by an unusually steep temperature rise or an unusually slow circulation rate. The only remaining possibility appears to us to be an undetected leak of high pressure nitrogen into the system. As we have mentioned above, the plant was using more nitrogen than was expected and the investigation into the reasons for this had not been completed at the time of the disaster.

(b) Although the PCT who carried out the venting in the early hours of the morning did not consider the need to vent as unusual, others took a different view. If it was unusual then before the final shift took over there had been two unusual events, namely such venting and the fast rise in pressure mentioned in (a) above.

(c) A further unusual event was that owing to the shortage of nitrogen the plant was required to circulate at operating temperature and pressure for some hours before oxidation was due to commence.

(d) After the disaster the temperature indicator for Reactor No. 3 was found apparently showing a minimum temperature of 168°C. It could, however, easily have been moved in the explosion and we therefore regard this as of no significance.

(e) Although we have referred to pressures in units of kg/cm^2 the chart of the pressure recorder in the Control Room was marked in units from 1–100, each division on the chart being 2mm and representing 0·3 kg/cm^2. A variation in pressure of 0·1 kg thus represented a movement of 0·67mm. On the right-hand side of the instrument there was a red mark to indicate 8·5 kg/cm^2 and on the left-hand side a movable set point red indicator which was set at the particular pressure desired to be achieved. The actual pressure was recorded on the chart by a red ink pen. On the controller there was stated the conversion factor (0·15) for converting the numerical units to kg/cm^2. A full-size reproduction of the chart appears at Fig. 5.

One PCT made it clear that the method of operation was to set the point needle using this conversion factor and that thereafter what prompted action was the amount by which the trace on the chart was above or below this set point. We have little doubt that this is the reason for a certain imprecision in some of the evidence given before us. It also confirms our view that an operator who did not take action until there had been a variation of 2mm would be acting entirely properly.

(f) During the course of our investigations the possibility of a sudden rise in pressure during the final shift due to some internal incident was considered. We were able to exclude all of the possible internal incidents suggested. Of them we would mention only two:

(i) the possibility of the rapid decomposition of an accumulation of peroxides in the system;

(ii) the possibility of the operation of a nitrogen purge having occurred.

As a matter of probabilities we exclude both and we mention them specifically only to show that they have not been overlooked. There was some evidence before us to suggest that either could theoretically have occurred but we were satisfied that both were unlikely.

The explosion and aftermath

89 At about 4.53 pm the massive explosion occurred, the broad effects of which we set out in Paragraph 1. We have already stated (paragraph 6) that it resulted from the escape of cyclohexane from Section 25A.

The force of the explosion was estimated (from ionospheric readings) by Dr T B Jones of Leicester University, by the Atomic Weapons Research Establishment by way of comparison with another blast, by Dr K Gugan of J H Burgoyne and Partners, from the barograph chart of a glider in the vicinity, and by Dr A F Roberts of SMRE from a blast damage survey. From their evidence and other evidence called before us we estimate that the explosion was of the equivalent force to that of some 15–45 tons TNT. We also concluded that the explosion was a complex one with an ill-defined epicentre and think there may in fact have been a secondary explosion so close in time that it could not be separately identified. The main but ill-defined epicentre was situated above ground level and to the NE of Section 25A.

90 Following the explosion there were fierce fires in many parts of the plant. We do not find it necessary to go into any detail with regard to them at this stage. We would, however, like to pay a tribute to the work carried out by the various organisations involved and to the gallantry of certain individuals. We refrain from any attempt to mention anyone in particular since we heard only such evidence as was necessary for our purposes and if we were to make special mention of one we might unjustifiably imply that there were no others equally deserving of praise. We would also mention that contrary to what has occurred on some occasions the work of fire fighting and rescue was not hampered by morbid sightseers.

Source of Main Explosion

91 Since, as we have pointed out (paragraph 6(b)) the source of the escape was established to be in the general area of Section 25A, initial investigation of the site was concentrated there. At a very early stage two matters of significance were discovered:

First, the 20 inch by-pass assembly with the bellows at both ends torn asunder was found jack-knifed on the plinth beneath, thus leaving two 28 inch openings in Reactors 4 and 6 (Plates 8, 9, 10).

Secondly, a 50 inch split was found in the 8 inch stainless steel piping joining Separators S2538 and S2539 (Plate 14).

92 Subsequent investigations established that there were no other openings on Section 25A (save those caused by the main explosion) and confirmed that there was no other possible source of escape elsewhere in the plant.

93 Prior to the main explosion eye-witnesses had observed or heard unusual events in the area of Section 25A but nothing was heard or seen more than, at maximum, two minutes before the explosion. It was established that within such a period the discharge from the 28 inch openings in Reactors 4 and 6 would have been sufficient, unaided by an escape from any other source, to have formed a vapour cloud of sufficient size to have caused the main explosion and that vapour from these emissions could have reached the hydrogen plant, which it appeared from some of the eye-witness evidence was a possible source of ignition. It was further established that discharge from the 50 inch split in the 8 inch line might have been capable of producing, within the available time, a vapour cloud of sufficient size to cause an explosion (or deflagration with pressure effects). It was, however, certainly not capable of producing a cloud of sufficient volume to cause an explosion of the size of the main explosion or anywhere near that size.

94 It is therefore clear that the immediate cause of the main explosion was the rupture of the 20 inch by-pass assembly. Concerning this there was no dispute between any of the parties or witnesses represented or called before us.

95 Since the by-pass assembly was known to have had many unsatisfactory features the conclusion that it had failed solely due to such unsatisfactory features might seem a simple one and was in fact reached at an early stage by at least one investigator. Such a conclusion could not, however, be reached without much further investigation. In the first place, the by-pass assembly had survived two months of normal working of the plant without apparent trouble. Secondly, there was no evidence of anything abnormal having occurred at or about the time of the failure. Thirdly, even if it had failed without any external agency, there was the possibility of some internal incident in the plant which, whilst it had affected only the weakest link, could, had there been no weak link, have been sufficiently severe to have caused a rupture elsewhere. Fourthly, there was the 50 inch split which could have preceded the failure of the by-pass. Finally, there were found additional cracks in the 8 inch line and in other lines, the cause of which was unknown and which, even if they had nothing whatever to do with the explosion, could well have affected the desirability of rebuilding the plant.

The problem stated

96 In the light of the foregoing the essential problem before us was to determine what had caused the rupture of the by-pass. Put in summary form the possibilities which we investigated were:

(i) rupture of the by-pass assembly through internal pressure;

(ii) rupture of the assembly in two stages, a small tear in the bellows from over-pressure leading to an escape and minor explosion causing final rupture;

(iii) rupture of the 8 inch line at the 50 inch split leading also to a minor explosion causing rupture of the by-pass assembly.

97 All three of the above remained as live possibilities until a late stage in the inquiry but there was in the end no evidence to support a two-stage rupture of the by-pass assembly. As it may be dealt with shortly we will do so at once.

Two-stage rupture of the by-pass

98 The possibility of a two-stage failure of the by-pass arose because a small tear was found in one of the bellows fragments after the disaster; because it was at least possible that some of the eye-witness evidence pointed to an explosion bringing down the by-pass and because the Leicester University ionospheric observations indicated that there had been a smaller explosion approximately 48 seconds before the main explosion.

99 The possibility of a small tear being produced by internal pressure was investigated experimentally in bellows tests at Nottingham (see Appendix I). Very small tears were in fact produced but not until a pressure of 14·5 kg/cm^2 had been reached.

There was no evidence that pressure on the plant had ever approached even safety valve pressure (11 kg/cm^2).

100 We have (see paragraph 88**(f)** above) already rejected excess pressures due to internal incident and without some serious and sudden internal incident it is plain that pressure sufficient to cause a tear could only have been reached if the safety valves had failed to operate and the Control Room staff had been grossly negligent. We stated in the course of our hearings that we would only attribute blame to the Control Room staff if driven so to do by the most cogent evidence. Not only was there no cogent evidence there was no evidence at all of any negligence of any sort. We therefore reject the two-stage rupture possibility.

Eye-witness, film and photographic evidence

101 Before we proceed separately with an examination of the relative probabilities of, on the one hand, a simple rupture of the 20 inch assembly from internal pressure and, on the other hand, its collapse due to external explosion resulting from an escape from the 50 inch split, it is necessary to deal with the evidence of eye-witnesses and contemporary films and photographs. This is because it was forcibly contended before us, principally by Dr J I Cox of L H Manderstam & Partners (UK) Ltd, who were advising Nypro, that this evidence established, or at least was most consistent with, the collapse of the by-pass due to external explosion arising from prior rupture of the 8 inch line.

102 If this contention were sound it would or might have an important bearing on our ultimate conclusion. Clear evidence from the wreckage and other sources could not be over-ridden but if other evidence left the relative probabilities of the alternative hypotheses evenly balanced then eye-witness evidence and photographic evidence could tip the scale one way or other.

103 In evaluating evidence of this nature it is in our view of importance to consider the main sequence of events which would have been available to have been seen or heard on each of the two hypotheses. For convenience we shall hereafter refer to the alternatives as the "20-inch hypothesis" and the "8-inch hypothesis".

104 On the 8-inch hypothesis the sequence would have been as follows:

(i) The sound of the violent rupture of the 8 inch line.

(ii) The sound of the initial escape.

(iii) The sound of the continuation of the escape coupled possibly with a rumble due to the boiling of the liquid in Separator S2538 due to release of pressure.

(iv) The sight of a vapour cloud forming above the by-pass assembly, vapour emissions in other directions, and, possibly, flames from local fires.

(v) The sight of ignition.

(vi) The sight and sound of the explosion or deflagration which caused the collapse of the by-pass.

105 Thereafter the sequence would have been largely the same for both hypotheses:

(vii) The mechanical noise of the rupture of the assembly and the destruction of parts of the scaffold coupled with the sight and sound of scaffold poles and debris being flung out of the gap between Reactors 4 and 6.

(viii) The noise of the initial escape from the 28 inch openings.

(ix) The noise of the continued escape and rumbling from the liquid in the reactors boiling.

(x) The sight of the vapour cloud forming and emissions in various directions.

(xi) Possibly the sight of flames from local fires.

(xii) The sight of ignition of the vapour cloud.

(xiii) The sight and sound of the main explosion.

106 From the foregoing it will be seen that on the 8-inch hypothesis the complete sequence involves, so far as sound is concerned and when simplified the following:

Bang ——— *escape and rumble* ———
bigger bang ——— *bigger escape and rumble* ——— *final bang*

107 Dr Cox conducted a painstaking analysis of the eye-witness evidence (38 were called before us) and caused re-enactments to be carried out by a number of witnesses. He also visited the viewing positions of some witnesses to see if they could see what they recollected having seen and he prepared with great care a plan showing the angle of view of the numerous witnesses both on and off the site. By the second day of our hearings he was reaching the conclusion that the eye-witnesses established that there had been an explosion bringing down the by-pass and thus that the 8-inch hypothesis (or two-stage rupture of the by-pass) had occurred. As time went on he became more and more convinced that this was so.

108 We are indebted to Dr Cox for the work he has done. This has aided our task of considering the eye-witness evidence. The angle of view plan in particular we have found most helpful. We differ, however, from his conclusions. We do not propose to lengthen this report by dealing with all the many points raised by Dr Cox to support his contentions. To do so would serve no useful purpose. We shall, however, deal with two matters on which at one time or another he placed special reliance.

Evidence of the laboratory witnesses

109 These witnesses numbered eight. It was contended by Dr Cox that what they heard and saw was the noise of or consequent upon the rupture of the 8 inch line and the emissions from it, not the collapse of the by-pass, whatever its cause, or events associated with such collapse.

We have no hesitation in rejecting this contention. In the first place, if this were the case it is remarkable that not one of them gave any reliable evidence of having heard or been otherwise aware of the subsequent failure of the by-pass, which, on this view, would not be merely the noise of the mechanical failure but the noise of the explosion well up on Section 25A which brought it down. It would also have been very much louder than the noise of the 8 inch rupture which it is said they had heard. This remarkable feature was sought to be explained principally on the basis that the alarm felt by the witnesses would have obscured such noise. Having seen and heard the laboratory witnesses give evidence and considered what they did remember seeing and hearing we are unable to accept this explanation.

We are satisfied that what they did see and hear was the rupture of the by-pass assembly and its immediate results. One of the laboratory witnesses indeed gave evidence of seeing debris and scaffold poles flying out of the gap between Reactors 4 and 6. This could only be consistent with such a view. A number of reasons were put forward for rejecting this piece of evidence. We accept that, since no other laboratory witnesses saw anything similar, the evidence required to be carefully considered before being accepted and we also accept that there were certain points of detail which suggested that it might be unreliable. We consider, however, that it was truthful and accurate and we accept it.

110 The foregoing does not mean that the 8 inch line did not fail first. It merely means that the laboratory witnesses afford no evidence that it did.

Witnesses said to have seen something before the first event whatever that event may have been

111 In his final written report Dr Cox observed "taken together Ayre & Hotchin present a strong case that something was happening before the first reported noise". Mr J E Hotchin was one of the laboratory witnesses. In his evidence-in-chief he did say in answer to a question "The first thing I noticed or remember noticing was the escape of vapour". However, in answer to the very next question, he said that he did not know if he had heard anything prior to the vapour escaping. Moreover in a signed statement made to HMFI on the 4th June and put to him in cross-examination he said:

"I cannot remember the exact sequence of events but the following is my recollection of what happened. There was a rumbling sound — I cannot remember whether this occurred when I was in the lab or after I got out. It sounded to me like a superheated liquid boiling on a very large scale. I think it probably came from Section 25. Something drew my attention to the window — I looked out and saw vapour escaping — this definitely came from Section 25."

In the light of this we cannot regard the single answer which we first quoted as any reliable evidence that Mr Hotchin saw something before he heard something.

112 Mr E Ayre was watching television some 400 yards away when, at a time which he put as three, four or five minutes before the main explosion, he saw a wisp of steam. Later he observed the main escape. We can attribute no importance to this. Dr Cox agreed that to see a wisp of steam over a chemical plant would be an everyday occurrence.

113 We have dealt with these witnesses specifically, since we consider they are indicative of an approach to the evidence which is wholly unsound and has been largely responsible for an entirely mistaken conclusion as the effect of the whole body of eye-witness evidence.

Conclusions on eye-witness evidence

114 The eye-witness evidence was in our view of great value in directing attention to the general area in which to search for the cause of the explosion. It also afforded evidence that the source of ignition for the main explosion might have been the hot surfaces below the burners at the hydrogen plant and subsequent experiments carried out by Dr P H Kemp at University College, London, indicated that the vapour cloud from the escape from the 28 inch nozzles could have reached the hydrogen plant. The eye-witness evidence does not in our view go further. It does not indicate, much less establish, an escape prior to the 20 inch by-pass rupture. On the contrary, if it indicates anything it indicates that there was no such prior rupture and escape. This indication is negative rather than positive and lies in the fact that there was no witness who saw or heard both the sights and sounds of the prior events and the rupture of the 20 inch by-pass itself. Had there been any such prior events, we think it probable that one or more of the witnesses, who undoubtedly heard the rupture of the by-pass or saw its immediate effects would also have seen or heard such prior events. No witness gave any reliable evidence to this effect.

115 We would, however, feel it wrong to place any great reliance on such an indication. Witnesses of quite small accidents, let alone major ones, are well known to be uncertain not only about timings but also about the order in which events occurred and the precise nature of the events themselves. In addition, the variety of words used to describe the same sight or sound makes it quite impossible to attribute any significance to the particular word used.

The general picture presented is a clear one and is of a main explosion preceded by about half a minute by some noisy event which was followed immediately by rumbling.

Films and photographs

116 We examined a number of films and photographs taken shortly after the disaster but we need not comment on any of them save on the 8mm amateur film taken by Mr M B Goodchild within a minute or two after the main explosion. This was much relied on as supporting the 8-inch hypothesis. We viewed it more than once both in its original 8mm form and in a 16mm version prepared by the BBC and we inspected it frame by frame in a viewer also on a number of occasions.

The film shows two columns of flame the left-hand one of which was said to be a near vertical turbulent jet of flame coming from the rupture in the 8 inch line. If this was right it would strongly support the 8-inch hypothesis for there would not have been time for the 50 inch split to have developed between the main explosion and the taking of this film.

117 The column of flame relied on was measured as being some 250 feet high and this was the height of flame which was calculated by Dr Cox as being obtainable from an upward facing 8 inch nozzle. We were unable to understand and were never given any satisfactory explanation of the proposition made by Dr Cox that a similar flame height would be produced from a discharge from the downward

facing 8 inch line when discharging into the irregularly shaped 50 inch split, one part of which was so orientated as to give rise to a horizontal throw to the north. There would no doubt be some upward throw but it would be relatively small.

It was subsequently confirmed by Dr K Gugan, who is a specialist on such matters, that the flame resulting from discharge from the 50 inch split would be largely horizontal, and that any upward element would produce a very much shorter flame than from an upward facing 8 inch nozzle. (This matter is dealt with in further detail in paragraphs 166–172.)

This, in our judgment, robs the film of any significance which it might ever have had. We do not consider that it supports or detracts from either hypothesis. No inference can be drawn from it. We are, however, indebted to its creator and owner for making it available and to the BBC for making available their 16mm version.

118 Before leaving this part of the evidence we consider it necessary to make one observation. We regard the re-enactment by witnesses before they have given evidence as undesirable. We have no doubt that the re-enactments in the present case were carried out with the best of intentions and we have equally no doubt that when they were carried out no attempt was made to influence any witness.

119 Nevertheless, we consider such re-enactments to be dangerous, principally because a witness who has performed a re-enactment is likely to have his recollection of what he saw or heard overlaid by what happened on the re-enactment. Re-enactments may be useful, on occasions, to check a timing given by a witness or what a witness could have seen from a stated viewpoint, after that witness has given evidence. We consider, however, wholesale re-enactments before the witnesses have given their evidence are unsatisfactory and unlikely to assist in the process of arriving at the truth.

The 20-inch hypothesis examined

120 This hypothesis is, in effect, that the 20 inch by-pass ruptured in one stage as a result of internal pressure/temperature conditions which might reasonably be expected to occur in the final shift without any lack of care on the part of any of the Control Room staff.

121 As a first stage in assessing the probability or otherwise of such a rupture having occurred it was necessary to ascertain under what, if any, conditions of temperature and pressure the assembly could be expected to rupture.

122 From an early stage a theory was advanced that rupture had occurred by reason of the pipe having yielded catastrophically at the lower mitre, ie "jack-knifed" and thus torn out the bellows at either end. In order to determine whether rupture in this or any other mode could be expected at any and what conditions of internal temperature and pressure, a series of experiments were carried out and much expert evidence was called before us. These experiments are described in Appendix I. That appendix also contains our comments upon and analysis of the experimental results and expert evidence. Here we set out only our conclusions in simple and condensed form.

123 The experiments and evidence established in our judgment the following:

(i) that rupture initiated by jack-knifing was unlikely to occur before relief valve pressure;

(ii) that at pressures below relief valve pressure and at normal operating temperature the bellows at both ends of the assembly could suffer a gross permanent deformation (which we shall refer to as "squirm") without any rupture of the assembly. The squirm phenomenon is shown clearly in Plates 11 and 12;

(iii) that if this phenomenon (squirm without rupture) occurred, the assembly with both bellows squirmed would not then rupture save at pressures above relief valve pressure;

(iv) that at pressures below relief valve pressure and at normal operating temperature, double squirm of the bellows could be followed immediately by violent jack-knifing of the pipe causing complete rupture;

(v) that the conditions of pressure and temperature which would produce either squirm alone or squirm followed by jack-knifing depended principally on the axial stiffness of the bellows and on the number, position and length between centres of the support scaffold poles.

124 The axial stiffness of the bellows used in the 20 inch by-pass assembly lay between 2,800 and 3,300 lbf/in. Their actual stiffness can never be known. We have concluded that there were four supporting scaffold poles positioned as shown in Fig. 4 and that these poles were supported on standards 6 feet 6 inches apart. For such scaffold conditions we set out below in tabular form:

(a) the pressures at which, at temperatures respectively of 150°C and 160°C, bellows at the extremes of the stiffness range could be expected to squirm;

(b) the pressures at which, at such temperatures, jack-knifing of the pipe (followed by rupture) could be expected to follow squirm.

	Pressure for Squirm		Pressure for jack-knifing (and rupture) to be a consequence of squirming		
	Bellows Axial Stiffness		Probability		
	2800	3300	Low	50%	High
150°C	8·8 kg/cm²	10·3 kg/cm²	9·3 kg/cm²	9·9 kg/cm²	10·6 kg/cm²
160°C	8·6 kg/cm²	10·1 kg/cm²	9·1 kg/cm²	9·7 kg/cm²	10·4 kg/cm²

The figures given in this table are not exact figures and Professor D E Newland of the University of Sheffield who spoke to them stated that he would not be surprised if squirm or squirm followed by jack-knifing occurred at slightly lower pressures.

125 It is known that the assembly had previously survived start-up in April, normal operation for two months during which the temperature was about 155°C and pressure about 8·8 kg/cm², and in the morning of 1st June a pressure of about 9·2 kg/cm² at a temperature well below 155°C. It follows from this that the bellows cannot have been at the lower extremity of the stiffness range.

126 If, in the final shift, the pressure reached 9·2 kg/cm² again at a temperature of about 155°C in Reactors 4 and 6 the assembly would thereby have been subjected to conditions more conducive to squirm alone or squirm and jack-knifing than had ever occurred before. Even had a similar pressure and reactor temperature been reached on start-up in April the conditions would have been less favourable to squirm and jack-knifing because the assembly had not then been lagged and the temperature of the pipe and bellows would therefore have been lower.

127 We have already stated that we regard it as quite possible that a pressure of 9·2 kg/cm² or a little higher at a temperature of about 155°C in Reactors 4 and 6 would have been reached at some time in the afternoon shift (see paragraphs 87–88). It can be seen from the above table that if this occurred the assembly would have been in the range where squirm followed by jack-knifing and thus complete rupture could occur albeit the probability on the basis of Professor Newland's calculations would have been low.

128 We therefore conclude that rupture of the assembly within the temperature/pressure relationship which could reasonably be expected to have occurred in the last shift is a probability albeit one which would readily be displaced if some greater probability to account for the rupture could be found. The only other possibility suggested was the 8-inch hypothesis and we now turn to examine this.

The 8-inch hypothesis

Background

129 It is clear from Paragraphs 124–128 above that at a pressure of 8·8 kg/cm² and 155°C the 20 inch assembly was nearing the point of squirm followed by jack-knifing. Hence a small external explosion causing a downward force could have triggered collapse. We now consider the probability of such an external explosion having occurred. This hypothesis in its simplest form is that a prior rupture of and escape of cyclohexane from the 8 inch line between Separators S2538 and S2539 had resulted in a minor explosion which triggered the collapse of the 20 inch assembly.

130 We shall in later paragraphs deal in detail with the various features of the 8 inch line which fall to be considered in examining the 8-inch hypothesis but it is convenient to give an outline of the basic facts at this stage so that before going into detail we may set out the 8-inch hypothesis as presented.

131 The rupture itself (a 50 inch split) occurred just downstream of elbow G on Figs. 8, 9 and 10 (see also Plate 14). Immediately downstream of the split the line had been supported by a mild steel strap round the pipe and this was attached to a vertical mild steel rod $\frac{5}{8}$ inch in diameter. This rod passed through a $3 \times 3 \times \frac{5}{16}$ inch mild steel angle iron bracket and then through a spring, the top end being secured by a collar. For these features see Fig. 9.

132 After the disaster the strap was found bent to the east and to have dented the surface of the pipe. The vertical rod had suffered a tensile point fracture $3\frac{1}{2}$ inches above its point of attachment to the strap. The upper part of the rod with a 90° bend in it below the angle iron had remained in its hole in

the angle iron and the corner of this angle iron was bent upwards. All three features may be seen in Plates 14 and 15.

133 Downstream of the hanger was a non-return valve (NRV) and block-valve (Fig. 9). Further downstream elbow K on Fig. 10 had, after the disaster, suffered a 40° twist.

134 Five feet above the elbow G there was a 3 inch split in the vertical run of the pipe accompanied by local bulging (Figs. 9 and 10). A further but much smaller crack also showing local bulging was found in the "petal" of the 50 inch split at elbow G which was torn outwards to the east. Numerous other cracks were found in other places on the pipe run between Separators S2538 and S2539.

135 Prior to the disaster the pipe itself was lagged with a 2 inch thickness of rock wool held in place prior to the application of aluminium cladding by galvanised wire. The aluminium cladding was 20 swg. It was held tightly in place by self-tapping screws. The pipe lagging and cladding terminated on either side of the NRV and block-valve with a metal capping. Plate 16 looking down on the NRV from above shows the vertical run of pipe and the elbows during construction with the lagging partially complete.

136 The NRV and block-valve were enclosed in a removable lagging box. We were unable to ascertain precise details of the construction of this lagging box but it possessed the following features.

(i) It was constructed in two halves of 18 swg aluminium and was held together by toggle clips.

(ii) Two inches of rock wool lagging was affixed to all main surfaces.

(iii) It extended far enough upstream and downstream of the NRV and block-valve assembly to fit over the cladding of the upstream and downstream pipes.

(iv) Any vapour collecting in the box could escape:

(a) At the annulus where the box fitted over the upstream pipe cladding.

(b) At the annulus where the box fitted over the downstream pipe cladding.

(c) At the orfice to the east where the spindle for the control wheel of the block-valve emerged.

(d) All round the line of the division between the two halves of the box.

Figures 8 and 9 show details of the lagging box as deduced by us from the evidence.

137 Situated some 12 inches above the top of the NRV and 18 inches to the east there was before the disaster a quartz sensor bulb set to activate the sprinkler system at a temperature of 68°C or above. The sprinkler position is shown on Fig. 8 and the sprinkler can be seen on Plate 16. The quartz bulb was below the sprinkler.

138 Situated high up on the plant to the east of the line of reactors was a cooling system which consisted of stainless steel tubes to which galvanised mild steel fins were attached. Air was blown upwards through these fins by ten large fans each with its own motor. The position of this system can be clearly seen on the photograph of the model (Plate 6), it being the horizontal box-like structure running north to south along the northern half of the section.

139 The 8-inch hypothesis took various forms at various times. It was supported in one or other of its forms by Dr Cox and Dr Gugan whom we have already mentioned and by Professor J G Ball, the last two being retained by Nypro's insurers. They were not represented but were called by us as witnesses. Its genesis was the eye-witness evidence which we have already considered and the discovery after the disaster of two loose bolts on the cage of the NRV. There was dispute as to whether these had been loose at the time of the disaster or had been undone later. We shall consider this below. At present we desire to point out only that the 8-inch hypothesis is built wholly upon the fact, if it be one, that these bolts were loose before the disaster. If they were not, then the hypothesis ceases to exist for, in whatever of its forms, they are its essential foundation.

The steps in the hypothesis

140 The sequence of events postulated by the proponents of the hypothesis is as follows:

(i) Two loose bolts.

(ii) The loose bolts lead either to a slow leak of process fluid lasting anything from 1–18 months or to a partial gasket blow very shortly before the disaster and a comparatively large escape of fluid.

(iii) In the case of the slow leak there is an accumulation of tarry residues in the lagging inside the box leading to self heating to a sufficient temperature both to cause the bolts on the NRV to expand and thus increase the size of the leak and also to cause the cyclohexane vapour escaping from the box to ignite spontaneously on admixture with the oxygen in the air.

(iv) In the case of the gasket blow the escaping vapour creates a static charge in the unearthed cladding and is ignited by a subsequent discharge.

(v) In either case the escaping and ignited cyclohexane produces a flame in the direction of elbow G (Figs. 9 and 10).

(vi) This flame does a number of things:

(a) It directly destroys the cladding and lagging in the area where the small crack in the petal of the split was ultimately found so as to enable the galvanised wire presumed to be there to come into contact with the stainless steel pipe.

(b) It directly destroys the cladding and lagging at the intrados of elbow G so as to be able to play directly on the wall of the pipe.

(c) It either directly or indirectly destroys the lagging and cladding in the area of the 3 inch split (Fig. 9) so as to enable the galvanised wire presumed to be there to come into contact with the pipe.

(d) It directly heats the zinc on the glavanised wire in both places to a sufficient temperature to melt the zinc on to the pipe and, possibly, blows it on to the pipe.

(e) It directly heats the pipe at the 3 inch split, at the intrados of elbow G (see Fig. 9) and in the area of the petal crack to a sufficient temperature to cause zinc embrittlement (see Appendix II) of the pipe despite the cooling effect of the flow of liquid in the pipe.

(f) It directly heats the intrados to a sufficient temperature and for sufficiently long to produce creep cavitation of the kind ultimately found (see Appendix II).

(g) At the same time it directly heats the hanger rod and strap to a temperature at which, on rupture at the elbow, the rod will suffer a straight pull tensile point fracture but will still have sufficient strength to bend the strap (see Appendix II).

(vii) The flame does all these things without the sensor bulb being actuated by:

(a) radiation from the flame itself;

(b) radiation from or direct impingement of flames from other orifices or joints in the lagging box.

(c) convected heat from other flames, if below the bulb;

(d) radiation from the pipe at the 3 inch split, the intrados, the area of the petal crack and the hanger and strap.

(viii) Neither the flame which is essential to the hypothesis nor the other flames emerging from the lagging box destroy the box.

(ix) After the rupture there is an immediate discharge of fluid in all directions but with a main upward component, which will probably quench all pre-existing flames.

(x) The liquid discharge is followed by a vapour discharge also with a main upward component.

(xi) A fire occurs in the region of the fin coolers which melts the zinc on the fins on to the stainless steel pipes and raises them to a temperature at which zinc embrittlement occurs.

(xii) This is followed by an explosion in the region of one of the southern fan motors which blows the two southern rotors up through the space left by the zinc embrittled pipes and fins which will have been blown away by the explosion.

(xiii) The same explosion will cause a near vertical downward force on the 20 inch assembly sufficient to trigger jack-knifing.

141 We have set out the steps in the hypothesis from answers given by Dr Cox when examined by the Court at the conclusion of his evidence. This examination was directed to ensuring that we had correctly appreciated the main steps in the hypothesis some of which appeared to us in conflict with facts which were beyond dispute.

142 We now turn to consider each of the steps in the hypothesis in turn. In doing so we shall have occasion to refer frequently to Appendix II in which we have set out both a more detailed description of the significant features of or associated with the 8 inch line than appears in Paragraphs 131–138 above and also the results of the metallurgical evidence relating thereto.

Examination of individual steps in 8-inch hypothesis

Loose bolts

143 The existence of two loose bolts in the bolt cage of the NRV was recorded on photographs taken within a few days of the disaster by the Factory Inspectorate and by Dr Gugan. It was not until a considerable time after the photographs were taken that the fact that the two bolts were loose was observed by anyone. Neither Dr Gugan nor the Factory Inspectorate representatives observed this fact at the time the photographs were taken. We conclude that the photograph taken by the Factory Inspectorate was taken on the 6th June and that Dr Gugan's photograph was taken on the 5th June. There was some confusion about the date of the latter photograph owing to the fact that when it was originally produced it had been dated by Dr Gugan on the reverse as the 10th June but we are satisfied from his evidence that this was the date on which the roll of film was sent for processing and not the date on which this particular photograph was taken. We accept his evidence that it was, in fact, taken on the morning of the 5th June. From this it follows that we conclude that the bolts were loose at that time.

144 The bolts were, beyond question, loose to an extent which could not be accounted for by the disaster itself. They must therefore either have been loose before the disaster or loosened between the time of the disaster and the morning of 5th June.

145 We heard evidence from Mr Halderit that he had at some time caused a Nypro employee or employees to attempt to loosen the bolts on the bolt cage. This was done on the instructions of Mr D H Jas of DSM. The attempt was abandoned through lack of tools. He could not remember exactly when this attempt was made but we are satisfied that it was not before the morning of 5th June. Examination of the bolts themselves by Mr H G Orbons of DSM revealed some but not a reliable indication that one of them had been tight at the time of the disaster. No indication either way could be gained from examination of the other bolt.

146 We had no reliable evidence to suggest that any other person had had access to or attempted to interfere with the bolts between the disaster and the morning of the 5th June. We therefore conclude that the two bolts were probably loose before the disaster.

147 In arriving at this conclusion we have taken account of the fact that such a conclusion necessarily involves the implication that Nypro employees must at some time have failed to tighten them properly and that this, in view of the attention paid to preventing and stopping leakage, is not very probable. We refer to Nypro employees for it was clear that, whenever and however the bolts came to be loose, Sim-Chem Limited, the constructors of the plant, were in no way responsible. This was fully accepted by Nypro.

148 Having found that the first step and essential foundation of the 8-inch hypothesis is a probability we should comment, before we advance to the next step in the sequence, that the fact that much time had to be spent on investigating the question whether the bolts were loosened after the disaster points forcibly to the need for an established procedure to be introduced for preventing the possibility of uncertainty about such matters. Rescue questions must of course take precedence but had the site immediately been put under the control of, for example, the Factory Inspectorate and everyone excluded from access save for rescue work there need have been no room for uncertainty about this vital matter.

Leaks and lagging fire

149 The hypothesis of the lagging "fire", as the origin of the flame said to have caused the 50 inch rupture, was first propounded by Dr Gugan. The suggested steps in the generation of the lagging "fire" are as follows:

(1) A slow leak of process fluid would have taken place at the NRV (Fig. 9), on account of the two loose bolts referred to in Paragraphs 143–148. There are two joints where the NRV butts up against the retaining flanges which are connected by the bolt cage; there could be leakage through either or both of these joints on account of the loose bolts.

(2) The process fluid which would have leaked consisted of about 94% cyclohexane and less than 4% cyclohexanol and cyclohexanone, the remaining 2% consisting of a variety of compounds including tarry by-products which were about 0·5% of the whole process fluid. If leakage occurred over a period of months, these tarry by-products would have accumulated.

(3) When sufficient of these substances had accumulated in the lagging round the pipe, spontaneous heating would occur.

(4) This would have heated the bolt cage round the NRV causing the bolts to expand, thus increasing the leakage of process fluid.

(5) The augmented leakage of process fluid would then have resulted in an emission of cyclohexane vapour at auto-ignition temperature. This would, on obtaining sufficient oxygen, have been ignited as a result of continued emission, at such temperature, of further cyclohexane vapour.

150 Lagging fires are quite common in industry, and the above steps seemed on first examination to form a reasonable hypothesis. However, the usual lagging "fire" involves a relatively involatile substance, eg a drip of lubricating oil on to the lagging round a steam pipe. In such a case the oil, being relatively involatile, can accumulate and eventually given decomposition products which may self-ignite. The so-called "process oil" flowing normally in the 8 inch pipe, is a quite different material: as noted above, the greater part of it is cyclohexane which boils at about 80°C at atmospheric pressure; the 8 inch pipe normally runs at 155°C, and hence almost all the cyclohexane — and some of the other components in the process oil — would have evaporated on leaking out into the lagging box. This raises the question whether condensation would occur within the lagging box and if so to what extent. The possibility of a lagging fire with highly volatile process oil thus posed a problem completely different from the more usual problem of a lagging fire with relatively involatile oil. The problem of the volatile oil was clearly unfamiliar to the experts on lagging fires who gave evidence, namely, Dr Gugan and Mr P C Bowes of the Fire Research Station.

151 We now consider the steps in the hypothesis which we have set out in Paragraph 149 above.

(i) *Leak of process fluid*

Although we have concluded (paragraph 146) that the bolts were probably loose before 1st June it is by no means clear that this would have led to a leak. The flanges held by the bolts are massive, the remaining ten bolts were tight and the stresses in the 8 inch pipe in normal service would produce no bending moment at the NRV. Conditions were not therefore conducive to a leak occurring. Nevertheless there was ample evidence that leaks frequently occurred on the plant and we therefore conclude that a leak, as such, is a reasonable possibility.

(ii) *Accumulation of low-volatiles within the lagging box*

Had there been such a leak the next question is whether it would have gone undetected for the necessary period (1–18 months) for accumulation of self-igniting materials. It was envisaged that the leak would produce the equivalent of about 300 × $\frac{1}{8}$ inch diameter droplets per minute. The cyclohexane content of this would at once vaporise and we accept that a large proportion of the vapour would escape from the box. Since such vapour is heavier than air it would tend to fall to the area of the sight glass below, which was regularly read by the operators of the plant. We had ample evidence that all personnel were constantly alert for leaks and that cyclohexane has a distinctive smell. Although we accept that at times the emissions might have gone undetected due to dispersal by wind or masking by other odours, we regard it as most unlikely that such an emission could have gone undetected for even one month and, had it been detected, we have no doubt that action would have been taken.

It was also suggested that the leakage would have resulted in corrosion of, and therefore drips from, the lagging box as a result of the caustic content of the fluid. This may well be so but we find it unnecessary to reach any conclusion on this matter.

(iii) *Self-heating of accumulated residues*

The question of self-ignition of low-volatile residues was the subject of experiments by Dr C P M Sadée of DSM, who gave evidence. Mr Bowes, an authority on lagging fires, also gave evidence on this topic.

Dr Sadée carried out experiments on the self-heating of residues from process fluid, and on simulated residues, in a specially designed apparatus, using either a sample of the residue or a sample of lagging soaked in the residue.

In no case was it possible to get self-heating below a temperature of 215°C, as compared with the process temperature of 155°C. Thus Dr Sadée's work suggested that self-heating is unlikely. The relevance of his experiments to the particular problem was criticised and we accept that it is almost impossible to simulate the plant conditions exactly.

Mr Bowes considered that such experiments had not disproved the possibility of residues from the process fluid undergoing self-heating but he was also of the view that "... the tarry residues ... would only present a hazard if the pipe temperature were suddenly to be increased substantially above the usual operating value". There was no evidence to suggest that this ever happened. Further tests were suggested. We considered this suggestion but were of the view that to produce any worthwhile results the experiments would be very costly, would or might have to occupy a period of many months and would even then be inconclusive. We did not therefore consider the undertaking of such tests — and the delay involved — to be justified.

We proceed on the basis that it is a possibility, albeit remote, for an accumulation of low volatiles to have resulted in self-heating to the temperatures necessary to support the next step in the hypothesis although, despite enquiries, we were unable to find any instance of such an occurrence having resulted from a leakage of process fluid in any of the plants in which it is used.

Our conclusion as to the possibility of self-ignition and the occurrence of a lagging "fire" is that it is possible but very unlikely.

(iv) *Increase in amount of leakage*
Assuming self-heating we accept that the differential expansion between bolts and valve cage could have opened up the gap for increased leakage at the flange or flanges.

(v) *Ignition of escape*
We accept that, given an appropriate temperature, escaping vapour could be subject to auto-ignition.

152 Our conclusion with regard to this step in the 8-inch hypothesis is that even if (i) there could have been a sufficient accumulation in the minimum postulated period of one month and (ii) that accumulation could have caused the necessary self-heating it is most unlikely to have gone undetected.

Before departing from this subject, however, we would mention one more feature connected with it. The most obvious and well known feature of lagging "fires" is that, when self-heating reaches a certain temperature, copious fuming results. Since such fuming would undoubtedly have been observed had it occurred the hypothesis has to assume that the self-heating process had not yet achieved fuming temperature.

Gasket burst
153 At a very late stage in the hearings, Dr Cox declared the difficulties of the lagging "fire" hypothesis to be such that he preferred as an alternative that one of the gaskets at the NRV had failed on account of the loose bolts — and that the resulting large escape of process fluid had then created a static electricity charge on the aluminium cladding which, on discharge, had led to the ignition of the escaping vapour. Our misgivings about the lagging "fire" were thus shared by at least one of its original supporters.

154 We accept that gaskets can blow and that flashing liquid can produce a charge of static electricity on conductors such as aluminium. We also accept that once a charge has been created a discharge could cause ignition of an inflammable vapour. We therefore accept the hypothesis as a theoretical possibility. We consider, however, that it is little more than this. The hypothesis was very much an afterthought; no similar incident was brought to our attention; Dr Gugan remained of the view that it was less likely than the lagging "fire" hypothesis.

The flame to produce the desired results
155 It is in connection with this part of the 8-inch hypothesis that its proponents allowed their belief in its correctness to cloud their judgment and to lead them into a lack of objectivity which we found difficult to understand. Dr Gugan for example, who was the first to give evidence in support of the theory, proceeded, throughout his examination and cross-examination, which occupied a period of four days, on the basis that the only flame emerging from the lagging box was a flame directed towards the intrados of elbow G: that this flame would not destroy the box — the continued integrity of which was essential to the theory — because it would be an off-port flame: and that this flame would not actuate the sensor bulb because it was directed away from it. It was only at the conclusion of his evidence that he said, in answer to questions by the Court, that if there was a flame in the direction of the intrados there would also be a broadly equivalent flame from the annulus at the downstream of the box and other flames from the opening by the spindle of the block-valve and from the joint in the two halves of the box. In this situation he accepted that the lagging box was likely to be destroyed (if it was split vertically) and that the sensor bulb was likely to be actuated.

156 We regard his answers to the Court at this stage as being of considerable importance and we quote some passages from the transcript:

Q Just for the moment take it stage by stage with me. I have got to get a flame going towards the elbow?
A Yes.

Q The minimum flame you contemplate is 5 feet 4 inches?
A Yes.

Q If there is pressure in the box sufficient to produce a flame at that length from the annular orifice upstream and since pressure will be equal throughout the box it will, presumably, have a tendency, at any rate, to produce a like result through the annular orifice at the other end of the box?

A Yes.

Q Since the vapour will be under equal pressure and at the same temperature, I would suppose that when it went out at the other end of the box it would also, on meeting the friendly oxygen, ignite?

A Yes.

Q At the same time as that is happening I would expect also that the cyclohexane under a like pressure would have emerged from the orifice, also annular but smaller, where the wheel control is?

A Yes.

Q I would expect that that too, on meeting the friendly oxygen would ignite?

A Yes.

Q You agree this must happen?

A If it ignites I agree.

Q Is there any possible reason why it should not ignite? All this vapour is coming from inside the box at a like temperature?

A Gas cookers can provide flames and still have leaky gas pipes. The gas is still coming through the same pipework, but you need not have ignition of a leak in the presence of a flame.

Q We are not talking about gas cookers, but about a lagging box which is full of cyclohexane vapour under pressure at a temperature at which on meeting oxygen it ignites?

A Yes.

Q Tell me if there is any reason why the escape which must inevitably happen round the control valve does not also ignite?

A No.

Q There is no reason?

A No reason; it depends upon magnitude, of course.

Q What does that mean?

A I do not know how big the hole is through which it will come.

Q No, but assume for the moment that there is a clearance round the spindle where the wheel emerges?

A Yes.

Q That would be an orifice through which cyclohexane would escape and ignite?

A Yes.

Q Finally, there would be a general escape all around the places where the box joins?

A Yes.

Q That also would probably ignite?

A Yes.

Q So at the very moment that the flame emerged from the upstream end, towards the elbow, there is an emergence of flame downstream — there is an emergence of flame sideways through the place where the wheel goes in?

A Yes.

Q And there is an emergence of flame all round where the joints are?

A Yes, the flames will all bear the size relationship with the orifices they escape through.

Q Those flames would certainly be hot enough to melt the aluminium?

A Yes.

Q When the aluminium was melted the lagging would disappear?

A Yes, the whole thing falls down if the lagging box does not survive.

Q . . . I have understood the possibility of the quartz bulb not going off while I was considering one flame going along the axis of the pipe upstream.

A Yes.

Q I find that difficult. I find it even more difficult when I have got not merely that flame, but all these other ones too. Do not answer quickly, think. Do you really regard it as at all probable that with all these flames the quartz bulb would not have gone off?

A I think the best answer I can give you, which is the only one I can without having established it by actually doing an experiment — which I think could be done quite straightforwardly — is that with total general fire around that, with a larger number of orifices, with flame projecting out of them all, it is most unlikely that the sensor bulb would not have detected fire.

157 This same failure to take account of important matters was revealed, in the same connection, by Messrs Manderstam in calculations produced by them for the heat likely to reach the sensor bulb. These took into account radiation from the pipe wall assumed to be at 1000°C for 15 inches up and downstream of the intrados and a thin flame towards the elbow only. No account was taken of the other flames, of radiation from the pipe hanger and strap or of radiation from the surface of the lagging box which, on the lagging "fire" basis, would have been at a temperature sufficiently high to produce a minimum temperature in the escaping cyclohexane of 270°C (auto-ignition temperature).

158 With this preliminary we now turn to consider the major points in the theory. In doing so we shall proceed on the basis that (i) a lagging "fire" could cause spontaneous ignition or (ii) that an escape from a gasket burst could cause ignition by creating a static charge on the cladding and subsequent discharge. We proceed directly to consider the quantities of cyclohexane envisaged and the flame thereby produced.

159 The quantities of cyclohexane envisaged varied considerably. Dr Gugan envisaged an initial slow-leak of 0·52 lb per hour extending over a long period (for the production of the build-up of tarry residues necessary to create a lagging "fire") increasing to 0·5 lb per second as a result of the assumed heat of the lagging fire leading to a widening of the gap by expansion of the flange bolts. Messrs Manderstam, towards the end of the hearings, produced calculations showing about 0·03, 0·06 and 2 lb per second and Dr Gugan when recalled almost at the end of our hearings, also advanced a figure of 2 lb per second as the reasonable escape from a partial gasket blow.

160 There was no evidence of any actual emission. We shall therefore consider the 0·5 lb per second postulated by Dr Gugan for the emission resulting from a lagging "fire" and the 2 lb per second postulated by both Manderstam and Dr Gugan as the emission resulting from a gasket blow. Dr Gugan calculated that a discharge of 0·5 lb/second proceeding wholly through the upstream annulus would have produced a flame releasing approximately 30,000,000 BTU's per hour or enough heat to heat 600 small houses. Combustion at an emission rate of 2 lb per second would have released approximately 120,000,000 BTU's per hour. All this heat would be released in the area between the two separators not more than 24 inches from the sensor bulb if one considers the flame towards the intrados alone. If one considers also the flames emerging in other directions, particularly that from the spindle of the block-valve and the joints between the two halves of the lagging box (whether horizontally or vertically divided) they would be considerably closer to the sensor bulb.

We agree with Dr Gugan that it is most unlikely that such flames would not have actuated the sensor. They would so have actuated it at a very early stage and thus the fire would have been doused. Even if the flames alone would have been insufficient to actuate the sensor, parts of the pipe were required to be at high temperatures to support the theory, and radiation from this hot metal and from the lagging box itself, would, when added to the effect of the flame, surely have actuated the sensor.

We infer, indeed, that there is little or no dispute about this. It was on Friday 29th November 1974 that Dr Gugan stated that it was most unlikely that the sensor would not have been actuated by flames from emissions of the size then contemplated. No-one from any source produced, before the end of our hearings on 20th February 1975, any calculations to suggest the contrary. Instead a suggestion was somewhat tentatively raised that there may have been no sensor bulb present at all. We reject this suggestion. There was convincing evidence that there was.

161 We are also satisfied that, even if there had been no sensor, the lagging box would have been destroyed and the necessary directional flame with it. The strap and hanger rod must have been subjected to direct flame sufficient to raise them to temperatures in the region of 920°C (see Appendix II) and the lagging and cladding immediately upstream must also have been subjected to direct flame in order to remove them and enable the wire to fall onto the pipe. At the position of the petal crack the pipe must then have been raised to 800–900°C (the point of zinc embrittlement, see Appendix II). This involves of necessity, the flame being in contact with the upstream end of the box (see Fig. 9), and its swift destruction, the melting point of the aluminium cladding being about 650°C. In addition, the flames from other orifices, particularly the joint between the two halves of the box (whether vertically or horizontally split) would in our judgment have contributed to its destruction.

162 We now turn to consider whether, even if a directional flame could have been maintained without being doused by the operation of the sensor bulb and deluge system and without destroying the lagging

box (without which the flame could not survive) it could have done what was required of it. We shall proceed upstream from the lagging box.

(i) *Heating of the hanger*

163 No doubt it could have done this if stabilised although Messrs Manderstam proposed a flame which did not stabilise until it reached the intrados.

(ii) *Removal of cladding and lagging*

Equally no doubt it could have done this if stabilised. The cladding melts at about 650°C and flame temperature would have been more than sufficient to melt it away and thereafter to destroy the lagging.

(iii) *Transfer of zinc from assumed galvanised wire to the 8 inch pipe at petal crack*

We are unable to see how this can have occurred. Whether the zinc transfer is to be effected by the wire with zinc on it being in contact with the pipe, or by having melted zinc on it being blown onto the pipe, the cladding and lagging between it and the pipe, have first to be removed. As soon as the cladding has been melted away the wire would be directly exposed to the flame. Since zinc melts at 419°C and vaporises at 907°C, we cannot regard it as a real possibility that the wire would still have had zinc on it when it was finally free to fall onto the pipe.

(iv) *Heating of pipe to zinc embrittlement temperature at petal crack*

It was accepted by all experts that the temperature of the pipe wall could not be raised to any material degree save by an intense flame sufficient to cause vapour blanketing within the pipe. We had no evidence to suggest that a vapour blanket would have extended downstream of the elbow to the position of the petal crack.

It was suggested by Dr Cox that the desired temperature might be reached by downstream heat transfer from the vapour blanket area but this suggestion was dispelled by the evidence of Dr Gugan who stated in effect that the temperature drop would be immediate outside the vapour blanket area.

(v) *Destruction of lagging and cladding at the intrados*

This is clearly possible.

(vi) *Raising of the intrados to a sufficient temperature to cause creep cavitation rupture* (see Appendix II)

As creep cavitation rupture depends upon a combination of stress, time and temperature, it is first necessary to consider what assumptions need to be made as to stress and time. Only then can it be determined what temperature is needed to be reached to support this hypothesis.

In our judgment (and here we accept Nypro's submission), if the 8-inch hypothesis is to be sustained, there must have been nothing wrong that was readily visible earlier than 20 minutes before the rupture or it would certainly have been observed and action taken. It must therefore be assumed that the maximum period during which a directional flame could have occurred was 20 minutes. Once the flame is established it must then destroy lagging and cladding before it can play on the intrados and it must then raise the intrados to and maintain it at the relevant temperature.

At the material time the plant was dry-cycling under normal operating pressure and this would produce maximum hoop stress of about 7 kg/mm^2 at the elbow. At this stress the Cottrell/Swann experiments indicate that a temperature of 925°C or more would be required to produce a creep failure within 20 minutes. Taking into account that the time to remove cladding and lagging and to heat the pipe wall to the relevant temperature must be deducted from the available 20 minutes it follows that a minimum temperature of 925°C would be required.

In his evidence, both in chief and under cross-examination, Dr Gugan said that he did not see how the flame which he envisaged could raise the temperature of the pipe wall beyond 900°C on pure heat transfer grounds and reaffirmed this in answer to a question from the Court:

The Chairman Am I missing something? — I had thought that you could not get the pipe wall to anything like 900°C without a vapour blanket and that once you got the vapour blanket you could go on well beyond 900°C.

A No it is 900°C with a vapour blanket.

Since Dr Gugan was envisaging that the flame might have been in operation for up to two hours before rupture this in no way conflicted with his theory. At 900°C and operating pressure, failure would be likely to occur in about 100 minutes. (See Appendix II.)

Dr Cox expressed the view that a higher temperature might possibly be reached but we consider this unlikely.

Accordingly we conclude that even if the envisaged flame could have persisted it is improbable that it would have been able within the available time to have caused a creep failure at the elbow.

(vii) *Destruction of cladding and lagging beside the 3 inch split*
This is clearly possible but, if done by direct impingement rather than by the lagging and cladding falling off because of the flame destruction of the lagging and cladding at the elbow, it requires a flame to have extended 5 feet up the pipe from the elbow. Such a flame must in our judgment have destroyed the lagging box.

(viii) *Deposit of zinc at 3 inch split*
The proposed mechanism here is the same as that for the "petal" crack. We consider it unlikely for the same reasons.

(ix) *Heating of pipe wall at 3 inch split to zinc embrittlement temperature*
The temperature required is 800°C to 900°C. Such a temperature could not be reached without the creation of a vapour blanket and the creation of a vapour blanket requires an intense directional flame. There was no evidence whatsoever to suggest that such a flame could have been produced by any emission from the lagging box or that, if it could have been, it was capable of creating a vapour blanket in the area of the 3 inch split which was in the vertical part of the pipe. We reject as fanciful tentative suggestions made that the whole section of the pipe might have been the subject of a vapour lock.

We conclude that there is no reasonable possibility of the envisaged flame raising the pipe wall at the 3 inch split to the necessary temperature to cause zinc embrittlement.

Fire in area of fins and embrittlement of enclosed pipes
164 Since the 8-inch hypothesis assumes that the explosion bringing the 20 inch assembly down and blowing the cooler motors up occurs within about 10 seconds of the rupture this section of the hypothesis requires that within such 10 seconds:

(i) A fuel source (presumably cyclohexane from the 50 inch rupture) is provided in the area of the fins.

(ii) The fuel should have been ignited in some way.

(iii) The fire thereby produced should have heated the fins to 419°C so as to melt the zinc onto the stainless steel tubes.

(iv) The fire thereby produced should have raised the stainless steel tubes to a temperature of not less than 800°C so as to cause zinc embrittlement.

(v) Despite this fire an unignited vapour cloud was building up at the same time so as subsequently to ignite and produce an explosion which would exert a downward force on the 20 inch assembly and an upward force on the fan motors.

No evidence of any kind was given to suggest that this was a reasonable possibility much less a probability. We regard it as most unlikely.

Explosion exerting downward force on 20 inch assembly and at the same time an upward force on the motors sufficient to blow them up and to the west
165 In view of the fact that pressure waves caused by deflagration or explosion produce unpredictable effects we would not regard the difficulties involved in envisaging the required explosion as a serious impediment to the acceptance of the 8-inch hypothesis if the remainder of it were acceptable. We need therefore take no further time on it.

Vertical emission from 8 inch rupture lasting for short period after the main explosion
166 This is not a particularly important part of the hypothesis for it is connected with the flame seen on the Goodchild film which we have already considered. It deserves further mention, however, because one of the objections to the 20-inch hypothesis was that, if it was correct and thus that the 50 inch split occurred some minutes later, a vertical jet of flame from that split would have been seen and no witness had reported seeing any such flame.

167 Dr Gugan caused experiments to be carried out to ascertain whether a vertical flame could be expected from the 50 inch split. These were on a small scale and were made the subject of a written report dated the 10th February 1975.

168 These investigations involved the discharge of propane through a $\frac{1}{4}$ inch diameter pipe which was an approximate scale model of the configuration of the 8 inch pipe run between S2538 and S2539. The 50 inch split was initially represented by a carefully machined cut which did not, however, attemt to reproduce the "tulip" effect in the original split. Discharge under these conditions produced a largely horizontal flame in the direction of S2539 with no vertical component.

169 Further experiments were then carried out using jubilee clips partially to close either or both ends of the machined cut out and it was found that when the downstream end of the cut-out was partially obscured it gave rise to "a diffuse discharge forwards and sideways but a clear and singular near vertical upwards discharge". This was illustrated by photographs in the report from which we have quoted.

170 The report continued "Any *exact* simulation of the flow conditions arising from the elbow therefore, it would appear, would give rise to a vertical emission since the jubilee clip reproduced the circumferential tear which occurred at the downstream end of the 50 inch split where the tear in the metal bifurcated then turned and ran parallel to the hanger strap".

171 We do not doubt that Dr Gugan, when he wrote his report and when he confirmed it in evidence, sincerely believed that by the use of the jubilee clip he had produced a near exact simulation of the flow conditions because the clip reproduced the circumferential tear. It did not, however, do so, as Dr Gugan readily agreed when questioned by the Court at the conclusion of his evidence on the 12th February 1975. We quote these questions and answers.

Q When I look at the photograph No. 7 which shows the jubilee clip which was round the bottom end of the machined orifice, one thing that strikes me somewhat forcibly is that, whereas on the split as it was you have a continuous more or less downhill run, where the tear is your jubilee clip extends well into the machined part and has no equivalent of the horizontal directional flap at all.

A No.

Q Do you not think that perhaps if part of the jubilee clip had been torn away so as to give such a flap, it might have been a more accurate representation?

A Yes.

Q If you had done that, would it not be right to expect that with that configuration the horizontal throw would be even more considerable than without the jubilee clip at all?

A Yes.

There was therefore no evidence that an emission from the actual 50 inch split would produce any substantial upward component and it is improbable that it would.

172 We have dealt with this particular point in some detail for it appears to us to be a good example of the way in which the enthusiasm for the 8-inch hypothesis felt by its proponents has led them to overlook obvious defects which in other circumstances they would not have failed to realise.

General observations on the 8-inch hypothesis

173 This hypothesis had its origin in eye-witness evidence at a time when the cracks found in the 8 inch line were thought to be "cold" stress corrosion cracks. At that time there was no doubt, much to commend it. When, however, it was ascertained that the cracks were "hot" cracks caused by zinc embrittlement it became at once a less likely hypothesis. From then until the possibility of a gasket burst and ignition by static was raised by Dr Cox on the 62nd day of the hearings it depended upon a lagging "fire" and it finally depended in the alternative on a gasket burst. Both theories, however, had as essential features the production of a directional flame from the lagging box for a sufficient time and of sufficient intensity to cause, at least, creep cavitation at the elbow, zinc embrittlement at the petal crack, and heating of the hanger and strap. Moreover this must be done without either destroying the lagging box, the integrity of which was essential to the continuation of the directional flame, or actuating the deluge system which would have doused it. It involved great ingenuity to produce any argument to support such a proposition even when the fact that there would be other flames was ignored. When, however, the inevitable presence of other flames was taken into account the hypothesis became so improbable that in our judgment it has to be excluded unless there is some compelling evidence from the wreckage that the 8 inch line must have ruptured before the main explosion.

174 We have hitherto mentioned mainly the evidence of Dr Gugan and Dr Cox. We should now refer also to the evidence of Professor Ball, who was retained by Dr Gugan on behalf of the Insurers on the 24th June 1974 to examine and report on the metallurgical aspects involved. Professor Ball submitted a report to us and gave evidence before us. His support for the 8-inch hypothesis was unqualified. His evidence consisted in interpretation on a general basis of the experimental and theoretical work done by others for he himself had done neither experiment nor calculation. He at

no time faced up to the difficulties involved in the 8-inch hypothesis and we received little or no assistance from his evidence.

175 We should also mention again in this connection the reports submitted and evidence given by Professor Sir Alan Cottrell and Dr P R Swann, who advised Nypro. They confined themselves to the metallurgical aspects of the case and wisely kept within their own expertise. Their first report ended:

"The straight forward interpretation of our observations suggests the following sequence of events taking place as the temperature of the 8 inch pipe rose, during the disaster:

1 Molten drops of zinc splashed onto exposed parts of the pipe while these were still moderately hot and not yet heavily oxidised.

2 When the splashed spots became heated to about 800°C they formed small cracks in the pipe. A few of these cracks joined up and became the 3 inch crack.

3 The loss of the mechanical strength in the region around the 3 inch crack, due to the high temperature and to the presence of the crack itself, caused the plastic bulge to form there under the action of the pressure, which presumably at this stage had attained the limit set by the relief valves.

4 Over a period of several or many minutes at these high temperatures, the sustained pressure caused the red hot parts of the pipe to swell by creep deformation.

5 At the elbow G, where the temperature may have exceeded 900°C, this creep deformation became sufficient to cause failure by w-type cavitation and the 50 inch split then formed.

While it is our opinion that the above represents the most direct interpretation of the metallurgical evidence, we nevertheless recognise that this evidence is not sufficient by itself to exclude other interpretations".

176 We agree with these observations and would accept them unless, again, there is compelling evidence from the wreckage that the 8 inch line must have ruptured before the main explosion. To an examination of this problem we now turn.

Damage connected with the 8 inch line

177 We shall deal here briefly only with four features associated with the 8 inch line which were considered at one time or another to have an important bearing on the relative probabilities of the rival hypotheses:

(i) the 3 inch split and bulge;

(ii) the bent pipe hanger, spring and angle iron;

(iii) the orientation of zinc cracks on the 8 inch pipe;

(iv) deformation at elbows G and K.

178 The 3 inch split resulted from the combination of zinc embrittlement and the hoop stresses produced by internal pressure. The local bulging was also due to internal pressure. There were strong indications that the split and bulge had occurred before the 50 inch split. There appeared to be no possibility of producing any flame capable of causing embrittlement before the 50 inch split if the latter had occurred before the collapse of the 20 inch assembly. (See paragraph 163 (ix)). This pointed strongly to that collapse having preceded the 50 inch split.

179 Until Dr Cox gave evidence it was suggested by the proponents of the 8-inch hypothesis that the 3 inch split had followed the 50 inch split and had occurred in fires following the main explosion, albeit that the 50 inch split would by then have caused a very considerable reduction in pressure in the line.

180 We considered it improbable that if the 50 inch split had been the first event there would have been sufficient pressure left to create the stresses necessary for zinc embrittlement and bulging. Further experiments were suggested to us but we did not consider they were justifiable. It was clear that the 3 inch split and bulge were consistent with the 20-inch hypothesis. Experiments could only therefore have assisted on the question whether they were also consistent with the 3 inch split having occurred after the 50 inch split. We were and are content to accept the fact that this was possible without reaching any conclusion on the point.

181 The alternative suggestion in support of the 8-inch hypothesis put forward by Dr Cox that the necessary zinc embrittlement had preceded the 50 inch split and had resulted from (a) a deposit of zinc from the galvanised wire securing the lagging after a flame from the lagging box had directly or indirectly removed cladding and lagging and (b) the heating of the pipe wall to zinc embrittlement temperature by a flame from the lagging box or radiation from the intrados we have already rejected (paragraph 163 (ix)).

182 We conclude that there is no feature connected with the 3 inch split and bulge which makes the 8-inch hypothesis any less improbable.

Fractured pipe-hanger, spring and angle iron

183 This damage is shown on Plate 15. It is plain that all this damage is consistent with the 20-inch hypothesis. We mention only one feature namely that the hanger rod was fractured by tensile fracture at high temperature (above 900°C) and the top half was bent sharply through 90°.

184 On the 8-inch hypothesis the fracture is said to have occurred by reaction forces from the opening of and efflux from the 50 inch split. This would produce reaction towards the bottom left-hand corner of Fig 9. We are quite unable to see how at the same time the rod can have achieved a 90° bend. Such a bend implies a very large movement of the NRV assembly since, by virtue of the clearance between the rod and the support bracket a rotation of 100–120° would be required. This would demand an upward movement of elbow G such that the point of attachment of the hanger rod to the strap was above the level of the angle iron bracket (Fig 9). No possible suggestion as to how this could have occurred was made to us.

185 It was, however, suggested that after the fracture of the rod had taken place at the time of the rupture, the top portion of the rod with the spring would have been left in place with the rod projecting downwards through the bracket. It was then suggested that the bend had been produced by some flying object at the time of or after the main explosion. We regard this explanation as most unlikely. In the first place the spring would have been under compression at the time of the tensile fracture. It is therefore unlikely that when the compression in the spring was released by the fracture of the rod the spring and rod would have remained in position. Secondly, even if the rod and spring did so remain it would be difficult if not impossible to produce a 90° bend at a time when the rod was lacking any firm support.

186 We find the bend in the hanger rod increases the improbability of the 8-inch hypothesis.

Orientation of zinc cracks in the 8 inch line (See Appendix II)

187 We attach no special significance to the directions of zinc cracks in the sections D, F, G, H, J and K (Fig 10) which were suggested to strengthen the 8-inch hypothesis and weaken the 20-inch hypothesis. The combination of thermal stress due to differental heating, hoop and longitudinal stress due to internal pressure, and torsion and bending stress due to loading from falling columns and other items of equipment, could produce a whole variety of stresses varying with time. All these zinc cracks could, in our view, readily have been generated in the post-explosion period when the pattern of stress could have been changing from moment to moment.

Deformation of elbow K and 40° pipe twist

188 This damage could, on the 20-inch hypothesis, have occurred when the 50 inch rupture formed, and the efflux of vapour gave a violent reaction force on the pipe, causing the 40° twist and the deformation of elbow K at high temperature. This mechanism explains why the weld on the extrados of the pipe at elbow G is untwisted, and why the bolt cage on the NRV showed little sign of torsion; the twist took place after the 50 inch rupture, when the opening of the elbow due to the 50 inch split made the elbow torsionally weak so that it would rotate about the weld at the extrados; also due to the high temperature the metal would be relatively weak so that the elbow material could exert almost no torque on the bolt cage.

Deformation of elbow G

189 It was said that the absence of the appearance of torsional stress ruled out post-explosion failure of elbow G. But during the explosion the movement of separators S2538 and S2539 was very limited (See Appendix II) and the movement of the points of attachment of the pipe to the separators even more limited. Thus immediately post-explosion, due to the configuration of the pipe work (Fig. 8), there would be only a limited torsional strain on elbow G. Furthermore, torsional stresses would relax plastically when the material was heated, whereas the internal pressure in the pipe would

continue to generate hoop stress in spite of plastic yielding. It is therefore reasonable to expect that the creep cavitation cracks should appear to have been generated by hoop stress only.

190 We conclude that the deformation at elbows G and K is compatible with the 20-inch hypothesis and does not decrease the improbability of the 8-inch hypothesis.

20-inch and 8-inch hypotheses compared

191 We now consider the relative probabilities of the two hypotheses. The 20-inch hypothesis involves no more than acceptance of squirm followed by jack-knifing and rupture at or near the lower end of the temperature/pressure range in which this phenomenon could occur, ie *a single event of low probability* (see para 124). The 8-inch hypothesis, on the other hand, depends upon *a succession of events, most of which are improbable* (Fig. 11). These events are virtually independent, but many require all the preceding events in the chain to have taken place. The sequence is shown in Fig. 11. It is the improbability of the whole sequence that is so great as to make the 8-inch hypothesis unacceptable. An apt analogy is the balancing of billiard balls on top of one another: two balls can just be balanced, though the event is improbable; to balance three balls is much more difficult; to balance ten or more balls one on top of the other is so unlikely as to justify the adjective impossible. Yet this balancing of ten balls is analogous to what is postulated for the 8-inch hypothesis.

192 We accordingly conclude that the disaster resulted from a one stage failure of the 20-inch assembly.

193 We have considered the 8-inch hypothesis at what may perhaps appear unnecessary length. We have done so because its proponents were all highly qualified in their respective fields and because the hypothesis was given considerable attention in some sections of the press before it had been fully examined.

Miscellaneous

194 Before we deal with the question of lessons to be learned there are certain miscellaneous matters which we should mention:

(a) *Security*

Although there was, in general, proper attention to security there were two unguarded gates through which it was possible for any one at any time to gain access to this site.

(b) *Safety precautions generally*

There can be no doubt that Nypro were very safety conscious and that Mr Brenner was an able and enthusiastic safety and training manager. He had created a proper system for dealing with normal hazards and was in the course of preparing and putting in hand a disaster plan. It was not in operation but this has no relevance for it was not designed to deal with an instantaneous catastrophic disaster such as occurred. It was concerned with an escalating situation.

There were undoubtedly certain shortcomings in the day to day operations of safety procedures but none had the least bearing on the disaster or its consequences and we do not take time with them.

(c) *Storage of potentially dangerous substances*

As at 1st June 1974 Nypro were storing on site 330,000 gallons of cyclohexane, 66,000 gallons of naphtha, 11,000 gallons of toluene, 26,400 gallons of benzene and 450 gallons of gasolene. The storage of these potentially dangerous substances is nominally controlled by the local authority issuing licences under the Petroleum (Consolidation) Act 1928. In fact the only licences that had been issued related to 7000 gallons of naphtha and for a total of 1500 gallons of gasolene. The unlicensed storage of large quantities of fluids had no effect upon this disaster but it is clearly useless to have a licensing system which is so ineffective that it can lead to such results. We regard the present situation relating both to storage and use of hazardous materials as unsatisfactory.

Lessons to be learned

Introduction

195 In his evidence to us Mr V C Marshall who was appointed Safety Adviser to the Transport and General Workers Union on the day after the disaster and who was called on their behalf stated "hazard analysis recognises that hazard cannot be entirely eliminated and that it is necessary to concentrate resources on those risks which exceed a specific value".

196 We agree with this statement which accords with reality. No plant can be made absolutely safe any more than a car, aeroplane, or home can be made absolutely safe. It is important that this is recognised for if it is not, plant, which complies with whatever may be the requirements of the day tends to be regarded as absolutely safe and the measure of alertness to risk is thereby reduced.

197 When Mr Marshall refers to risks exceeding a specific value we understand him to refer to risks which exceed what at a given time is regarded as socially tolerable, for what is or is not acceptable depends in the end upon current social tolerance and what is regarded as tolerable at one time may well be regarded as intolerable at another. Nowhere perhaps is this more apparent than in the field of road transport where the construction and use regulations have, over the years, become ever more stringent.

198 It would be both wrong and impossible for us to attempt to assess the current level of social tolerance in relation to risk within the chemical industry but the evidence before us covered a very wide field and has, we believe, enabled us to draw attention to certain objectives which could usefully be considered by the industry itself and by other bodies concerned with safety. We shall, therefore, in addition to dealing with lessons to be learned from the causes and circumstances of the Flixborough disaster make some observation in regard to such objectives.

199 In referring to the industry itself in the last paragraph we would stress that we mean everyone concerned in the industry and not merely designers, constructors and management. If risks are to be identified and reduced to tolerable levels it requires a conscious and constant effort on the part of everyone including those on the shop floor and their Unions.

200 Finally, in these introductory paragraphs we would, albeit it may appear only to be stating the obvious, point out that even when resources have been concentrated on an unacceptable risk that risk will not be completely eliminated. It can never be reduced beyond a certain point because not only may there be human error in the operation of equipment, there may also be human error in the construction of a safety device built in to the equipment. It is to reduce risk from such error that, for example, there are dual braking systems on cars. The existence of the second system substantially reduces the risk of dire consequences if the first system fails but no one can guarantee that the second system will not also fail.

General observation

201 At no point in the inquiry was there any evidence that the chemical industry, or Nypro in particular, was not conscious of its responsibilities relative to safety. On the contrary, there were indications that conscious and positive steps were continually taken with this objective in mind. Nevertheless, the matter is worthy of further discussion. In the chemical industry a number of factors are taking place simultaneously. On the one hand improvement in technology and an ever increasing body of experience intelligently interpreted makes for a continuous increase in safety, while, on the other hand, new processes and increases in the scale of operations can, to some extent, reduce the benefit of experience and can impose problems not only differing in magnitude but also in kind.

202 Although, as we have said, Nypro were safety conscious, the fact that the explosion at Flixborough did take place shows that by accident, mishap and misadventure the stage may unconsciously be set for disaster. The direct causes of the disaster are dealt with fully in other sections of this report. But in a complex plant such as that at Flixborough there are many other factors which contribute to hazard risks.

203 In the first place however carefully specifications controlling the construction of chemical plant are drawn up there is in any plant a probability, however remote, of some failure in each part of the plant. The next question to be asked is, therefore, whether such a failure could lead to a disaster without allowing time for corrective action, even by the most expert and diligent staff, to be taken. If the possibility of failure cannot be eliminated then arrangements for corrective action before a disaster is generated by the failure should, if at all possible, be built into the system. This we would term "second chance design" as opposed to "fail safe design". We believe that this was the concept that was behind certain suggestions made to us such as that the amount of escape of hazardous material from a pressure system should be limited by including in its design such devices as automatic cut-off valves and nitrogen filled "dump tanks" which would automatically operate if a sudden loss of pressure within the system should occur. In certain cases this might be appropriate, in others the added complexity might induce more dangers than it eliminated. This simple example is sufficient to show the difficulty of the problem. Many more examples could be quoted. It is not for us to investigate

such problems in detail, but this is a field of investigation and design which should, in our view, be energetically examined by the appropriate professions and the industry. In many cases there might be a better return (with regard to safety) from expenditure on making the original plant safer than by providing elaborate safety systems to deal with potential inadequacies.

204 In the field of engineering the study and application of ergonomics to manual and monitoring processes are well known. While certain aspects of the Flixborough plant (eg the pressure indicator chart) were not without criticism, the evidence before us indicated that there were no deviations from normal practice in force on other chemical plants. Nevertheless we conclude from the evidence that greater attention to the ergonomics of plant design could provide rewarding results. This again is a field which we feel should be further investigated by the professions and industries concerned.

205 But beyond the accepted field of ergonomics there is a need for the extension of the principle into the field of management. We consider it desirable that plant should be designed and run so that the minimum number of critical management decisions (such as shutting down the plant) have to be considered in any given period of time. When such decisions have to be made they should be made under conditions of minimum conflicting priorities and minimum personal strain. The latter depends on the workload of the person concerned.

206 We repeat that there was no evidence whatsoever that Nypro placed production before safety. Nevertheless, if production is below target and profit is below budget, there are inevitably conflicting, albeit perhaps unappreciated, priorities when decisions have to be made. If factors which could necessitate the shut-down of a chemical plant were given special attention both in the design stage and during planned plant maintenance, this might well increase the first cost of the plant but it might also have the double advantage of:

(a) reducing the number of interruptions to production; and

(b) the number of management decisions to be made under competing priorities.

Taking the total working life of the plant into account, this approach to plant design could represent a very sound investment. It is also likely that it would add to overall safety of the plant. This aspect of plant design and construction is no doubt already being examined by the industry. But every encouragement should be given to further development.

207 Also it is essential that the management structure should be so organised that the feedback from the bottom to the top should be effective to ensure not only that instructions given are effectively carried out (although that is essential) but

(a) that persons given certain responsibilities are competent to carry out those responsibilities,

(b) that top management has a clear understanding of the responsibilities of individuals and the magnitude and type of demand made upon them, and

(c) that top management has a clear knowledge and understanding of the total work load placed on each individual in relation to his capacity. Even good and competent individuals have increased potential for errors of judgement when overworked. Also, in times of crisis and extreme demand it is easy to overwork the willing horses some of whom may not know their own limitations.

The serious errors in the design and installation of the by-pass could well have been avoided with a better system of feedback and controls, and a greater awareness of the matters mentioned in (a) (b) and (c) above.

Immediate lessons to be learned from the disaster

208 The lessons under this heading fall into three types: specific, general and miscellaneous.

Specific lessons

Integrity of plant

209 The disaster was caused by the introduction into a well designed and constructed plant of a modification which destroyed its integrity. The immediate lesson to be learned is that measures must be taken to ensure that the technical integrity of plant is not violated. We recommend:

(i) that any modifications should be designed, constructed, tested and maintained to the same standards as the original plant.

(ii) that all pressure systems containing hazardous materials should be subject to inspection and test by a person recognised by the appropriate authority as competent after any significant modification has been carried out and before the system is again brought into use.

(iii) that existing regulations relating to modifications of steam boilers which do not apply to pressure systems containing hazardous materials should be extended so as to apply to such systems. In framing such regulations consideration will no doubt be given to the question whether the inherent nature of the material requires greater control than applies in the case of steam boilers.

(iv) that the British Standard referring to the pressure to which pipe work should be tested (see paragraph 73 above) should be clarified. At present it is ambiguous in its reference to testing to $1 \cdot 3 \times$ the "design pressure". It should be made clear whether, "design pressure" means "maximum operating pressure" or "relief valve pressure" and if the former that a test to above relief valve pressure is always required.

(v) that compliance with the British Standard requirement for hydraulic testing (paragraph 73 above) should be obligatory. The by-pass was tested pneumatically to 9 kg/cm^2 for leaks without any previous test at all. Had it burst there might well have been serious if not fatal injury.

Management operation

210 At the time of the installation of the by-pass the key post of Works Engineer was vacant and none of the senior personnel of the company, who were chemical engineers, were capable of recognising the existence of what is in essence a simple engineering problem let alone solving it. We consider that there are in this connection two important lessons to be learned:

(i) that when an important post is vacant special care should be exercised when decisions have to be taken which would normally be taken by or on the advice of the holder of the vacant post. This, in the present instance, would have involved the reference by senior management to Mr Hughes of the problems created by the defect in Reactor No. 5 and the design of the by-pass.

(ii) that the training of engineers should be more broadly based. Although it may well be that the occasion to use such knowledge will not arise in acute form until an engineer has to take executive responsibility it is impossible at the training stage to know who will achieve such a position. All engineers should therefore learn at least the elements of other branches of engineering than their own in both their academic and practical training.

General lessons

Nitrogen supply

211 At Nypro the safety of the hazardous processes depended upon an adequate supply of nitrogen. Evidence showed that the routine procedures were interrupted when nitrogen was in short supply due to a dependence on outside supplies and inadequate storage facilities on site. It is therefore recommended that all plants whose safety relies upon nitrogen should have nitrogen supplies which are ample to cover all contingencies. This means that there must be either adequate "in-house" supply or massive reserves.

Nitrate stress corrosion

212 The cracked Reactor R2525 initiated the sequence of events which led to disaster. Examination of the crack by expert metallurgists showed that the crack had been caused by nitrate stress corrosion. This corrosion was created because nitrate treated cooling water had been used in the past to dilute small leakages of cyclohexane from the plant. This phenomenon had not been appreciated by the management at the time the water was used and may not even yet be appreciated by other sectors of the chemical industry. Where such cooling water is used the vessels and piping should be adequately protected. The attention of industry should be drawn to the risk.

Zinc embrittlement of stainless steel

213 Many of the stainless steel pipes taken from the disaster site had suffered cracking due to a process of embrittlement caused by zinc. The zinc had come into contact with the steel whilst it was under stress and elevated temperature. The conditions under which such embrittlement can occur are described in Appendix II. It is plain that a relatively small but fierce fire can, if there is a source of zinc nearby cause a sudden catastrophic failure. It is also plain that, after any general fire such as occurred at Flixborough all stainless steel equipment must be suspect if there is such a nearby source of zinc. The quantity of zinc required for embrittlement is very small. It could, for example, be produced accidentally in the course of a welding operation during maintenance. This might leave a piece of pipe work in a condition where it could crack during a subsequent fire. Since many chemical plants contain zinc coated components, eg galvanised wire and walkways, it is important that the attention of industry should be drawn to these matters.

Creep cavitation of stainless steel

214 Creep cavitation fractures in stainless steel have been known for some time but previously it had not been generally known that these could, under appropriate conditions of stress and temperature be produced in a relatively short time. Such a fracture can be produced in a matter of minutes by a small fierce fire. (See Appendix II). Again the attention of industry should be drawn to this phenomenon.

Miscellaneous lessons

Explosions of vapour clouds in the open air

215 Although unconfined vapour/air explosions have been known to happen in other parts of the world, there is a marked scarcity of information about the conditions under which an unconfined vapour cloud can result in an explosion or what is the mechanism leading to such an explosion. We do not know to what extent it is practicable to obtain this information but if it can be obtained it would clearly be useful.

Investigation of the disaster

216 We would have been greatly assisted in the inquiry if essential records such as those relating to the pressure and temperature of the cyclohexane in circulation in the plant during the fatal shift had been preserved. It is recommended that consideration be given to installing devices or systems for recording vital plant information in a form which would survive the effects of fire or explosion. An example of such a device is the "black-box" used in aircraft.

Matters to be referred to the special committee or other bodies

Plant layout and construction

217 It was clear on the evidence that no one concerned in the design or construction of the plant envisaged the possibility of a major disaster happening instantaneously. It is now apparent that such a possibility exists where large amounts of potentially explosive material are processed or stored. This possibility must therefore be recognised when planning, designing, and constructing such plants.

218 Many suggestions were made to us as to the consequences which should follow from taking account of such a possibility. These included: the siting of offices, laboratories and the like well removed from hazardous plants; the construction of control rooms on block-house principles with an outside air and electricity supply and with heavy equipment confined to the ground floor, the surrounding of hazardous plant with blast walls, the enclosing of hazardous plant in nitrogen purged block-houses and the like.

219 We felt quite unable to go into these matters within our terms of reference and we are conscious that measures which might be desirable and suitable in one plant might be unnecessary or unsuitable in another. Nevertheless we consider that these and other suggestions which were made to us should be urgently considered by the special committee which will be able to investigate the conditions prevailing at other plants, not only in the United Kingdom but elsewhere. It is in our view of the greatest importance that plants at which there is a risk of instant as opposed to escalating disaster be identified. Once identified measures should be taken both to prevent such a disaster so far as is possible and to minimise its consequences should it occur despite all precautions.

Siting of plant

220 The question of siting of the plant as a whole was also raised. This too we regard as a matter for the special committee. Siting must depend on both the risk of disaster and the size and nature of the disaster envisaged should the risk materialise. The greater the risk of disaster and the greater the disaster which may ensue the more important it is that the plant concerned should be, as was Flixborough, away from populated areas. In the case of existing plant, present precautions should be critically reviewed and, where necessary, strengthened.

Planning procedures

'Hazards in Discussion'

221 A scheme for co-ordination between the planning authorities and the Health and Safety Executive should be devised so that the planning authorities may be advised on the safety problems involved in any proposed plant before planning permission is granted.

Emergency arrangements

222 In any area where there is a major disaster hazard a disaster plan for the co-ordination of rescue, fire-fighting, police and medical services is desirable. Although all services appeared to us to work satisfactorily at Flixborough we were specifically asked to draw attention to this need. We suggest that the special committee should give consideration to it.

Licensing storage

223 We have pointed out in Paragraph 194 (c) above that the present situation is unsatisfactory. We recommend review of the existing regulations.

Clad mild steel

224 We were alarmed by the development of the large crack in Reactor 5 which reached a length of some 6–8 feet before a leak became apparent. This growth of the crack to a substantial size prior to detection appears to have been due to the fact that the stainless steel cladding prevented the crack from reaching the inside of the reactor until the crack had become very large and therefore potentially dangerous. The question of crack propagation and detection in internally clad mild steel vessels and their protection from corrosion on the outside should be investigated as a matter of urgency.

Summary

225 Our main conclusions may be summarised as follows:

(i) The scene was set for disaster at Flixborough when, at the end of March 1974, one of the reactors in the cyclohexane oxidation train on the plant was removed owing to the development of a leak, and the gap between the flanking reactors bridged by an inadequately supported by-pass assembly consisting of a 20 inch dog-leg pipe between two expansion bellows. (See paragraphs 53–73).

(ii) The fact that the bridging of the gap presented engineering design problems was not appreciated by anyone at Nypro with the result that there was no proper design study, no proper consideration of the need for support, no safety testing, no reference to the relevant British Standard and no reference to the bellows manufacturer's "Designers Guide". (See paragraphs 56–63).

(iii) As a result of the above omissions, responsibility for which was very properly admitted by Nypro at an early stage, the assembly as installed was liable to rupture at pressures well below safety valve pressure, and at or below operating temperature. (See paragraphs 124–128).

(iv) The integrity of a well designed and constructed plant was thereby destroyed and, although no-one was aware of it, disaster might have occurred at any time thereafter.

(v) The blame for the defects in the design, support and testing of the by-pass must be shared between the many individuals concerned, at and below Board level but it should be made plain that no blame attaches to those whose task was fabrication and installation. They carried out the work, which they had been asked to do, properly and carefully. As between individuals it is not for us to apportion blame.

(vi) On the 1st June 1974 the assembly was subjected to conditions of pressure and temperature more severe than any which had previously prevailed but no higher than careful and conscientious plant operators could be expected to permit. For the attainment of such pressures and temperatures none of the Control Room staff at the time can be criticised much less blamed. (See paragraph 87).

(vii) The more severe conditions of pressure and temperature were sufficient to and did cause the assembly to rupture, and thus to release large quantities of cyclohexane. Such cyclohexane formed a cloud of vapour (mixed with air) which exploded. (See paragraph 6).

(viii) The alternative theory which was advanced before us, namely that the assembly failed as a result of a small external explosion following prior rupture of a nearly 8 inch line, although superficially credible, proved on detailed examination, to be founded on a sequence of improbabilities and coincidences so great as to leave us in no doubt that it should be rejected. There was, in our judgment no prior explosion. (See paragraph 191).

226 We have not included in the above summary any mention of lessons to be learned for we have dealt with this in detail very recently in this Report. (See paragraphs 195–216). We believe, however, that if the steps which we have recommended are carried out, the risk of any similar disaster, already remote, will be lessened. We use the phrase "already remote" advisedly for we wish to make it plain that we found nothing to suggest that the plant as originally designed and constructed created any unacceptable risk. The disaster was caused wholly by the coincidence of a number of unlikely errors in the

design and installation of a modification. Such a combination of errors is very unlikely ever to be repeated. Our recommendations should ensure that no similar combination occurs again and that even if it should do so, the errors would be detected before any serious consequences ensued.

Costs

227 Section 84 sub-section (7) of the Factories Act 1961 provides:

"The Court may require the expenses incurred in and about the investigation (including the remuneration of any persons appointed to act as assessors) to be paid in whole or part by any person summoned before it who appears to the Court to be, by reason of any act or default on his part or on the part of any servant or agent of his, responsible in any degree for the occurrence of the accident or cause of disease, but any such expenses not required to be so paid shall be deemed to be part of the expenses of the Minister in the execution of this Act".

228 At the conclusion of our hearings it was clear that, whatever we might ultimately conclude as between the 20-inch hypothesis and the 8-inch hypothesis, Nypro by their servants or agents were in some degree responsible and that we therefore had jurisdiction to require Nypro to pay in whole or in part the "expenses incurred in and about the investigation". We therefore invited submissions with regard to costs.

229 It was submitted by more than one party that "expenses incurred in and about the investigation" included the expenses incurred by all parties and thus that we could require Nypro to pay the whole or part of the expenses incurred by other parties. We reject this submission. If it were right it would follow that, in so far as such expenses were not required to be paid by Nypro, they would be deemed to be part of the expenses of the Minister and thus that the Minister must meet the costs of all parties. We consider that so to read the sub-section would be to depart from its plain meaning, namely that the expenses incurred by the Court should be met by the Minister in so far as not required by the Court to be paid by a party responsible.

230 We anxiously considered whether we should require Nypro to pay some part of the expenses incurred, for our investigations were necessarily lengthy and expensive. Nypro had at a very early stage acknowledged the shortcomings of the 20 inch assembly and with equal responsibility made no attempt to shift the blame for the loose bolts, which were the essential foundation of the 8-inch hypothesis, onto the constructors of the plant. Although these actions did not in the event lead to much saving in time they must militate against an order for costs. On the other hand Nypro advanced through Dr Cox, albeit they did not in the end support it by submission from their Counsel, the 8-inch hypothesis, which we consider to have lacked substance and to have been pursued, at times, with a lack of objectivity. The investigation of this hypothesis occupied much time and led to much expense. But for two matters we would therefore have felt it appropriate to require Nypro to pay some part of the expenses incurred. The matters which have convinced us that we should make no such requirement are:

(i) That the pursuit of the 8-inch hypothesis has led to the discovery of information concerning creep cavitation and zinc embrittlement which was not previously known and which can contribute to greater safety in the future.

(ii) Not only had Nypro incurred heavy legal costs themselves, they had also incurred and paid substantial expenses which might properly be regarded as expenses incurred by the Court and thus recoverable. But Nypro do not, however, seek to recover these expenses which include (1) the dismantling of the site so as to preserve all possible evidence and the preparation of a detailed site record containing details, in many cases with photographs, of the condition of every item of the plant, (2) the preparation of the site for the SMRE simulation tests, (3) the provision of an elaborate model of Section 25A which was of much assistance in our investigations, (4) metallurgical investigations in connection with creep cavitation and zinc embrittlement, (5) the salaries, travelling and hotel expenses of a large number of witnesses which we desired to be called before us.

231 In the light of the above matters we make no requirement that Nypro should pay any part of the expenses.

Acknowledgements

232 We would finally like to acknowledge the assistance we received from a great number of people. It is not possible to list everybody but we would mention specially the following:

1 The parties represented before us together with their Counsel, Solicitors and expert advisers.
2 The witnesses both lay and expert.
3 Sir Frederick Warner his staff and associates.
4 The verbatim shorthand writers of W B Gurney & Sons.
5 University College London, The Imperial College of Science and Technology London, The Universities of Cambridge, Leicester and Aston in Birmingham and The Open University.
6 The Safety in Mines Research Establishment and the Mining Research and Development Establishment.
7 Her Majesty's Factory Inspectorate.
8 Teddington Bellows Limited.
9 The Humberside County Constabulary.
10 The management and staff at Church House, Westminster, The Wortley Hotel, Scunthorpe, The Piccadilly Hotel, London and The Rembrandt Hotel, London.
11 BBC and Yorkshire TV.

In addition we wish to thank the Secretariat and supporting staff both in and outside the Court.

Signed

R J PARKER *Chairman*

J A POPE *Deputy Chairman*

J F DAVIDSON

W J SIMPSON

B M O'REILLY *Secretary*
11th April 1975

Appendix I Experimental and other evidence relating to rupture of 20 inch pipe and bellows assembly from internal pressure and temperature

1 In Paragraph 63 it was noted that an assembly containing two bellows is unsatisfactory, unless adequately anchored, particularly when the bellows are out of line. Figure 6 shows the primary forces acting on such an assembly consisting of a dog-leg pipe containing two offset bellows. Assuming the bellows are very flexible — and this is true to a first approximation — the only horizontal force acting across a section through each bellows is the thrust PA, P being the pressure in the bellows, and A their effective cross sectional area. The two thrusts PA in Figure 6 are parallel and separated by a distance 2e, giving a turning moment PA2e which is balanced by shear forces $F=PAe/L$ as indicated in Figure 6; 2L is the distance between the centres of the bellows. The shear forces give rise to bending moments indicated in Figure 6, ignoring the gravity effect which is small. The maximum bending moment is at the mitre joints and is PAel/L.

2 The system of forces indicated in Figure 6 may lead to failure in two ways:

(i) The shear force $F=PAe/L$ may cause "squirm" of the bellows, the form of distortion shown in Plate 11. This distortion occurs because bellows become unstable when subjected to significant shear force.

(ii) The bending moments may cause failure of the pipe by buckling at one of the mitres as shown in Plate 13.

3 In normal service, bellows would not be expected to support any shear force at all but under normal operating conditions at Flixborough PA was about 38 tonnes and F was about 2·8 tonnes. The assembly was therefore potentially hazardous but at the time of the disaster existing knowledge was inadequate to estimate reliably the pressure at which the assembly would become unstable, with the possibility of failure.

4 A programme of experimental and theoretical work was therefore undertaken which will now be described. The programme developed as the results came forward and as the complex behaviour of the pipe and bellows assembly became more fully understood. The investigations were necessarily elaborate and we are grateful to all who took part, many of whom worked long and unsocial hours.

Flixborough tests by SMRE

5 Soon after the Court was appointed, tests at Flixborough were commissioned, to simulate as closely as possible the behaviour of the original pipe and bellows assembly. For this purpose a replica bridging pipe was made, with new bellows as near as possible the same as the original. The assembly was fitted in place of Reactor 3, connecting Reactors 2 and 4, and supported on scaffold. The pipe was pressurised with nitrogen and loaded internally with a length of chain to represent approximately the liquid which was within the actual bridging pipe. The pipe was heated electrically to 155°C, the normal operating temperature.

6 Although every effort was made to reproduce the actual conditions on 1st June, there were a number of uncertainties:

(i) There was no drawing or specification for the original pipe — all records having been destroyed in the office — so it was necessary to rely partly on the memories of Nypro employees who were fully co-operative in this as in all other matters connected with the inquiry. Information about material thickness and specification was also available from the jack-knifed pipe (Plate 10). However, there were inevitable uncertainties with regard to dimensions and welding, and to quote Mr D Waterhouse, SMRE, "precise matching of properties was not practicable in the time available".

(ii) The stiffness of bellows is extremely sensitive to their dimensions and consequently there is a sizeable manufacturing tolerance on the stiffness. The stiffness of the original bellows used in the by-pass assembly will never be known. Those used in the Flixborough tests had less than average stiffness and, as will be seen later, this was an important factor.

(iii) As noted in Paragraph 68 and Figure 4, the original pipe had partial support from scaffold poles.

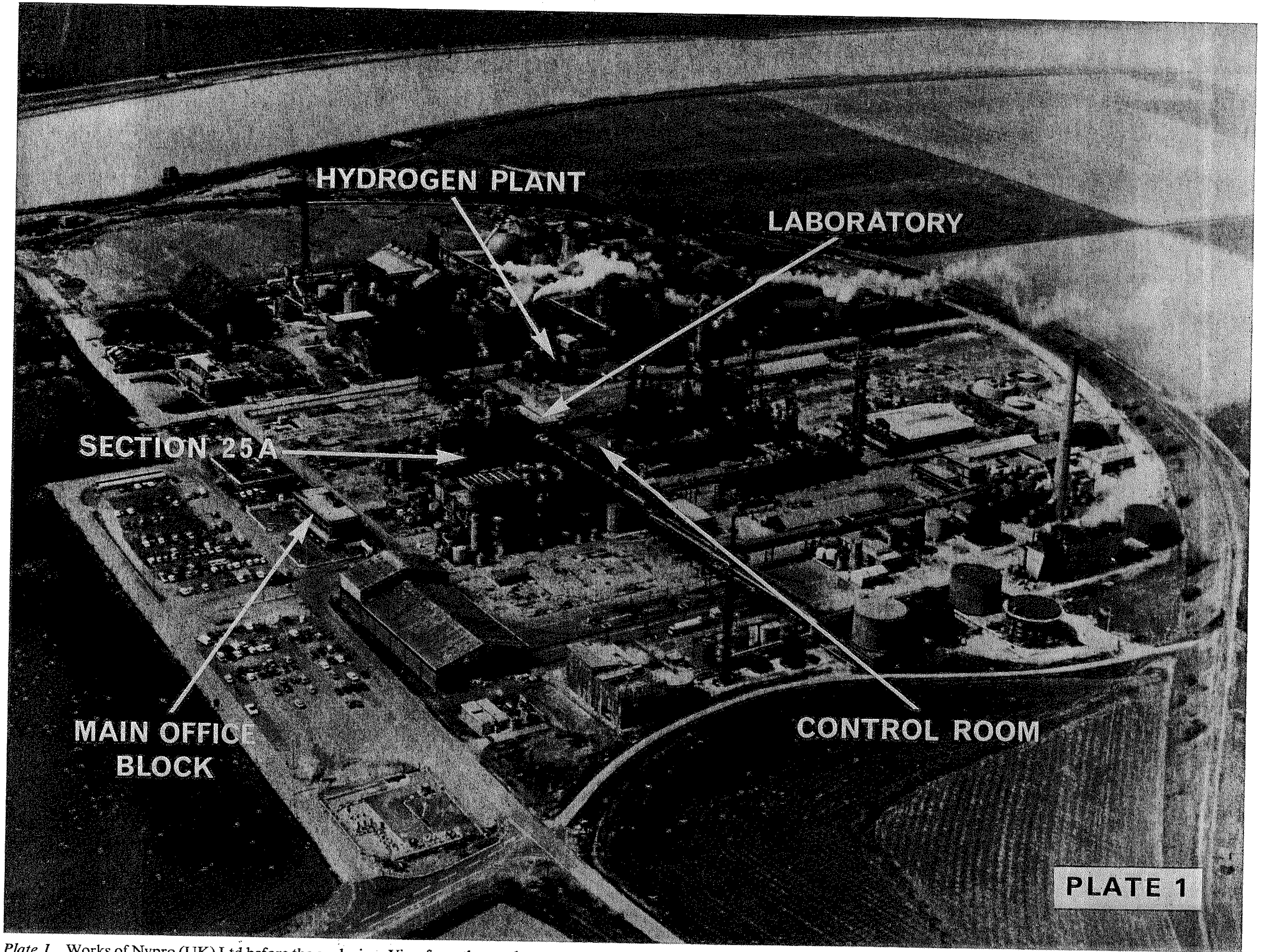

Plate 1 Works of Nypro (UK) Ltd before the explosion. View from the south-east.

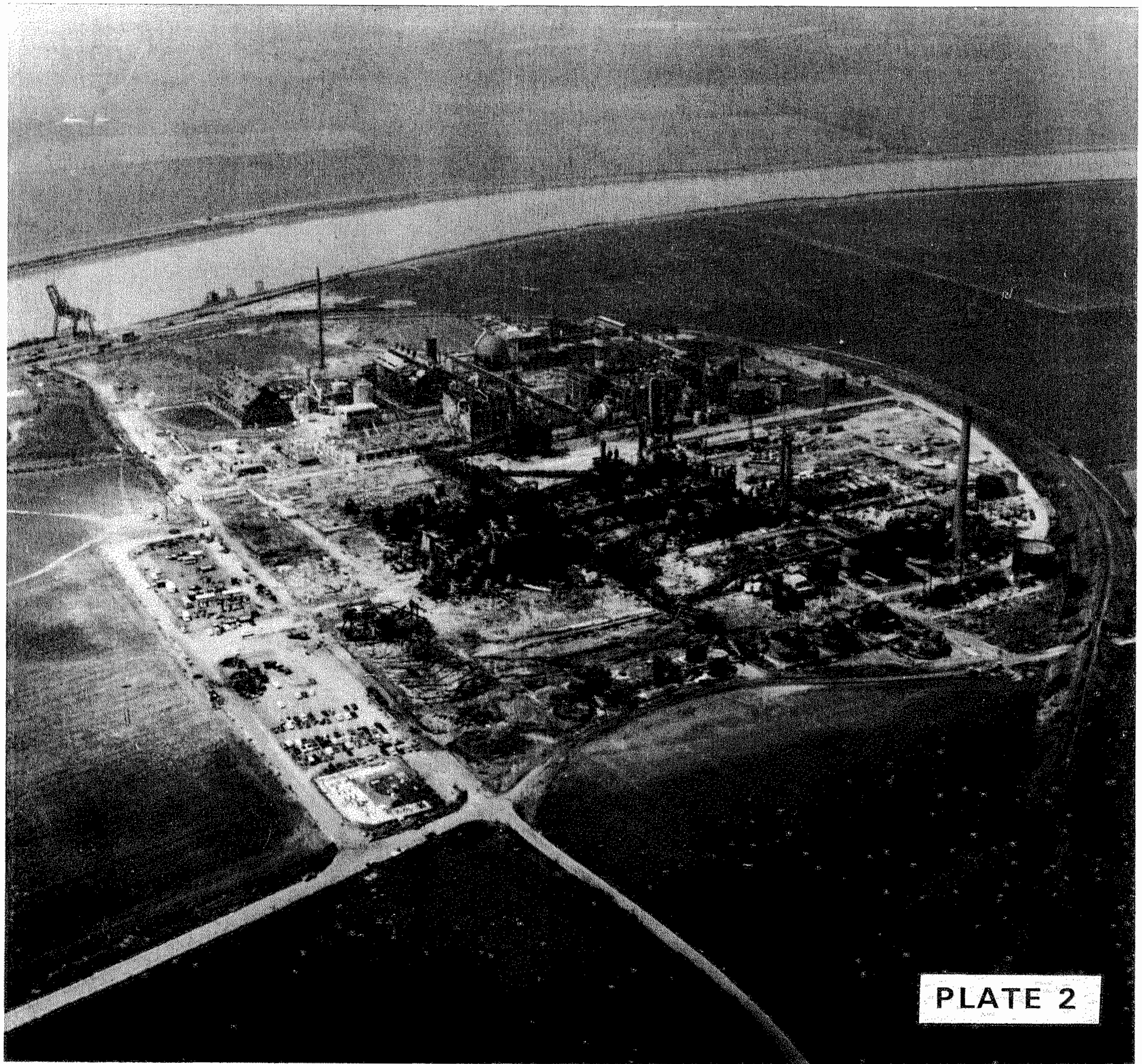

Plate 2 Works after the explosion. View from the south-east.
Plate 3 (*facing page*) Works after the explosion, seen from the north.

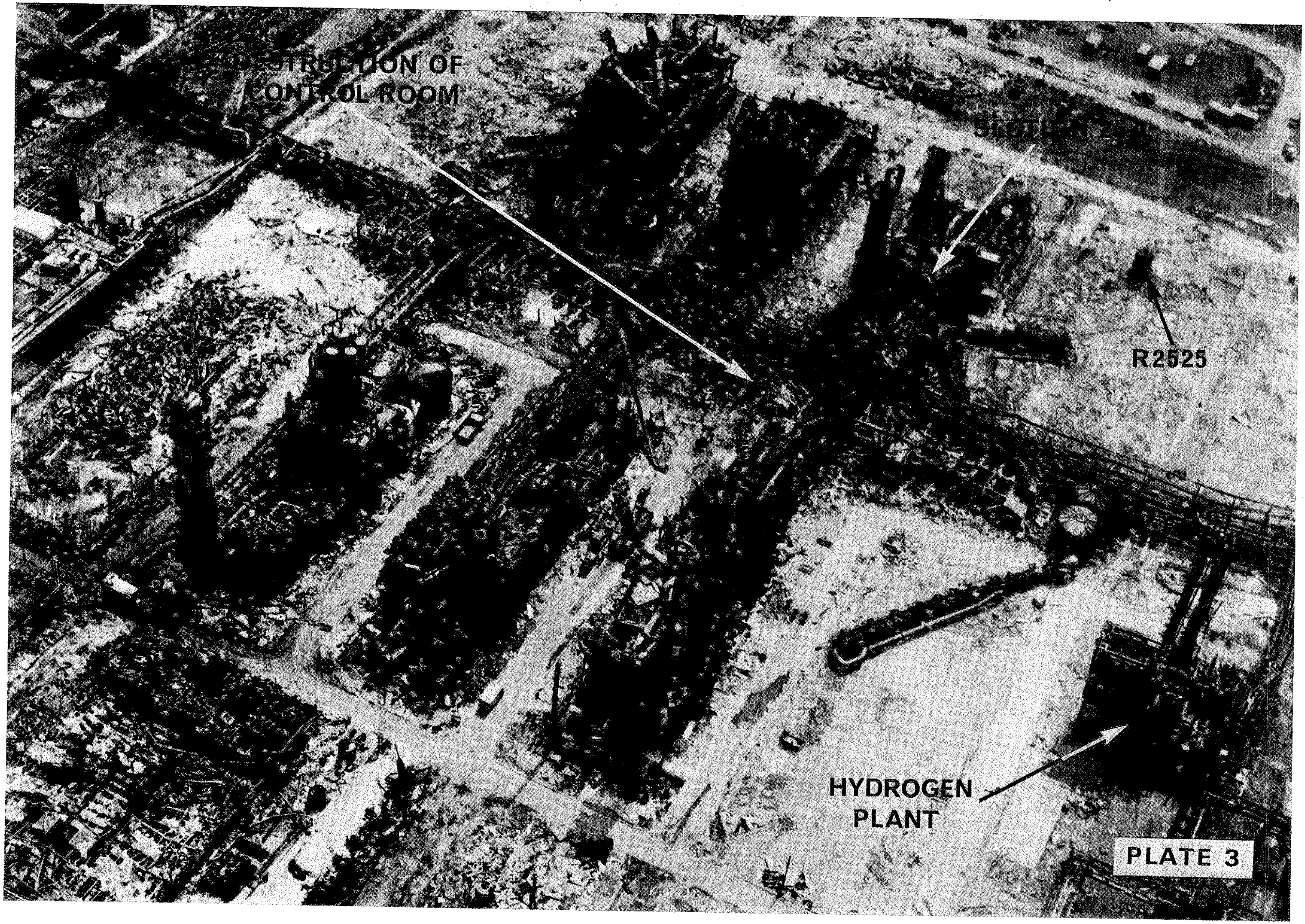

PLATE 3

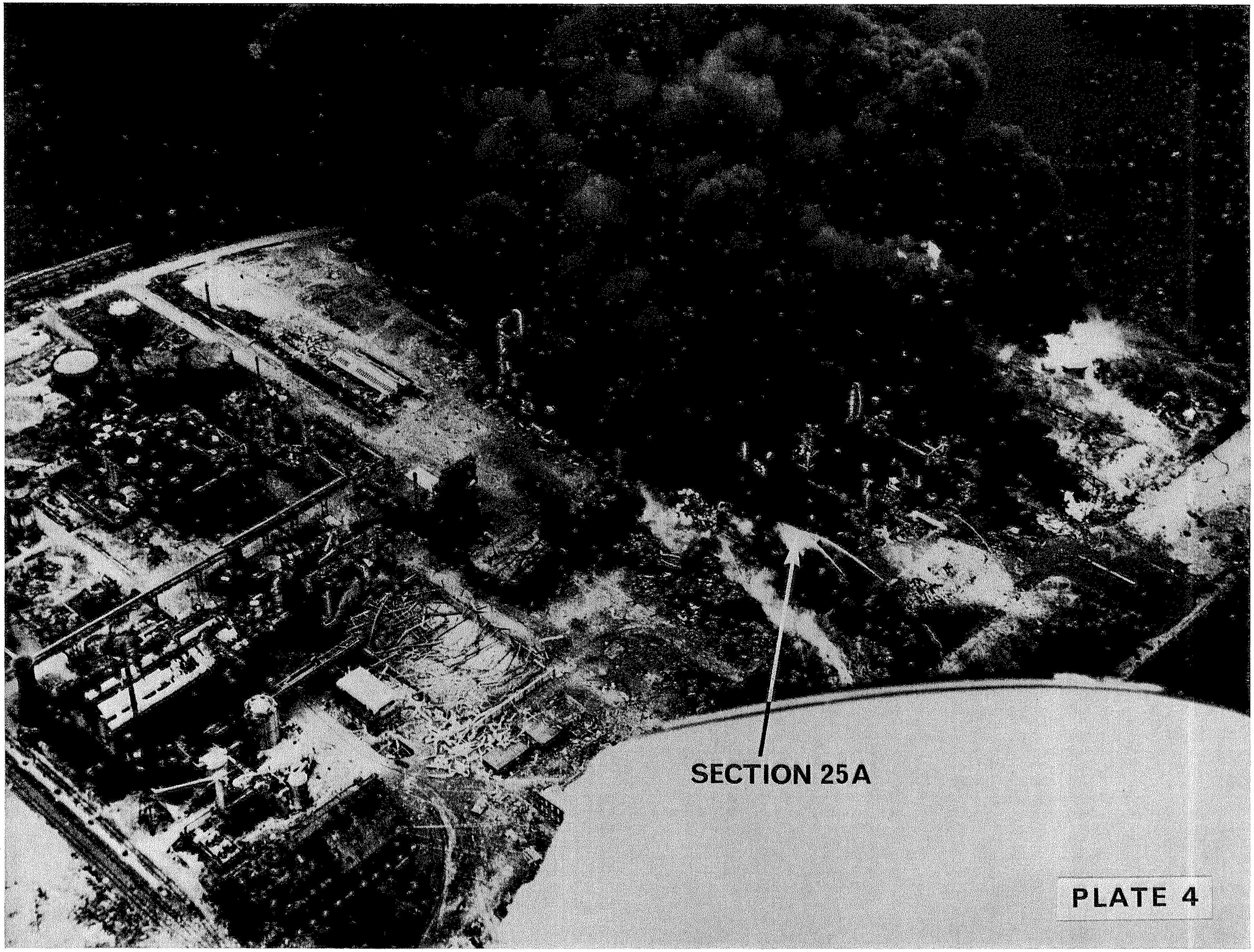

Plate 4 The conflagration, seen from the south-west.

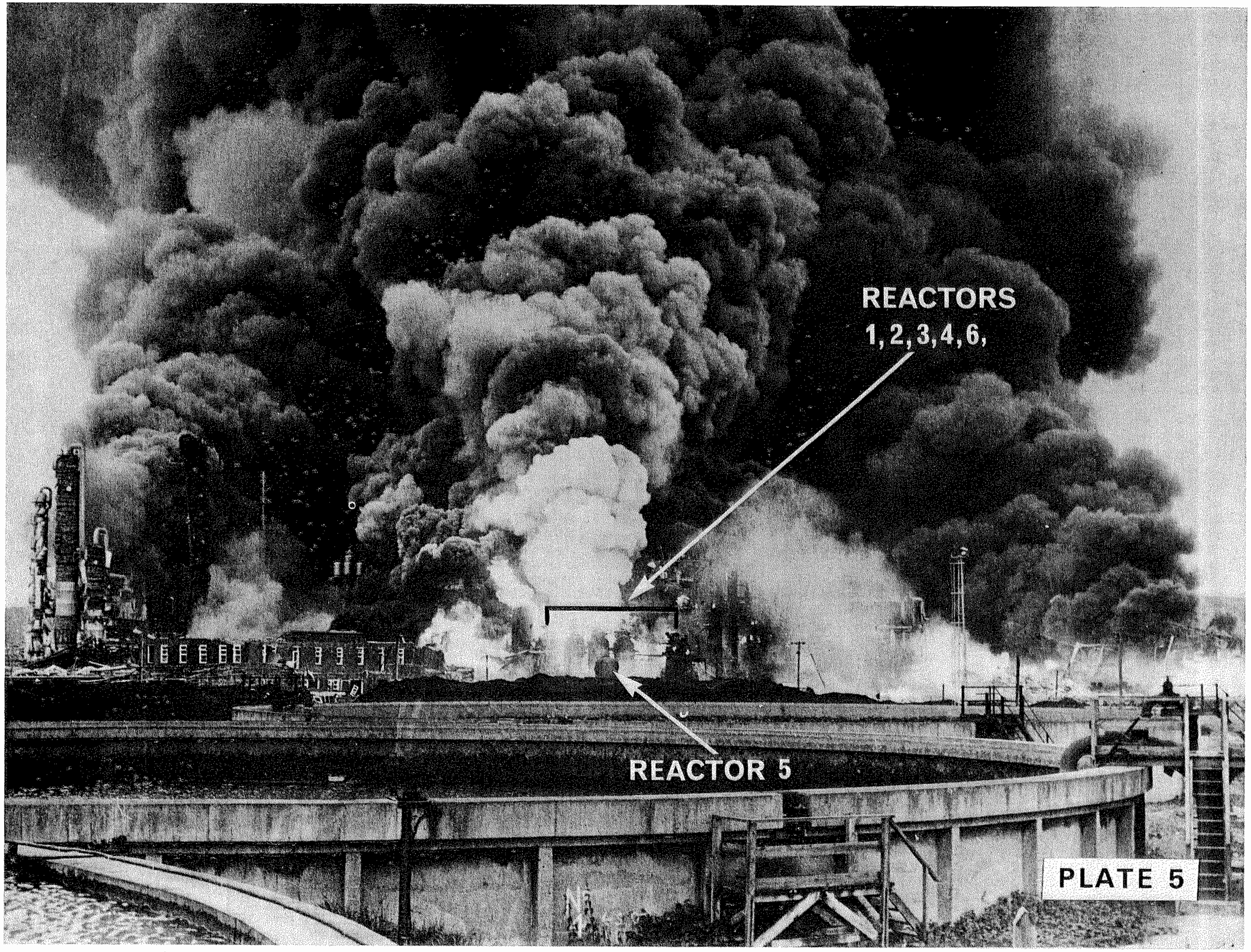

Plate 5 The fires seen from the south-west.

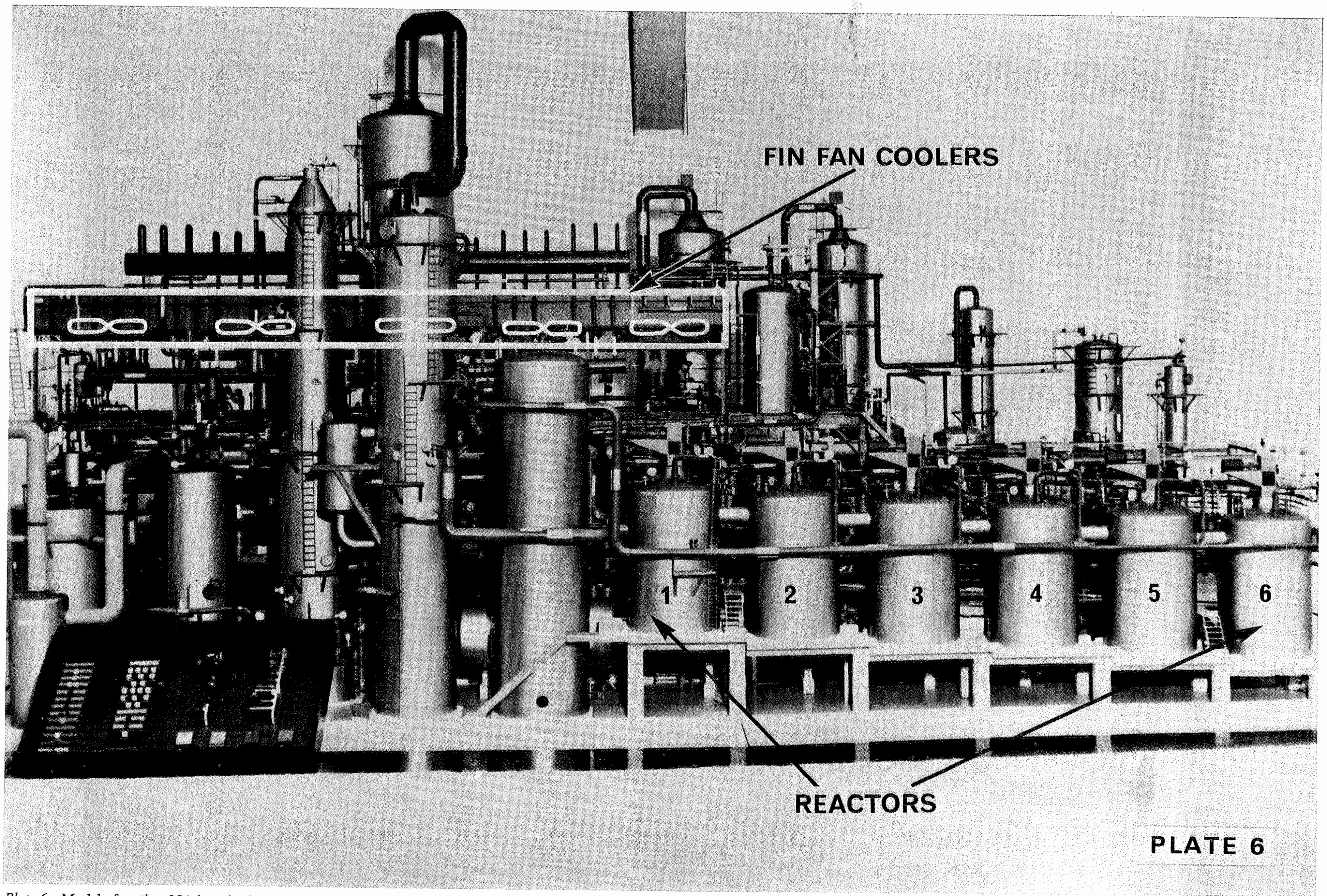

Plate 6 Model of section 25A kept in the control room and destroyed in the explosion.

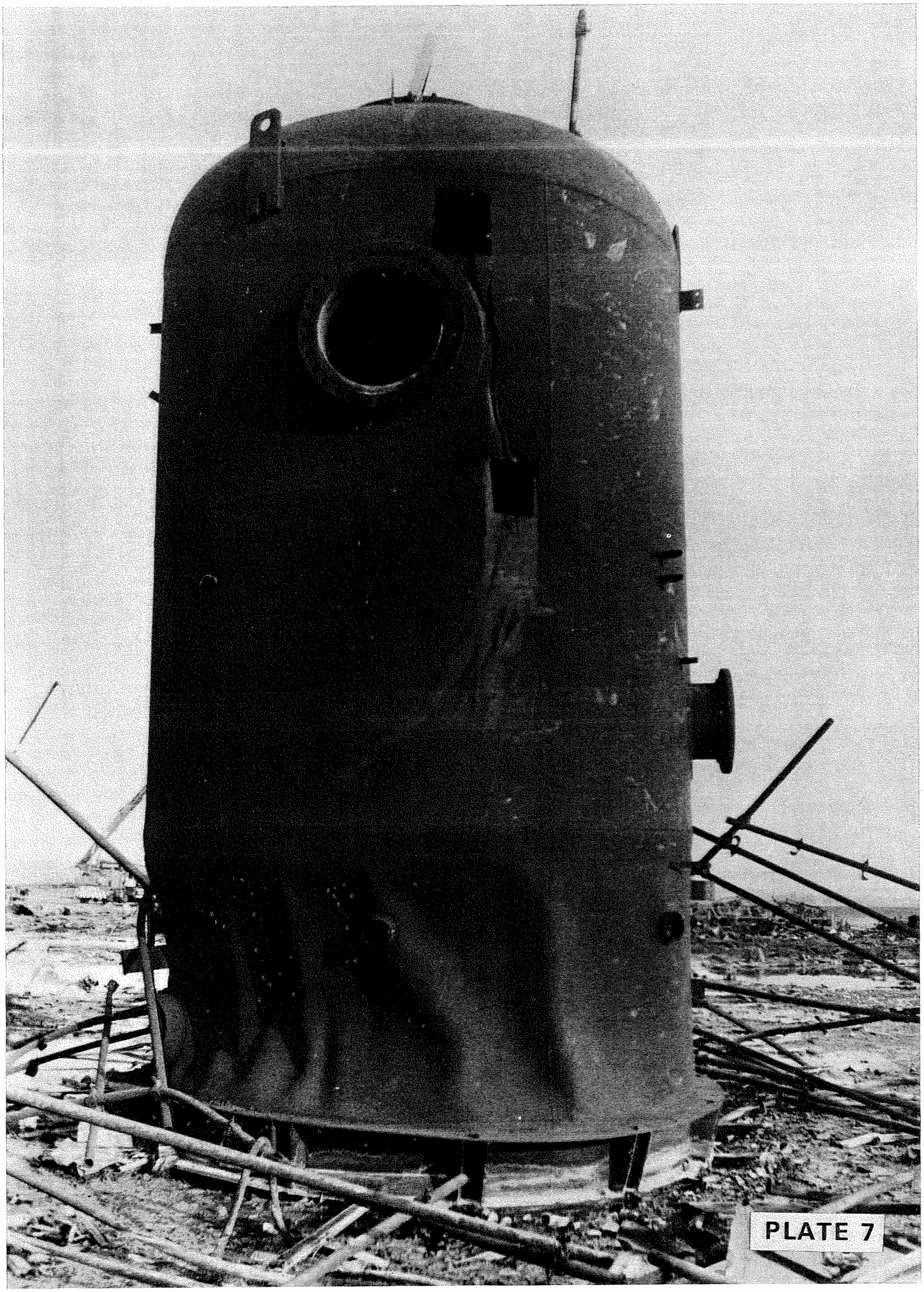

Plate 7 Reactor 5 showing crack which led to its removal and replacement by the by-pass in March 1974. The square holes are where metal has been removed for examination (crack opened by fire).

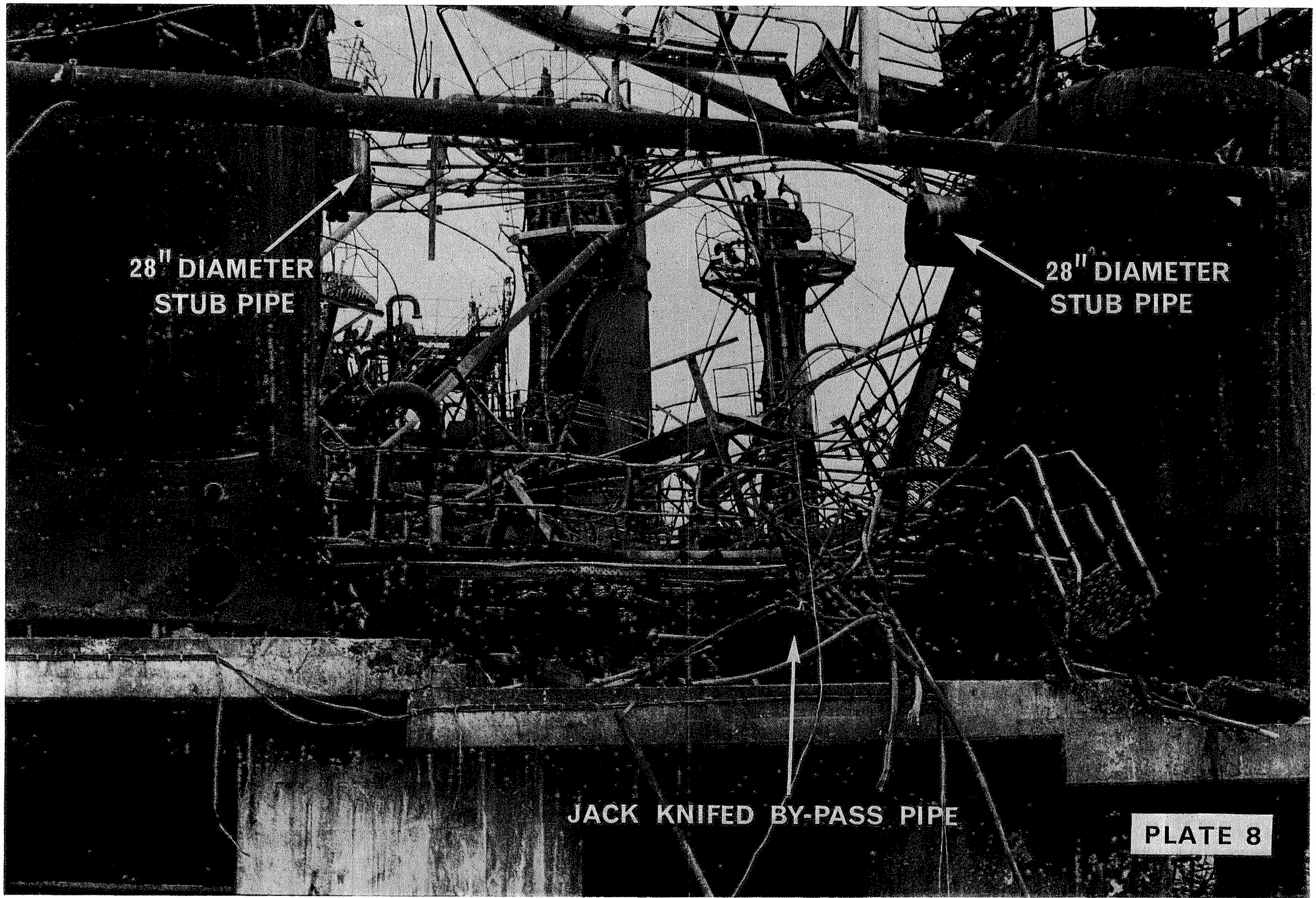

Plate 8 Reactors 4 and 6 soon after the explosion.

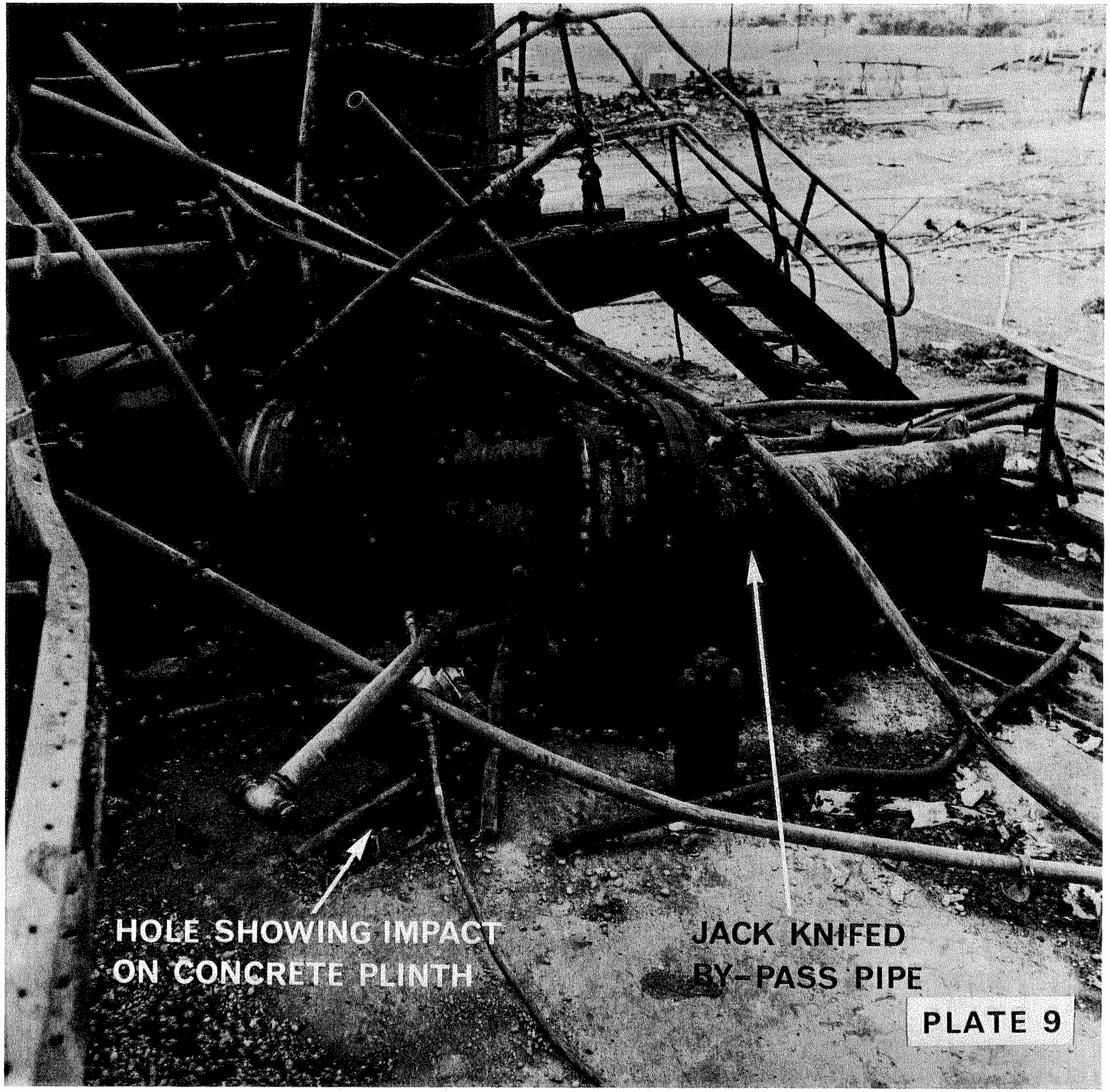

Plate 9 The jack-knifed 20 inch pipe with attached flanges and 28 inch stub pipes.

Plate 10 The jack-knifed 20 inch pipe showing trapped aluminium cladding and charred wood.

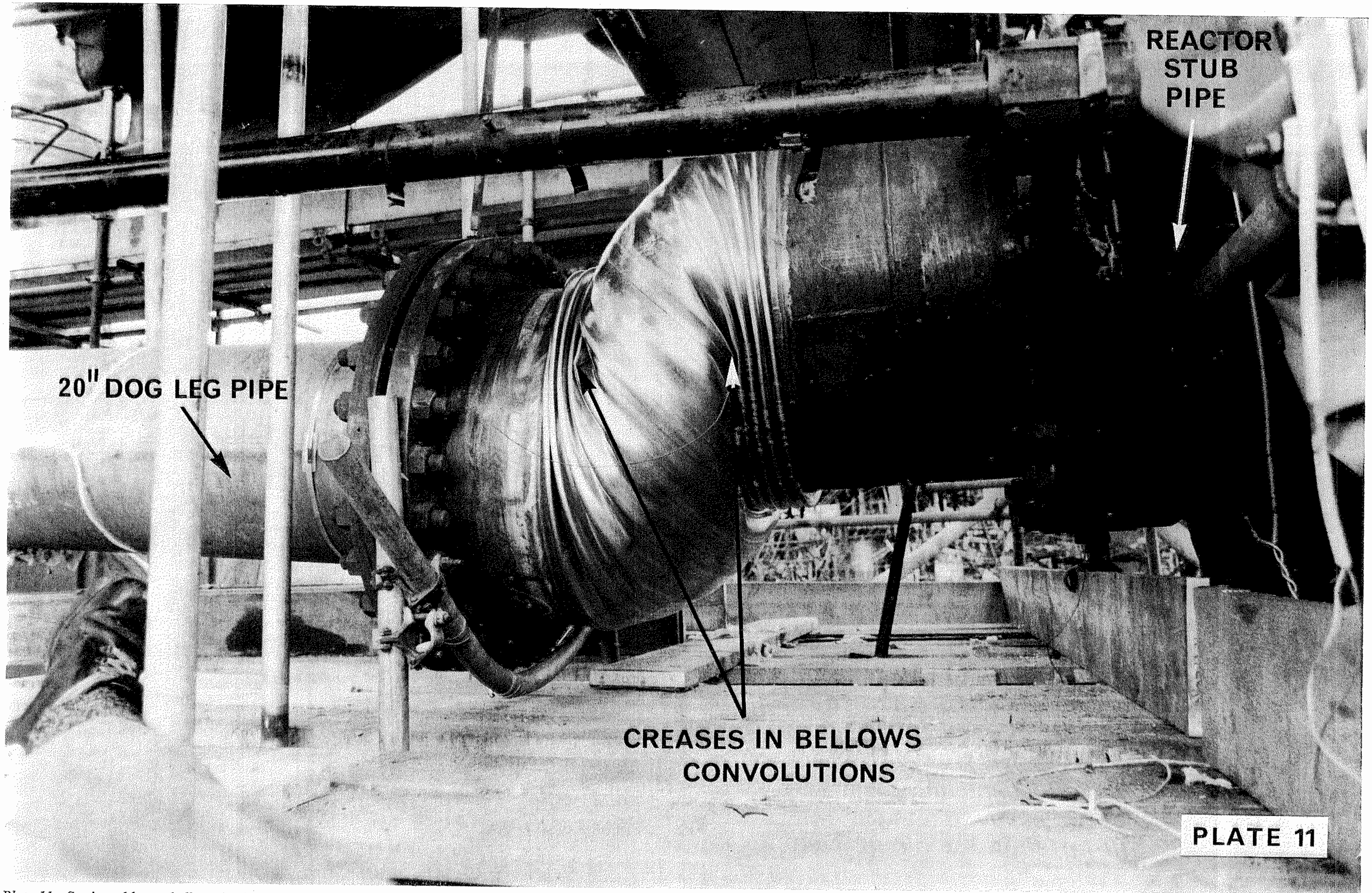

Plate 11 Squirmed lower bellows in the SMRE simulation test.

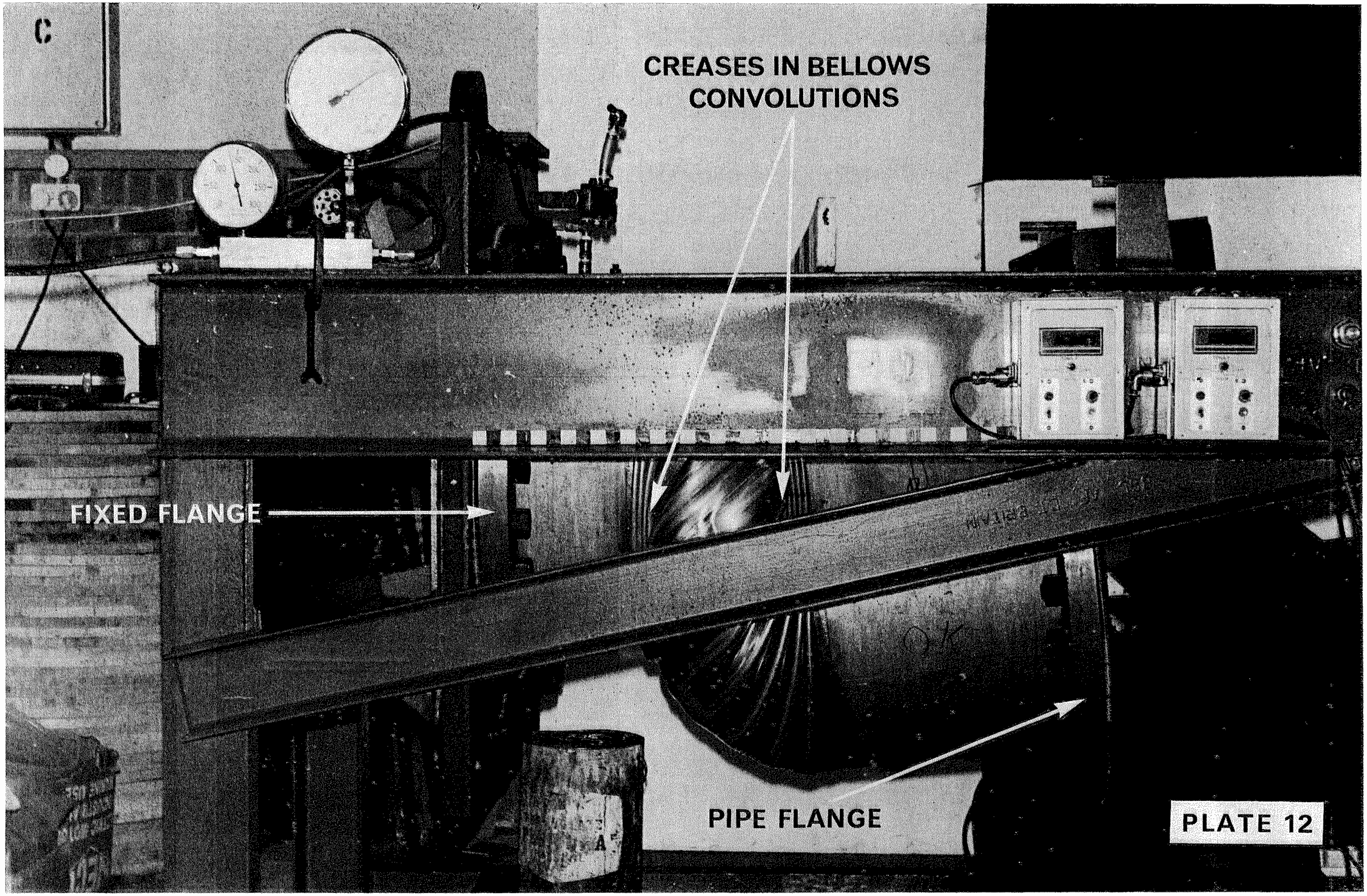

Plate 12 Squirmed bellows in the Nottingham rig. Note the similarity to Plate 11. (See also Fig. 7.)

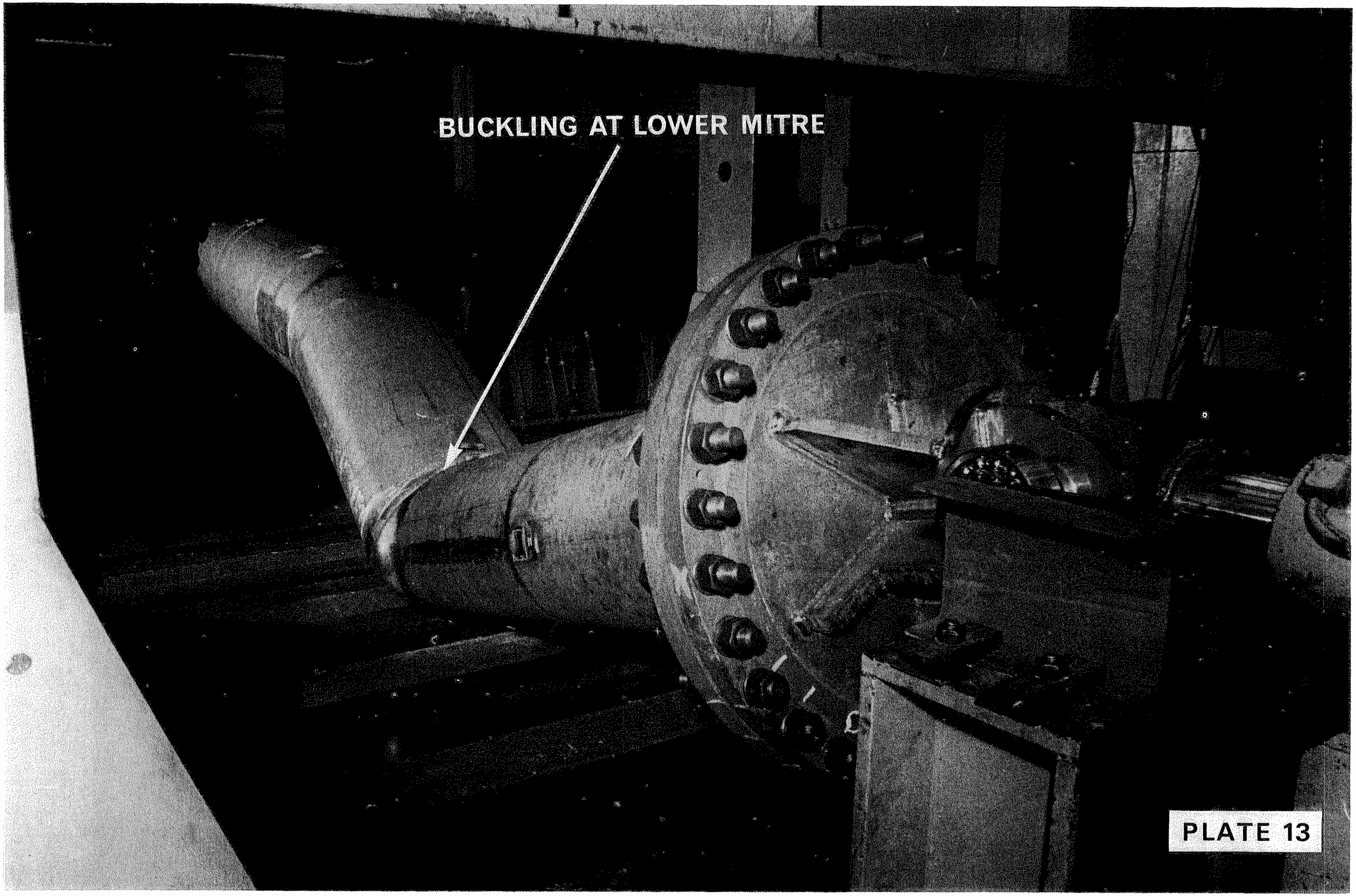

Plate 13 Buckled pipe at Bretby. Compare with Plates 9 and 10 in which the jack-knifing is complete.

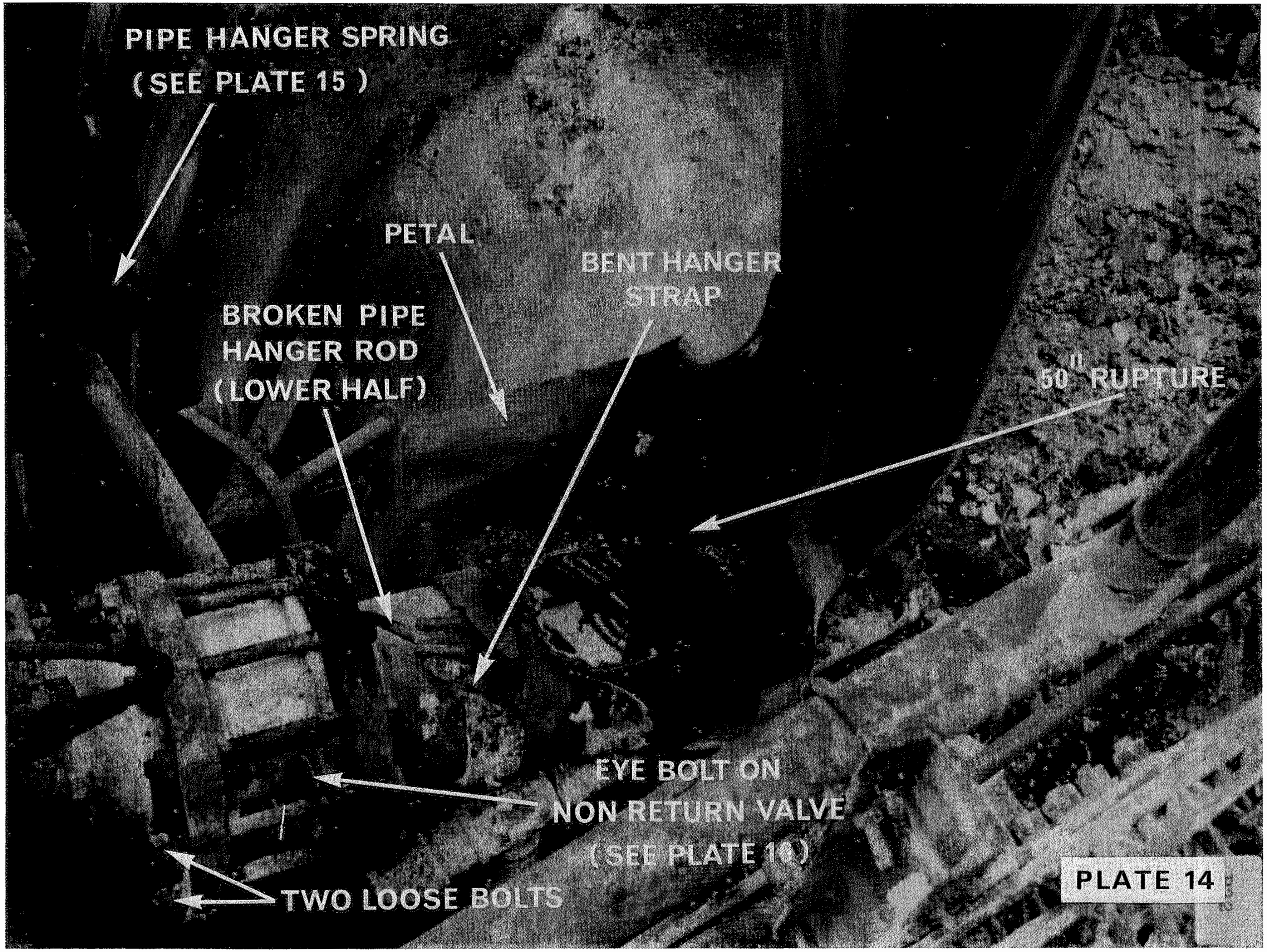

Plate 14 The 50 inch rupture on the 8 inch pipe.

Plate 15 Broken and bent pipe hanger rod, and spring.

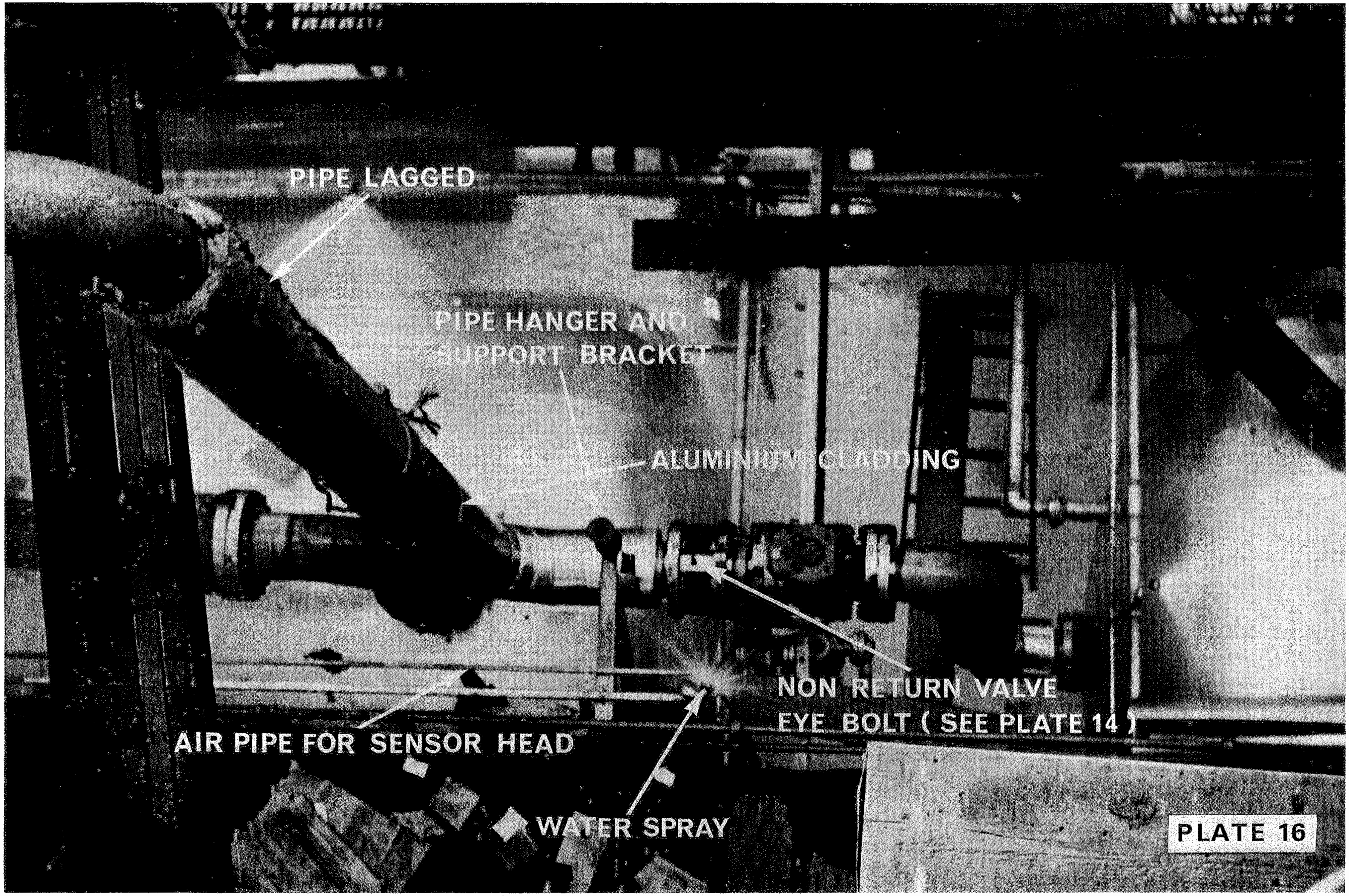

Plate 16 View of the 8 inch pipe while testing of water spray system during plant acceptance tests. (Pipe partly lagged.)

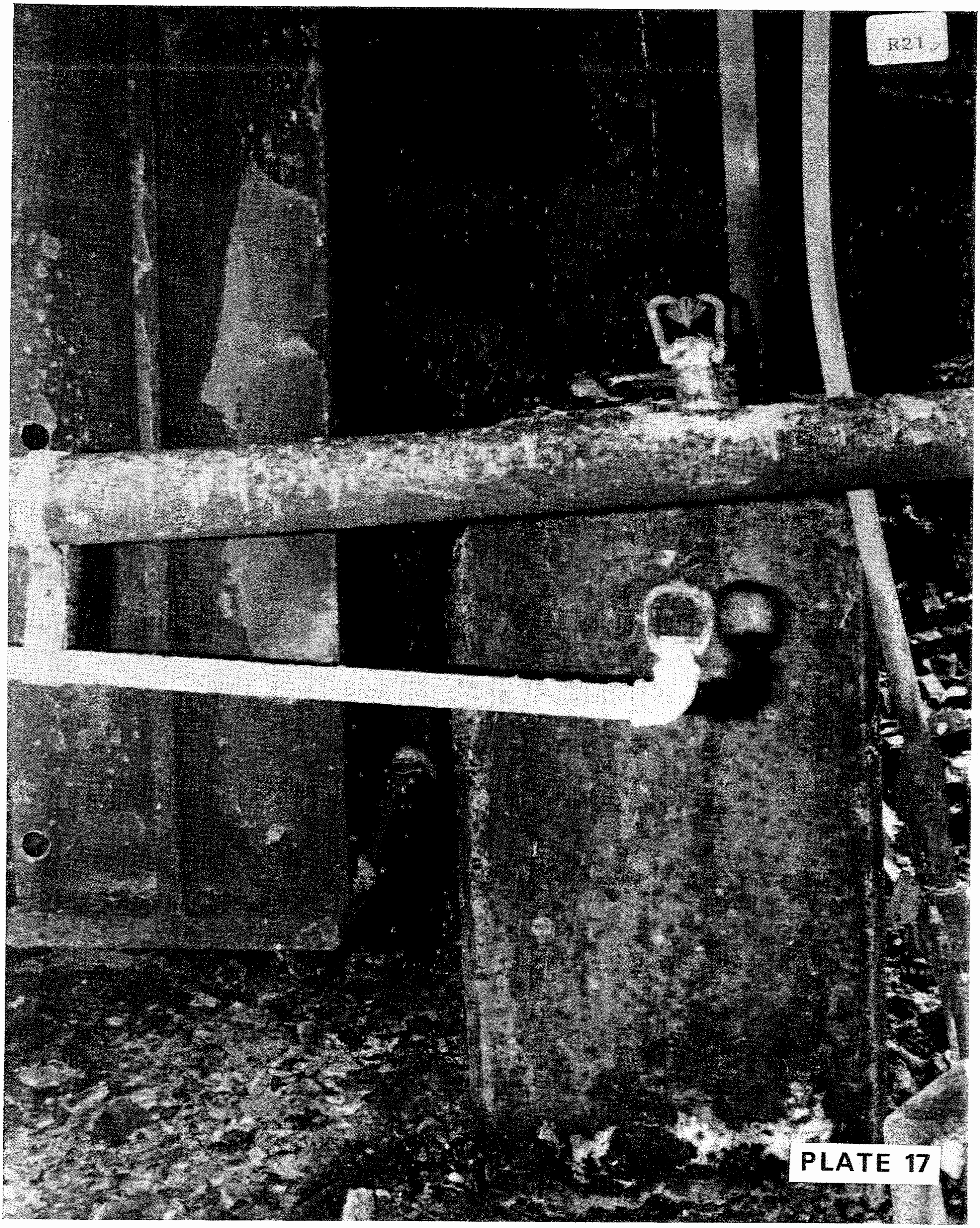

Plate 17 Typical water spray nozzle with air-operated sensor below. Photograph taken after the explosion.

Almost a week of hearings was taken up with evidence from scaffolders, fitters and laggers to determine the exact form of the scaffold because, as is usual with access scaffold, no drawing was made. However, because the hearings had not begun at the time of the SMRE tests, this evidence was not then available, and the scaffold used in the tests was different from our findings on the subsequent evidence. The principal uncertainty was in the length of the support poles under the pipe, shown in Figure 4. In the SMRE tests, a length of 43 inches was used, whereas we have concluded such length was 6 feet 6 inches. Subsequent to the hearing of scaffold evidence, measurements of bent poles from the wreckage indicated the possibility of a 43 inch span but after examining these poles we concluded that the oral evidence was more reliable. A further uncertainty was in the number of poles supporting the pipes. We have concluded that there were four poles in the position shown on Figure 4 but it is possible that five may have been used. In the SMRE tests, results were obtained both with five poles and with four. The scaffold layout on 1st June will never be known exactly and this gives an inevitable element of uncertainty to any estimate of the behaviour of the assembly.

(iv) The pipe and bellows assembly was pressurised with nitrogen, but for safety the reactors were filled with water, and blanked off to keep the pressurised gas volume to a moderate value. When the bellows distorted, their volume increased, giving a drop in pressure because of the finite volume of gas in the pipe and bellows. This meant that the tests were less severe than the actual operating conditions in which the pressure was virtually independent of change in bellows volume.

(v) In the original assembly there would probably have been locked in stresses which could not be simulated.

7 Strains and deflections at many points on the pipe were recorded as functions of pressure. There were eight test runs, under conditions summarised in Table 1.

Table 1 SMRE simulation tests at Flixborough

Test no.	Scaffold arrangement: No. of poles	Scaffold arrangement: Initial position	Temperature °C	Maximum test pressure kg/cm² gauge
1	5	In	Ambient	4·5
2	5	contact	Ambient	8·8
3	5	with	Ambient	8·8
4	5	pipe	155	8·8
5	5	Lowered ¼ inch to	155	8·8
6	4	represent expansion	155	8·8
7	4	of reactors.	160	9·8 Bellows squirm
8	4		160	14·6 Bellows burst

NB The scaffold poles supporting the bellows were moved between tests 5 and 6 so as to lie under the flames instead of the stub pipes.

8 The most important results were from tests 7 and 8. At the end of test 7, as the pressure was raised to 9·8 kg/cm² the downward deflection on the lower flange suddenly increased from about 0·75 inch to give the very large distortion shown in Plate 11; this is the phenomenon of *squirm* in which part of the bellows convolutions are pulled out and never return to the original shape. In test 7 the upper bellows "squirmed" almost simultaneously so that the axis of the pipe became permanently tilted. Although the squirm gave large distortions of the bellows, they did not rupture and were still gas tight when squirmed. Test 8 was to measure the bursting pressure of the squirmed assembly. This was 14·6 kg/cm² at which pressure the upper bellows burst.

9 There were two secondary results which had an important bearing on our subsequent investigations.

(i) *Progressive deterioration* At an early stage we considered the question of whether the structure formed by the pipe, bellows, and scaffold could deteriorate under steady loading or under the same load applied several times. Such deterioration might have explained the fact that the original assembly ran without mishap from 31st March to 29th May, and then failed during start up on 1st June.

The SMRE tests did appear to show progressive deterioration, in that at a given pressure, certain deflections and stresses showed a progressive increase from test to test, and when the pressure was reduced to zero the deflection at the end of a test was greater than it had been at the beginning. However, the theory of structures suggests that progressive deterioration will not occur: while a given load may cause permanent distortion (work hardening) the same load applied again should give no further increase in deflection. In this respect the SMRE tests were inconclusive because, as is apparent from Table 1, each of the main test runs was slightly different from the previous one: for example the number of scaffold poles and their layout was changed between tests 5 and 6.

(ii) *Pipe stresses* Many strain gauge readings were taken, and they indicated low stresses, of order 4 ton/in^2 at the maximum pressure in test 7. However, examination of published literature showed that the strain gauges had not been placed in the positions of maximum stress. Experimental evidence supported this in that the pipe became appreciably distorted during the tests, indicating high stresses in certain regions near the mitre joints.

10 In spite of these residual uncertainties with regard to deterioration, stress, and other matters mentioned above, the SMRE test results are important. The tests represent a remarkable achievement in a short time: they were complete within about 5 weeks of being requested. They established (a) that the assembly could distort grossly (squirm) without rupture following, and (b) if it did squirm without such rupture very high pressures would then be required to produce rupture.

Nottingham tests on bellows

11 Although the SMRE tests indicated the importance of the squirm phenomenon, there was a need for more information. Since bellows are not normally used with large shear loads, the manufacturers did not have relevant information and consequently a special bellows test rig was designed to obtain this information without the expense of full-scale simulation. The principle is shown in Figure 7. The objective was to reproduce the mechanics of bellows squirm on a simple rig and this was achieved by mounting a bellows with one end fixed and the other mounted on a hinged link as shown in Figure 7. The link rotated about a fixed hinge placed so that the lengths L and e were the same as on the Flixborough assembly, see Figure 6. Pressurisation gave shear force on the bellows in just the same way as in the SMRE tests and the two tests were identical provided that, in the SMRE tests, (a) the pipe was rigid, (b) the two bellows were identical and (c) the effect of gravity was small. These were important limitations but to a first approximation the Nottingham rig gave relevant test conditions.

12 Five bellows were tested at Nottingham to the point of squirm and a typical squirmed bellows in the rig is shown in Plate 12. There is a close similarity between the form of distortion in Plates 11 and 12. Moreover all the squirmed bellows showed characteristic "creases" (visible in Plate 11) which were similar to creases on the bellows fragments from the original assembly recovered after the disaster.

13 Quantitative results from the Nottingham tests were as follows:

(i) There was permanent set of the bellows after loading to about 7 kg/cm^2 or more. This showed the basic unsuitability of this application of the bellows which should normally work in the elastic range.

(ii) Pressures above 7 kg/cm^2 gave very large shear deflections, 0·1 inch or more. Although much of this deflection was permanent distortion, the bellows did not show progressive deterioration: repeated application of the same pressure gave no more deflection.

(iii) A theoretical analysis of the data by Dr G A Webster of Imperial College, London gave a method of predicting the squirm pressure, for a given bellows stiffness, and with allowance for scaffold support, and the effect of plastic yield in the bellows. This theory summarised the results of the squirm tests and enabled a prediction of the possible range of squirm pressures for the disaster bellows. This theory was most valuable, and the results agree with those of Professor Newland to whom we refer later. The range of pressures to produce squirming in the bellows tested was 11·2 to 11·6 kg/cm^2 at room temperature.

14 Three types of test on squirmed bellows were carried out at Nottingham, as follows:

(i) The pressure in a squirmed bellows was raised until a small crack in the bellows was produced. The minimum pressure required to produce this was 14·5 kg/cm^2.

(ii) An oscillatory load was applied, to see whether such a crack, which had formed at one of the creases, would propagate. No crack propagation could be obtained.

(iii) With bellows pressurised, a downward shear force was applied by a jack pushing on the pipe flange, Figure 7. This produced an effective force at the bellows of 37 tonnes without tearing the bellows, thus demonstrating that a squirmed bellows is very resistant to failure in shear.

15 At this stage it was clear that squirm could occur at or near working pressure, and that a two-stage failure of the assembly could be ruled out as described in paragraphs 98–100 of the Report.

Bretby pipe test

16 Calculations by Professor S S Gill and Dr R Kitching of University of Manchester Institute of Science and Technology, Mr G A Evans of HMFI and others predicted very high stresses near the mitres of the 20 inch pipe, on account of the bending moments indicated in Figure 6, and because of the axial stress in the pipe due to the reduction in diameter from the 28 inch bellows to the 20 inch pipe. These calculations were supported by the observations of appreciable pipe distortion in the SMRE tests. However, there was no way of predicting the buckling load of the pipe and tests were therefore arranged at Mining Research and Development Establishment, Bretby. Plate 13 shows the pipe at the end of the test. A further reason for the test was the possibility of progressive deterioration of the pipe under a few cycles of loading for which there was some evidence from the SMRE tests.

17 The pipe used was the one that had survived the SMRE test at Flixborough, albeit with a little distortion. The experiment was primarily a compression test, axial thrust Q being applied by a ram along the axis of each horizontal limb of the dog-leg, with balancing shear forces so as to give a bending moment diagram as in Figure 6. Hydraulic pressure P was applied within the pipe and the relation Q=PA was maintained throughout the tests, A being the effective area of the bellows. With this method of loading, the stress in the pipe was essentially the same as if it had been mounted between offset bellows of near zero stiffness; the Bretby test was thus a good approximation to the pipe conditions at Flixborough.

The test was at ambient temperature. In the laboratory available, a test at 155°C under hydraulic pressure would have been unacceptably dangerous.

18 The results were as follows:

(i) The relation between pressure and mitre deflection was linear up to a pressure of about 15 kg/cm^2; for higher pressures the deflection increased markedly but was only about 0·25 inches at the maximum pressure the pipe would sustain, 18·5 kg/cm^2 gauge; this was taken as the buckling pressure of the pipe at ambient temperature. At 155°C the buckling pressure would have been less, because the strength and modulus of stainless steel diminish as temperature rises. The reduction in buckling pressure may be of order 20% though a larger reduction was suggested. However it seems likely that the buckling pressure at 155°C would be above safety valve pressure 11 kg/cm^2.

(ii) Before the pipe buckled, a small number of cycles of repeated loading showed that prior to reaching 18·5 kg/cm^2 there was no significant progressive deterioration. At these lower pressures the behaviour was similar to that of the bellows: the first application of a given pressure between 17 and 18·5 kg/cm^2 caused some permanent set, but repeated application of the same pressure gave no further observable deflection.

(iii) The post-buckling behaviour — when the pipe had very large deflections as shown in Plate 13 — did show progressive deterioration in that the pipe sustained a progressively diminishing thrust as the deflection increased. It should be noted that in the post-buckling regime, the deflections were so large that it was not possible to maintain the simple relation Q=PA between thrust Q and pressure P; the thrust was applied with a reduced internal pressure.

Dynamic failure of bellows and pipe

19 From the Flixborough, Nottingham and Bretby tests it appeared that jack-knifing and rupture following squirm at or near normal operating pressure and temperature must be excluded unless there was some mechanism not reproduced in any of the experiments which would lead to such a result. Professor Newland suggested a dynamic mechanism as follows:

(i) The lower bellows would squirm at a pressure between operating and safety valve pressures. The lower bellows would squirm in preference to the upper, because gravity — acting on the pipe and the liquid within — would cause the shear force to be greater on the lower bellows than on the upper bellows.

(ii) The increase of bellows volume, due to squirm, results in the vapour within the reactors doing work on the bellows. This mechanical work appears as kinetic energy of the pipe which therefore swings round clockwise (as seen in Fig. 6) with an increasing velocity as the bellows squirm.

(iii) When squirm of the lower bellows is complete, the lower flange is suddenly brought to rest by the squirmed bellows, as in Plate 11; but the pipe is now moving rapidly downwards and its own momentum keeps it going so that there is inertia loading on the pipe, as well as the static loading due to internal pressure.

(iv) Squirm of the upper bellows follows, and the motion may then stop, leaving two squirmed bellows and a pipe somewhat distorted as a result of the loading mentioned in (iii).

(v) Alternatively, after both bellows have squirmed, distortion of the pipe may continue, the lower mitre acting as a hinge in the same way as in the final stages of the Bretby test. (See Plate 13).

(vi) The continued hinging of the lower mitre would extend both bellows: the resulting increase in bellows volume would give a very large input of work to the assembly since the term (reactor pressure) $\times$(bellows volume change) is large. This mechanical work generates kinetic energy of the flanges and at the conclusion of bellows extension Professor Newland estimated that each flange of the 20 inch pipe would be moving towards the other at about 40 mph.

(vii) This motion of the flanges would then tear off the bellows, letting out cyclohexane.

(viii) The flanges would continue to approach one another, jack-knifing the pipe which would then be propelled downwards.

(ix) The downward motion of the jack-knifed pipe would be combined with rotation so that the assembly would strike the plinth with high velocity causing the observed damage to the plinth and pipe flange. (Plate 9).

20 On this basis there are two alternatives:

(a) The lower and upper bellows may squirm in succession, distorting the pipe but not causing it to fail, so that step (iv) ends the sequence. This is what happened in the SMRE test No. 7 at Flixborough, leaving two squirmed bellows and a slightly distorted pipe (See Plate 11); such behaviour is to be expected with soft bellows, and the simulation bellows were soft, as noted above. (See paragraph 6 of this Appendix.)

(b) Squirm may be followed by continued jack-knifing, so the whole sequence is completed to (ix).

21 Professor Newland's suggested dynamic mechanism is based primarily on an energy balance, comparing the work put into the squirming bellows by the internal pressure with the energy needed to distort the bellows and buckle the pipe. In his calculations the energy terms were obtained from the Flixborough, Nottingham and Bretby tests so the analysis correlates all the experiments done on the bellows and pipe.

22 Table 2 below gives the most important results obtained by Professor Newland.

Table 2 Pipe and bellows assembly. Results of Professor Newland's analysis of pipe and bellows dynamics

(1)	(2)	(3)	(4)	(5)	(6)
Length of scaffold poles (inches)	**Approximate pressure for squirm**		**Pressure for jack-knifing to be a consequence of squirming**		
	Bellows axial stiffness (lbf/in.)		Probability		
	2800	3300	low	50%	high
43	9·7	11·3	9·5	10·1	10·8
78	8·8	10·3	9·3	9·9	10·6

Pressures in kg/cm² gauge at 150°C.
For 168°C, the pressures would be reduced by about 3%.

Columns 2 and 3 give the pressures for squirming of the bellows. Columns 4–6 give the pressure for catastrophic jack-knifing to occur following squirm. For example with a 2800 lbf/in. bellows and 78 inch poles, the bellows would squirm at 8·8 kg/cm², but squirm would probably not be followed by catastrophic jack-knifing; this is alternative (a). But with a 3300 lbf/in. bellows and 78 inch poles, the bellows would squirm at 10·3 kg/cm², and there would be a high probabiltiy of catastrophic jack-knifing; this is alternative (b).

In the table, a range of entries has to be given, because of the uncertainties about the original assembly. Thus the squirm pressure (columns 2 and 3) is sensitive to bellows stiffness, and 2800–3300 lbf/in. covers the expected range. In calculating the pressure for jack-knifing to follow squirming (columns 4–6), there are three uncertain quantities, namely (i) bellows stiffness, (ii) bellows volume change on squirming, and (iii) energy to deform the pipe as deduced from the Bretby data: these uncertainties give the range of probabilities shown. The results show that an assembly with soft bellows is (surprisingly) safer than one with stiff bellows. With a soft bellows, the assembly will "double-squirm" at a modest pressure; to cause rupture, a pressure well above safety valve pressure is needed. With a stiff bellows the first event is likely to be a squirm followed by catastrophic jack-knifing leading immediately to bellows rupture; this can occur at a pressure well below safety valve pressure.

23 The upshot of Professor Newland's work is that an assembly with soft bellows is likely to squirm without jack-knifing, but an assembly with stiff bellows is more likely to give squirm followed by catastrophic jack-knifing, these events occurring within a range of pressures between working pressure 8·8 kg/cm^2 and safety valve pressure 11 kg/cm^2. We accept Professor Newland's conclusions, and indeed they were accepted by all parties represented before us.

Appendix II Damage to the 8 inch pipe and related metallurgical investigations

1 This Appendix is based on papers and evidence submitted and given by:

Dr D J ARROWSMITH *University of Aston in Birmingham*
Dr J T BARNBY *University of Aston in Birmingham*
Sir Alan COTTRELL *Jesus College Cambridge*
Dr J H FOLEY *Safety in Mines Research Establishment*
MATCON *Consultants in Metallurgy & Materials Technology*
Dr C E NICHOLSON *Safety in Mines Research Establishment*
H G ORBONS *Dutch State Mines*
Dr P R SWANN *Imperial College London*
Dr J A WRIGHT *University of Aston in Birmingham*

We shall hereafter refer to them simply by their surnames.

2 We describe damage to the 8 inch pipe ("the pipe") connecting vessels S2538 and S2539 (Fig. 8) and related equipment. We also describe metallurgical investigations whose objective was to explain the observed damage.

Damage to the pipe

3 The pipe was ruptured as shown in Plate 14, and was distorted as shown in Figure 10. In addition to what the diagram shows, there was a 40° twist at elbow G, and a corresponding 40° twist at elbow K, because the pipe run from the NRV to K was virtually undistorted. This twisting is evident in Plate 14 from the position of the eyebolt which had been at the top of the NRV: thus Plate 14 shows that the bolt cage on the NRV was rotated through 40° about a north/south axis, the rotation being anti-clockwise as seen by an observer looking north.

4 There was a 3 inch axial split in the position D indicated in Figure 10. Around this split, the pipe wall had bulged out locally, opening up the split to a width of about 0·2 inch.

5 Numerous cracks were evident in the pipe in regions D, F, G, H, J and K in Figure 10. There was a single crack in one of the "petals" formed by the "tulip" of the 50 inch split.

Damage to pipe hangers, support bracket, gaskets and pipe lagging

6 The top spring hanger (Figs. 8 and 10) had suffered a shear type fracture. The lower spring hanger — shown in its original form in Figures 8 and 9 — had suffered complex damage indicated in Figure 10 and in Plates 14 and 15.

7 The hanger support bracket had rotated through 90° from its original position (Fig. 9) and its drilled end had been curled outwards (Plate 15).

8 The hanger rod, Figure 9, had fractured, leaving two fine pointed tips — one of them visible in Plate 14 — and the upper part of the rod was bent sharply through 90° (Plate 15).

9 The collar or strap that encompassed the pipe was found to have twisted (Plate 14) and slipped through 10° relative to the pipe, denting it in so doing.

10 The gaskets at the joints in the pipe and NRV were in such poor condition, due to exposure to high temperatures, that no reliable information as to their pre-disaster condition could be drawn from their final badly fire damaged state.

11 No trace of pipe lagging was evident on the pipe or on any adjacent pipes. The pipes were known to have been lagged with 2 inch thick pre-formed sections of rock wool insulation, secured by 14 swg zinc galvanised wire. The lagging and wire were then covered with aluminium cladding.

Displacement of separators

12 It was clear that some of the pipe distortion was due to movement of the separators, which was measured. S2538 moved about 7½ inches south; the north end moved about 17 inches west but the south end suffered little lateral movement. Likewise S2539 had no lateral movement at its south end, but a westerly movement at the north end of about 11 inches; it did not move north or south.

Metallurgical investigations

13 The stainless steel of the pipe was up to the standard of AISI 316L, and there was no evidence of corrosion.

Creep cavitation

14 Foley found that the 50 inch rupture in elbow G was initiated in a central 9 inch region by triple-point or w-type creep cavitation, and that the failure propagation was associated with the weld along the intrados. This suggested that torsional stresses had been small. The split was completed by plastic shear where the pipe was constrained by the strap (Plate 14), and the resulting tear gave the "tulip" or "petal" appearance.

15 Cottrell and Swann, and Barnby measured the time to failure of stressed specimens of stainless steel at high temperatures. The table is compiled from Cottrell and Swann's curves which are in reasonable agreement with Barnby's data.

Creep failure results for 316L stainless steel

	Temperature °C					
	1050	1000	950	900	850	800
Stress (kg/mm^2) to fail in 20 min.	3	4	6	9	12	17
Stress (kg/mm^2) to fail in 100 min.	2	3	4	7	9	13

16 At normal operating pressure (15 kg/cm^2) the stress at the intrados was estimated as about 7 kg/mm^2, so that for failure in 20 minutes a temperature of 900–950°C would be needed. However, if the temperature was below 900°C, 100 minutes or more would be needed for failure. The time to failure is thus very sensitive to temperature.

17 Metallographic examination showed that the w-type cavities in the SMRE specimen from the 50 inch split were remarkably like those from a specimen which failed in four minutes after reaching 950°C under a stress of 7·4 kg/mm^2 (Cottrell and Swann). Similar cavities were produced at other temperatures in the range 750–1050°C. This evidence showed that the appropriate cavities can be produced within a short time by a stress that could have occurred in the pipe.

Swelling of the pipe

18 Cottrell and Swann measured the swelling, ie the increase of circumference, and concluded that it was due to creep deformation. From the measurements they deduced what had been the maximum metal temperature at several sections of the pipe, using the assumptions:

(1) that the pressure in the pipe had been uniform, either working (15 kg/cm^2) or safety valve (17 kg/cm^2), and

(2) that the period of exposure to maximum temperature had been the same for the whole pipe.

19 From the swelling measurements, the percentage elongations were obtained and compared with results from creep deformation tests. This comparison gave the maximum temperature to which the metal had been exposed. The first analysis, using safety valve pressure gave the following:

Region (Fig. 10)	Deduced maximum temperature °C
G	845–910
C, D, I, J and upper part of E	750–815
A, B	less than 700

20 Cottrell and Swann showed that these results are relatively insensitive to the assumptions regarding the duration of peak temperature, and the magnitude of the internal pressure.

21 The assumptions in the analysis of swelling were the subject of criticism. Also, it was suggested that the tolerance on the pipe diameter had been underestimated. Nevertheless, the qualitative conclusion — that elbow G had been extremely hot and that sections C, D, I and J had been somewhat cooler — remained.

Zinc cracking

22 Orbons was the first to demonstrate, by microscopic examination, that the 3 inch split was due to contamination of the heated surface with zinc, and the presence of zinc was confirmed by Foley and

Nicholson. Orbons found that a stressed stainless steel specimen cracked within a fraction of a second when the surface was contaminated with zinc. Subsequent work by Cottrell and Swann confirmed the importance of zinc generated cracks; typical conditions for zinc embrittlement were a temperature of 800–900°C and a stress of 5·8 kg/mm^2 (the hoop stress in the pipe at working pressure), and under such conditions failure occurred within a matter of seconds.

23 Examination of the pipe by Foley and Nicholson and by Cottrell and Swann revealed that there were many "zinc cracks":

(1) *3 inch split* This was made up of a number of zinc induced cracks inclined at small angles (up to 28°) to the axis, and the average inclination to the axis was almost zero. It was concluded that the 3 inch split was due to hoop stress which must have been generated by internal pressure. The question of the bulging associated with the 3 inch split is treated below. (See paragraph 27).

(2) *Petal crack* In the eastern petal of the 50 inch rupture, a zinc induced crack was found. This crack must have formed before the 50 inch rupture because: (a) the stress necessary for cracking could only have been due to internal pressure acting prior to the rupture; and (b) there was local bulging, showing that the crack occurred whilst the pipe was under pressure.

(3) *Cracking in regions D, F, G, H, J, K* In these regions of the pipe, there were many zinc induced cracks, some of them observed by Foley to be right through the thickness. Cottrell and Swann plotted their orientations: the results indicated (a) the presence of torsional stresses at sections G, F and D, and (b) bending stresses at sections H, J and K, when the cracks occurred.

Sources of zinc

24 Possible sources of zinc suggested were (a) galvanised stairways and walkways (b) galvanised wire securing the lagging and (c) metal primer chromate paint. Source (c) was ruled out by experiments.

Zinc transfer to stainless steel

25 In a fire ravaged plant the zinc from sources (a) and (b) might be transferred to the stainless steel in many ways; relevant experiments were conducted by Orbons, by Cottrell and Swann, by Arrowsmith and by Matcon. No very clear picture emerged but:

(a) An oxide film on the stainless steel resisted zinc penetration: a crack or defect in the oxide film allowed zinc to penetrate.

(b) Oxidation of zinc droplets inhibited penetration.

(c) Applied stress was necessary for rapid zinc penetration.

(d) In most of the experiments liquid zinc was applied to a stainless steel specimen, but zinc vapour was shown to be capable of giving embrittlement.

(e) Zinc from a wire can cause embrittlement; the wire needs to be close to the specimen but contact is not essential.

(f) The temperatures of the metal and of the applied zinc are very important. Most of the tests which gave zinc embrittlement were at 800–900°C, but Arrowsmith obtained adhesion and penetration (without cracking) when a drop of liquid zinc at 650° fell on to an unstressed specimen at 200°C.

26 From the above, it is not possible to define clearly the conditions for zinc embrittlement: but for rapid attack it appears that the most favourable metal temperature is 800–900°C, and that a substantial stress is required. Zinc embrittlement does occur at low stress but much more slowly. Cottrell and Swann found that "at a stress of 1·6 kg/mm^2 failure did not occur during heating to 1050°C even though the specimen was coated on all sides by a pool of zinc".

Bulge at the 3 inch split

27 Orbons pointed out that the bulge implied plastic yielding of the pipe wall around the split, and that there must have been sustained internal pressure after the zinc embrittlement had caused the split. He generated a similar bulge by pressurising a replica pipe in which a 3 inch split had been cut. This idea was pursued by Foley and Nicholson who used theory of crack propagation to make an estimate of the combination of wall temperature and internal pressure that would give the observed bulging; with safety valve pressure (17 kg/cm^2), this gave a wall temperature of about 750°C. Cottrell and Swan used essentially the same method and came to the guarded conclusion that if there had been 17 kg/cm^2 within the pipe, the wall temperature must have been 750–920°C. This application of fracture mechanics was much criticised and the authors of the analysis readily agreed that their estimates had been only qualitative. Nevertheless the qualitative picture remained, and we accept, that (a) the pipe

must have been very hot at the time of bulging and (b) there must at that time have been some internal pressure.

Pipe hanger spring temperature

28 Foley measured the hardness of the spring from the lower pipe hanger (Fig. 9) and deduced that the metal had been at a temperature of about 750°C.

Pipe hanger rod temperature

29 The fine pointed tips of the fractured rod were indicative of tensile failure. Wright obtained a very similar fracture in a low carbon mild steel by applying a strain rate of about 10^{-1}/sec to a tensile test specimen at 920°C. We conclude that a similar temperature in the lower hanger rod had been reached at the instant of fracture.

Summary of metallurgical evidence as to temperatures

30 The table below summarises the deductions of temperatures from the methods outlined above. It should be borne in mind that these deductions depend upon certain assumptions given in the relevant paragraphs of this appendix: for example the deduced temperature at elbow G from the observation of creep failure was based on the assumptions of (a) 15–17 kg/cm² pressure (working or safety valve) and (b) that failure occurred in 20 minutes.

	Temperatures °C as deduced from				
Position on pipe (Fig. 10)	Creep failure	Swelling due to creep	Zinc cracking	Bulge	Hanger
	Para 16	Para 19	Para 26	Para 27	Paras 28, 29
A, B		less than 700			
C		750 to 815	800 to 900		
D (3 inch split)		750 to 815	800 to 900	750 to 920	
E		750 to 815	800 to 900		
G	900 to 950	845 to 910			750 spring 920 rod
H, I, J, K		750 to 815 (I, J)			

31 The deduced temperatures in the table are subject to some uncertainty but it can be concluded that much of the pipe was very hot — perhaps over 800°C — and that there were local high temperatures, of the order of 900–950°C, in the region of elbow G and at the pipe hanger rod.

Appendix III Procedural history

1 The Court first met privately on the 2nd July and then assumed control of the site.

2 A preliminary public hearing was held on 24th July 1974 at Church House, Westminster, SW1 at which the Chairman explained the terms of reference to the Court, outlined the procedure under which the Court would be conducted, and heard applications for legal representation.

3 Representation was granted to those parties as shown in Appendix IV on the ground that they or their members could be prejudicially affected during the course of the inquiry.

4 The Court then adjourned until 9th September when at the Wortley Hotel, Rowland Road, Scunthorpe, it commenced taking evidence from eye-witnesses. A total of 48 witnesses were heard in that week and on completion of that part of the evidence the hearings resumed in Church House on Monday 16th September.

5 During the course of the hearings at Scunthorpe, HMFI at the request of the Court, assumed control of the safety of the dismantling operations on the site. The preparation of a detailed record of the conditions of all items of equipment was undertaken by Nypro.

6 Hearings continued at Church House until 4th October when it was necessary to vacate those premises and move to the Piccadilly Hotel, London W1, for the week 7th–11th October.

Hearings were then adjourned until 4th November 1974 when the Court re-opened at The Rembrandt Hotel, Thurloe Place, London SW7 where they continued until 29th November. The final session of the Court was again held in the Rembrandt Hotel from 13th January to 20th February inclusive.

7 A further 125 witnesses were heard in London giving a total of 173 and the Court sat for a total of 70 days excluding the preliminary meeting.

8 Apart from the actual hearings, the Court met on several occasions to discuss the progress of the inquiry and also made visits to the Flixborough site and to various establishments where experimental work was being carried out.

9 Technical experts were requested to agree on as many technical matters as possible before the hearings commenced. To this end Messrs Cremer & Warner, Consultants to the Court, arranged meetings with the parties and persons carrying out the technical investigations.

10 When the inquiry commenced its public hearings, there was still a great deal of investigation to be done and it was formally decided that a Management Committee, comprised of the technical experts and advisers of the parties represented should be established.

11 This Committee met formally at Scunthorpe on September 9th under the chairmanship of Sir Frederick Warner. Further meetings were held to discuss the progress of investigations, the results from completed tests and the points where further investigations were required. Sub-committees were established to examine various aspects of the investigation and report back to the main committee. By this method it was possible for many of the technical problems to be clarified, for areas of differences between experts to be defined and for programmes of experimental work to be devised.

On one occasion a meeting was under the chairmanship of the Deputy Chairman of the Court. This meeting was concerned to a large extent with the design of the test rig for the tests subsequently carried out at Nottingham and the programme of testing there to be carried out.

12 By means of the Management Committee the Court was released from the burden of supervising the technical work and at the same time it allowed agreement between the experts to be noted without the necessity of formal proof.

13 Although this procedure at times caused difficulties it is mentioned because it is believed that it is one which could usefully be used — no doubt with variations — in the future.

14 Where an inquiry is such that, as the evidence proceeds, the need for new lines of investigation or experiment emerge, a method for enabling experts to discuss and if possible agree on the basic parameters of experiment, the nature of test programmes and the areas in which there is or is not room for difference is invaluable. The Court cannot, of course, be bound by any agreement reached between the experts but it is considered that in complex inquiries of a like nature to the present some such procedure will probably be required.

Appendix IV List of representations

Mr T H BINGHAM, QC, and Mr M HOWARD (instructed by the Treasury Solicitor) appeared as Counsel for the Court of Inquiry.

Mr K G JUPP, QC, and Mr J O ROCH (instructed by Mr RONALD V COWLES) appeared on behalf of Nypro (UK) Ltd.

Mr A P LEGGATT, QC, and Mr H W B PAGE (instructed by Clifford Turner & Co) appeared on behalf of Dutch State Mines.

Mr R J KIDWELL, QC, and Mr G M HAMILTON (instructed by Metson, Cross & Co) appeared on behalf of Sim-Chem Ltd.

Mr B J DAVENPORT (instructed by the Solicitor's Department) appeared on behalf of the Department of Employment.

Mr J D A FENNELL, QC, and Mr J B GOLDRING (instructed by Hett, Stubbs & Kemp) appeared on behalf of the personal representatives of deceased.

Mr G M LIGHTFOOT and Mr D SEARBY (instructed by A Maurice Smith & Co) appeared on behalf of British Association of Colliery Management.

Mr P R PAIN, QC, and Mr I G A HUNTER (instructed by John Latham & Co) appeared on behalf of the Transport and General Workers' Union.

Mr P R PAIN, QC, and Mr I G A HUNTER (instructed by Mr Clive Jenkins (Gen. Sec.) and Mr S Davison (Assist. Gen. Sec.)) appeared on behalf of the Association of Scientific Technical and Managerial Staff.

Mr A V de C du SAUTOY appeared on behalf of Humberside County Council.

Mr K A CAMERON (instructed by Joynson Hicks & Co, Sharpe, Pritchard & Co, and R A C Symes & Co) appeared on behalf of the Parishes of Amcotts, Burton-upon-Stather, Flixborough, Boothferry, Glanford & Scunthorpe.

Appendix V List of witnesses

ADAMS David
Chemist, Nypro (UK) Ltd

ARTINGSTALL Dr Graham
Head of Fluid Mechanics Section, SMRE

ARMATAGE Lawrence
Welder

ARROWSMITH Dr David John
Reader, University of Aston in Birmingham

AYRE Eric
Weighbridge Operator, British Steel Corporation

BAKER James Alfred
Fitter's Mate, Industrial Plant Services

BALL Prof John Jeffrey
Head of Department of Metallurgy & Materials Science, Imperial College London

BARRY Liam
Fitter's Mate, Industrial Plant Services

BARKER Geoffrey Lesley
Chemist, Nypro (UK) Ltd

BARNBY Dr Joseph Terence
Reader in Materials Technology, University of Aston in Birmingham

BARTLETT David Charles
Chemist, Nypro (UK) Ltd

BASS John Edward
Carriage and Wagon Examiner, British Rail

BASS Mrs Kathleen
Housewife,

BATSTONE Dr Roger James
Consultant, Cremer and Warner

BECKERS Johannes Hubertus
General Works Manager, Nypro (UK) Ltd

BEECH John
Plant Operator, Nypro (UK) Ltd

BELL Cyril Leslie
Project Liason Manager, Nypro (UK) Ltd

BLACKMAN Albert Brian
Area Engineer, Nypro (UK) Ltd

BOOTLAND Alan
Instrument Inspector, Nypro (UK) Ltd

BOOTLAND Trevor
Instrument Technician, Nypro (UK) Ltd

BOWES Philip Charles
Principal Scientific Officer, Fire Research Station

BOYNTON Brian Thomas
Services Engineer, Nypro (UK) Ltd

BRENNER Eric
Safety and Training Manager, Nypro (UK) Ltd

BURKE Arthur
Assistant Engineering Technician, Nypro (UK) Ltd

BURTON Edmond
Laboratory Shift Leader, Nypro (UK) Ltd

BURTON Roy
Maintenance Fitter, Industrial Plant Services

CABORN Kerry James
Process Operator, Nypro (UK) Ltd

CARTER Barry
Electrical Design Draughtsman, Nypro (UK) Ltd

CHAFER William Oliver
Locomotive Driver at Flixborough Wharf, British Steel Corporation

CHARLESWORTH David
Boiler Minder, British Steel Corporation

CLEGG Peter David
Assistant Engineer, Nypro (UK) Ltd

CLIFF Peter Harry
Production Manager (Designate), Nypro (UK) Ltd

COBB Bertram
Utilities Operator, Nypro (UK) Ltd

COCKBURN John
Process Control Technician, Nypro (UK) Ltd

COLQUHOUN David Crawford
Tanker Off Loader, Nypro (UK) Ltd

CONNOR Dr John
Principal Scientific Officer, Home Office

CORRIGAN Christopher
Engineering Technician, Nypro (UK) Ltd

COTTRELL Sir Alan Howard
Master of Jesus College, Cambridge

COUSINS Philip
Maintenance Fitter, Central Engineering Ltd

COX Dr John Idris
Consultant, British Rail

CRANAGE Stanley
Chief Superintendent, Humberside Police

CUBBINSON John Stewart
Contracts Manager, Nypro (UK) Ltd

CULPIN Richard
Assistant Area Engineer, Nypro (UK) Ltd

DAWSON Kenneth
Engineering Supervisor, Nypro (UK) Ltd

DICKINSON James
Waterman at Flixborough Pump House, British Steel Corporation

DICKINSON Lloyd Arthur
Fitter, Central Engineering Ltd

DIXON David Peter
Fitter, Central Engineering Ltd

DOBBS Neville Charles
Process Operator, Nypro (UK) Ltd

DOWNING Vincent
Shift Chemist, Nypro (UK) Ltd

DRAKE Alan
Shift Superintendent, Nypro (UK) Ltd

DREWRY Dr David Thornton
Assistant Shift Superintendent, Nypro (UK) Ltd

DRIVER Stephen Leslie
Trainee Chemist, Nypro (UK) Ltd

EVANS Glynne Arthur
Engineering Inspector, HMFI

EVERTON Roger
Deputy Plant Manager, Nypro (UK) Ltd

FAWCETT Keith
George Angus & Co Ltd

FISHER David
Process Technician, Nypro (UK) Ltd

FOLEY Dr Jeffrey Harold
Head of Metallurgical Section, SMRE

FORD Michael John
Fitter, Industrial Plant Services

FROST Geoffrey
Superintendent, Nypro (UK) Ltd

FROW Colin Gordon
Engineering Supervisor, Nypro (UK) Ltd

GAMES Graham Andrew Crawshaw
Higher Scientific Officer, SMRE

GILL Professor Samuel Sidney
Vice-Principal, UMIST

GLENN Michael John
Designer, George Angus & Co Ltd

GOODCHILD Melvyn Brian
Scrap Buyer

GRAHAM Miss Jennifer Ann
Leading Control Operator, Lincolnshire Fire Brigade

GREEN Michael John
Trainee Chemist, Nypro (UK) Ltd

GRIMLEY John Joseph
Temporary Process Technician, Nypro (UK) Ltd

GUGAN Dr Keith
Partner, Dr J H Burgoyne & Partners

HALDERIT Anton Hendrik Theresia
Commissioning Engineer, Stamicarbon

HANSON Philip
Crane Driver, Archer & Sharpe Scunthorpe

HARDY Christopher Peter John
Process Control Technician, Nypro (UK) Ltd

HARDY Thomas
Chemical Plant Operator, Nypro (UK) Ltd

HARRY Lawrence
Process Operator, Nypro (UK) Ltd

HEBBS Anthony Roy
Electrician, Nypro (UK) Ltd

HEWITT David Walter
Shift Superintendent, Nypro (UK) Ltd

HEWORTH William Eric
Plant Operator, CEGB

HIBBARD Cecil
Assistant Engineering Technician, Nypro (UK) Ltd

HOBSON Alan
Shift Superintendent, Nypro (UK) Ltd

HOBSON John William
Inspector, HMFI

HOLMES Leslie
Security Supervisor, Nypro (UK) Ltd

HOODLESS Robert Charles
Chemist, Nypro (UK) Ltd

HOPKINS Kenneth Charles
Area Superintendent, Nypro (UK) Ltd

HOTCHIN James Edward
Shift Chemist, Nypro (UK) Ltd

HUGHES James Frederick
Assistant Chief Engineer (Operations), National Smokeless Fuels Ltd

HUNSLEY Kenneth Raymond
Ammonia Plant Operator, Nypro (UK) Ltd

HUTCHINSON Cecil
Driver

HUTCHINSON Lawrence
Crane Driver, British Steel Corporation

ISBELL Charles Leslie
Assistant to Services Engineer, Nypro (UK) Ltd

ISBELL Vincent
Relief Shift Superintendent, Nypro (UK) Ltd

ISLE David
Laboratory Shift Leader, Nypro (UK) Ltd

JAS Diderick Hendrik
Dutch State Mines

JONES Leonard
Pipe Welder, Quinn Brothers Ltd

JONES Dr Tudor Bowden
Senior Lecturer, University of Leicester

JOHNSON David Francis George
Lorry Driver,

JOHNSON Robert John
Plant Operator, Nypro (UK) Ltd

KEMP Dr Patrick Hubert
Reader, University College London

KENYON Clifford
Project Group Manager, Simon Carves Ltd

KILLELAY Victor Herbert
Utilities Operator, Nypro (UK) Ltd

KING Eric
Statistics Clerk, BSC Chemicals

KIRK Robert
Detective Sergeant, Humberside Police

KIRTON Raymond
Supervisor, Nypro (UK) Ltd

KITCHING Dr Ronald
Reader, UMIST

LAWRIE Alistair
Security Officer, Nypro (UK) Ltd

LISSIAK Dr Luciano Danilo
Chemist, Nypro (UK) Ltd

MAJOR George Edwin
Engineering Technician, Nypro (UK) Ltd

MARSHALL Victor Christopher
Senior Lecturer, University of Bradford

MATHESON Donald Farquhar
Temporary Shift Supervisor, Nypro (UK) Ltd

MAYFIELD John
Chemist, Nypro (UK) Ltd

McCARTHY Michael
Scaffolaer, MKM Scaffolding

MIDDLETON Maurice Anthony
Engineering Technician, Nypro (UK) Ltd

MILLER Dr Keith John
Lecturer, University of Cambridge

MOAT Michael
Hydrogen Plant Operator, Nypro (UK) Ltd

MORLEY Dr Ronald James
Director of Chemical Investments, NCB (Coal Products) Ltd

MORTON Professor Frank
Emeritus Professor, University of Manchester

MUNDAY Dr George
Lecturer, Imperial College London

NAYLOR Ellis
Panel Control Technician, Nypro (UK) Ltd

NAYLOR Eric
Materials Handling Supervisor, Nypro (UK) Ltd

NEWLAND Prof David Edward
Professor, University of Sheffield

NICHOLSON Dr Christopher Edgar
Deputy Head of Metallurgical Section, SMRE

ORBONS Hubert Gerard
Head of Materials Testing and Corrosion Department, Dutch State Mines

PETTIT Brian
Assistant Shift Leader, Nypro (UK) Ltd

POLLARD Anthony John
Day Group Leader, Nypro (UK) Ltd

PRESCOTT Harold Edward
Scaffolder, MKM Scaffolding Ltd

RAKESTRAW Dr J A
Director, L H Manderstam & Partners (UK) Ltd

RAWSON Charles Edward
Engineering Supervisor (Construction), Nypro (UK) Ltd

ROBERTS Dr Alan Frederick
Principal Scientific Officer, SMRE

ROBERTS Leslie
Supervisor Fitter, Nypro (UK) Ltd

ROBERTS Malcolm Anthony
Process Technician, Nypro (UK) Ltd

ROBINSON Mrs Irene
Clerk Typist, Humberside Fire Brigade

RODEN Anthony Peter
Plant Operator, Nypro (UK) Ltd

ROWBOTTOM Martin Lee
Operator — Acid Plant, Nypro (UK) Ltd

RYDER Dr Dennis Arthur
Senior Lecturer, UMIST

SADÉE Dr Constant Pierre Marie
Research Engineer, Dutch State Mines

SAXBY George Frederick
Shift Superintendent, Nypro (UK) Ltd

SCOTT Phillip Ashley
Assistant Engineering Technician, Nypro (UK) Ltd

SELMAN Rudolph Eric
Managing Director, Nypro (UK) Ltd

SHAW Michael
Consultant, Dutch State Mines

SLATER David
Consultant, Cremer & Warner

SMITH Albert Edward
Machinist, Starco Engineering Ltd

SMITH Anthony Rowland Embleton
Superintending Inspector, HMFI

SMITH Donald R
Senior Fire Prevention Officer, Humberside County Fire Brigade

SMITH John Alexander
Power Plant Superintendent, Nypro (UK) Ltd

SPENCER Geoffrey
Panel Technician, Nypro (UK) Ltd

SPENCER Jeffrey
Panel Technician, Nypro (UK) Ltd

SQUIRE Herbert
Power Plant Operator, Nypro (UK) Ltd

STACEY Robert Harold
Process Control Technician, Nypro (UK) Ltd

STAPLETON Roy
Chemical Plant Technician, Nypro (UK) Ltd

STEVENS Tony Norman
Chemist, Nypro (UK) Ltd

SWANN Dr Peter Roland
Reader, Imperial College London

SYKES David
Shunter, British Rail

SYLVESTER-EVANS Roderick
Consultant, Cremer & Warner

TAYLOR Alan William
Fitter, Nypro (UK) Ltd

THOMAS David Henry
Engineering Supervisor, Nypro (UK) Ltd

THOMAS Glyn Owen
Technical Director, Teddington Bellows Ltd

THORNTON Dennis J
Senior Process Technician, Nypro (UK) Ltd

TORN Peter Thomas
Process Worker, Nypro (UK) Ltd

TOYNE Henry
Slinger, British Steel Corporation

TUNE Ian Alfred
Shift Chemist, Nypro (UK) Ltd

WADDINGHAM Steven George
Process Operator, Nypro (UK) Ltd

WALDEN John Stewart
Senior Process Technician, Nypro (UK) Ltd

WATERHOUSE David
Principal Scientific Officer, SMRE

WATSON Derek Roy
Mechanical Fitter's Mate, Industrial Plant Services

WATSON Peter
Shift Superintendent, Nypro (UK) Ltd

WEBSTER Dr George Arnold
Senior Lecturer, Imperial College London

WHITE Melvyn Oliver
Assistant Superintendent, Nypro (UK) Ltd

WHITEHEAD Stephen
Fitter, Starco Engineering Ltd

WHITELAW George
Bernard Hastie & Co

WHITLEY Leslie Arnold
Fitter's Mate, Nypro (UK) Ltd

WILKINSON Eric William
Plant Fitter, Nypro (UK) Ltd

WILLIAMS John Owen Melville
Temporary Mechanical Supervisor, Nypro (UK) Ltd

WILLIAMS Michael Alwyn
Engineering Inspector, HMFI

WRIGHT Dr John A
Lecturer, University of Aston in Birmingham

VAN GILS Denis John
Plant Manager, Nypro (UK) Ltd

Appendix VI Reports presented to the Court

1 *Reports given in evidence*

D J ARROWSMITH, MA, PhD
University of Aston in Birmingham
"Experimental observations on the behaviour of liquid zinc on 316L stainless steel".

G. ARTINGSTALL, BSc, PhD
Safety in Mines Research Establishment
"Appraisal of the damage in the reactor vessels in Section 25A".

PROFESSOR J G BALL, BSc, ARSM, SIM
Imperial College, London
"The Flixborough Disaster—a metallurgical assessment".

J F BARNBY, BSc, PhD, FIM
University of Aston in Birmingham
"Report on the high temperature short time creep rupture of AISI 316L steel".

P C BOWES, BSc, ARCSc, FRIC
Fire Research Station of the Department of the Environment and Fire Offices Committee
"Report on the subject of lagging fires in relation to the Flixborough Disaster".
"Fires in oil soaked lagging".

SIR ALAN H COTTRELL, BSc, PhD, MA, FRS
Jesus College Cambridge and

DR P R SWANN, BSc, PhD
Imperial College London
"Metallurgical examination of the failure of the 8 inch diameter stainless steel pipe" + Addendum.

J I COX, BSc, ARCS, DIC, PhD, CEng, FIChemE, MBCS
L H Manderstam & Partners (UK) Limited
"Report into the causes of the Flixborough Disaster".

CREMER AND WARNER REPORT No. 3
D H SLATER, BSc, PhD
"Markings on the non-return valve flange in the 8 inch line."

CREMER AND WARNER REPORT No. 4
G A WEBSTER, BSc(Eng), PhD, DIC, ACGI, CEng, MIMechE
Imperial College London
"Investigation of scaffolding deflection under load".

CREMER AND WARNER REPORT No. 5
R SYLVESTER-EVANS, BSc
"Investigation into the theory of overpressure of the 20 inch by-pass by high pressure nitrogen purge of the oxidation system".

CREMER AND WARNER REPORT No. 6
G A WEBSTER, BSc(Eng), PhD, DIC, ACGI, CEng, MIMechE, and

K E BETT, BSc, DIC, PhD, CEng, FCS, MIChemE
Imperial College London
"An analysis of the forces acting on the 20 inch nominal diameter piping assembly installed between R2524 and R2526".

CREMER AND WARNER REPORT No. 7
R J BATSTONE, BSc, DipSugTech, PhD, CEng, MIChemE
"Description and results of the bellows test programme and 20 inch dog-leg pipe tests".

CREMER AND WARNER REPORT No. 8
R. SYLVESTER-EVANS, BSc
"An investigation into the movement of the 8 inch line assembly to establish whether rupture occurred before or after the main explosion".

CREMER AND WARNER REPORT No. 9
G MUNDAY, BSc, PhD, CEng, MIChemE
Imperial College London
"The sizes of possible flammable vapour cloud discharged from Section 25A".

A G EVANS, CEng, MIMechE
HM Factory Inspectorate
"Flixborough Disaster — appraisal of 20 inch/28 inch bridging pipe assembly".
"Comparison of the results from tests 7 and 8 at Flixborough and the Bretby test".
"Comparison between DSM small scale and the simulation tests carried out at Flixborough".

J H FOLEY, BSc, PhD, and C E NICHOLSON, PhD, AIM
Safety in Mines Research Establishment
"Metallurgical examinations of damaged pipes from Section 25A".

PROFESSOR S S GILL, MSc(Tech), PhD, DSc
University of Manchester Institute of Science & Technology
"The Nypro by-pass line".

K GUGAN, PhD, DIC, CEng, FIChemE, MInstF
Dr J H Burgoyne and Partners
"The Explosion and fire at Nypro (UK) Ltd Flixborough".
"The 8-inch pipe".
"A small scale experimental study of the discharge and flame characteristics from the 8 inch pipe line between S2538 and S2539".
"Fire tests undertaken at Yarsley Testing Laboratories".

J F HUGHES, CEng, MIMechE, AMIED
National Smokeless Fuels Ltd
"Survey of the Nypro Engineering Department".

T B JONES, BSc, PhD, MInstP
University of Leicester
"Ionospheric effects of the Flixborough explosion".

P H KEMP, BSc, PhD, MICE, MIMun E
University College London
"Report on wind tunnel simulation studies".

R KITCHING, MSc, PhD, DSc
University of Manchester Institute of Science & Technology
"Stress concentrations at mitred pipe joint".

V C MARSHALL, MSc, CEng, FIChemE, AM InstF
University of Bradford
"Report on the Disaster".

MATCON
Consultants in Metallurgy and Materials Technology
*111 — "Metallurgical examination of certain components from the Flixborough plant".
*112 — "Report on zinc embrittlement of 316L stainless steel".
*113 — "Further investigation of zinc embrittlement".

PROFFESSOR F MORTON, DSc, PhD, MSc(Tech), FRIC, FIChemE, CEng
University of Manchester
"Assessment of evidence".

PROFESSOR D E NEWLAND, MA, ScD, CEng, MIMechE, MIEE
University of Sheffield
*"Report on an investigation of possible causes of failure of the 20 inch by-pass pipe assembly at Flixborough".

H G ORBONS
Dutch State Mines
*"Investigation of the 8 inch line between S2538 and S2539".
"Investigation on a number of bolts of the flanged joint of the non return valve in the 8 inch line between S2538 and S2539".

*See paragraph 5.

A F ROBERTS, BSc, PhD, DSc, MIChemE, CEng
Safety in Mines Research Establishment
"A survey of certain aspects of the blast damage".

D A RYDER, BSc, MMet, DiplPhysMet, FIM
University of Manchester Institute of Science & Technology
"Flixborough Disaster — comments on the metallurgical examination of the 8 inch line and the 20 inch by-pass assembly".

DR Ir C P M SADÉE
Dutch State Mines
"Self ignition of cyclohexane oxidation process liquids in insulation materials".
"Estimation of the TNT equivalent of the amount of reacted cyclohexane, and of the dimensions and the shape of the cloud in relation to the explosion which occurred on the Flixborough site of Nypro (UK) Ltd on 1st June 1974".

G O THOMAS, BSc
Teddington Bellows Ltd
"Axial and lateral spring rate tests".

D WATERHOUSE, BSc(Eng), MIMechE, CEng, and G A C GAMES, BA
Safety in Mines Research Establishment
"Construction of and tests on a reconstructed bridging pipe assembly".

G A WEBSTER, BSc(Eng), DIC, PhD, ACGI, CEng, MIMechE
Imperial College London
"An investigation of the stability of double bellows assembly".

J A WRIGHT, BMet, PhD
University of Aston in Birmingham
"The effect of prior cold work on the high temperature reduction of area in mild steel".

2 *Reports referred to in evidence*

CREMER AND WARNER REPORT No. 1 — Proof of evidence of Sir Frederick Warner.

CREMER AND WARNER REPORT No. 2 — General.

CREMER AND WARNER REPORT No. 10 — "Comparison of Discharge, etc".

HM FACTORY INSPECTORATE — Interim Report.

MERZ & McLELLAN — "Design assessment of pipe work".

M J G NOTTEN
Dutch State Mines
"Examination of the cause of the leakage of reactor R2525".

NYPRO (UK) LTD — Site investigation report.

3 *Reports made available to the Court*

V J StC CLANCEY, MBE, BSc, ARCS, FRIC
J H Burgoyne and Partners
"Appraisal of the approach adopted in Cremer and Warner Report No. 9".

B J L DARLASTON and R T D NICHOLSON
"An evaluation of conditions leading to cavitation damage similar to that found in the Type 316L stainless steel 8 inch pipe".

FIRE RESEARCH STATION
"Report on fire and explosion at Nypro".

GOVERNMENT CHEMIST
"Report on samples submitted".

F H GROVER
Procurement Executive, Ministry of Defence
"AWRE Report No. 046/74 — Infrasonic and wave records from the Flixborough and St Brigid explosion".

HUMBERSIDE FIRE BRIGADE
"Report on the explosion at Nypro (UK) Ltd".

J H K MINKHORST
Stamicarbon
"The static and dynamic behaviour of an unsupported pipe between expansion bellows".

F MOENS and C BRONKE
Stamicarbon
"The pressure in the neutralisation separator S2358".

D E J SAMUELS and T P O'BRIEN
Procurement Executive, Ministry of Defence
"An estimate of the energy of the explosion at Nypro".

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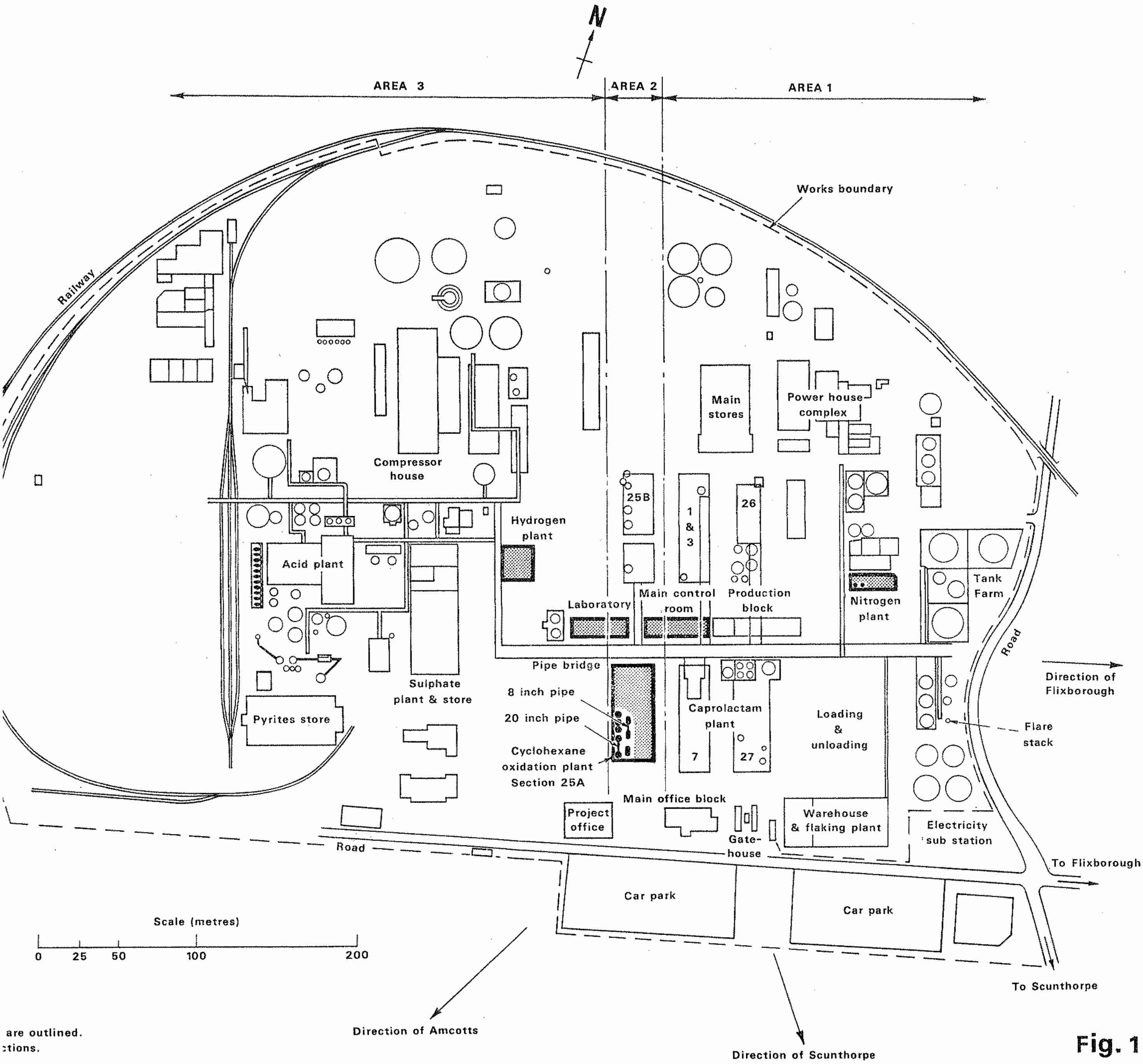
Dd 288800 K80 4/75



are outlined.
:tions.

Fig. 1

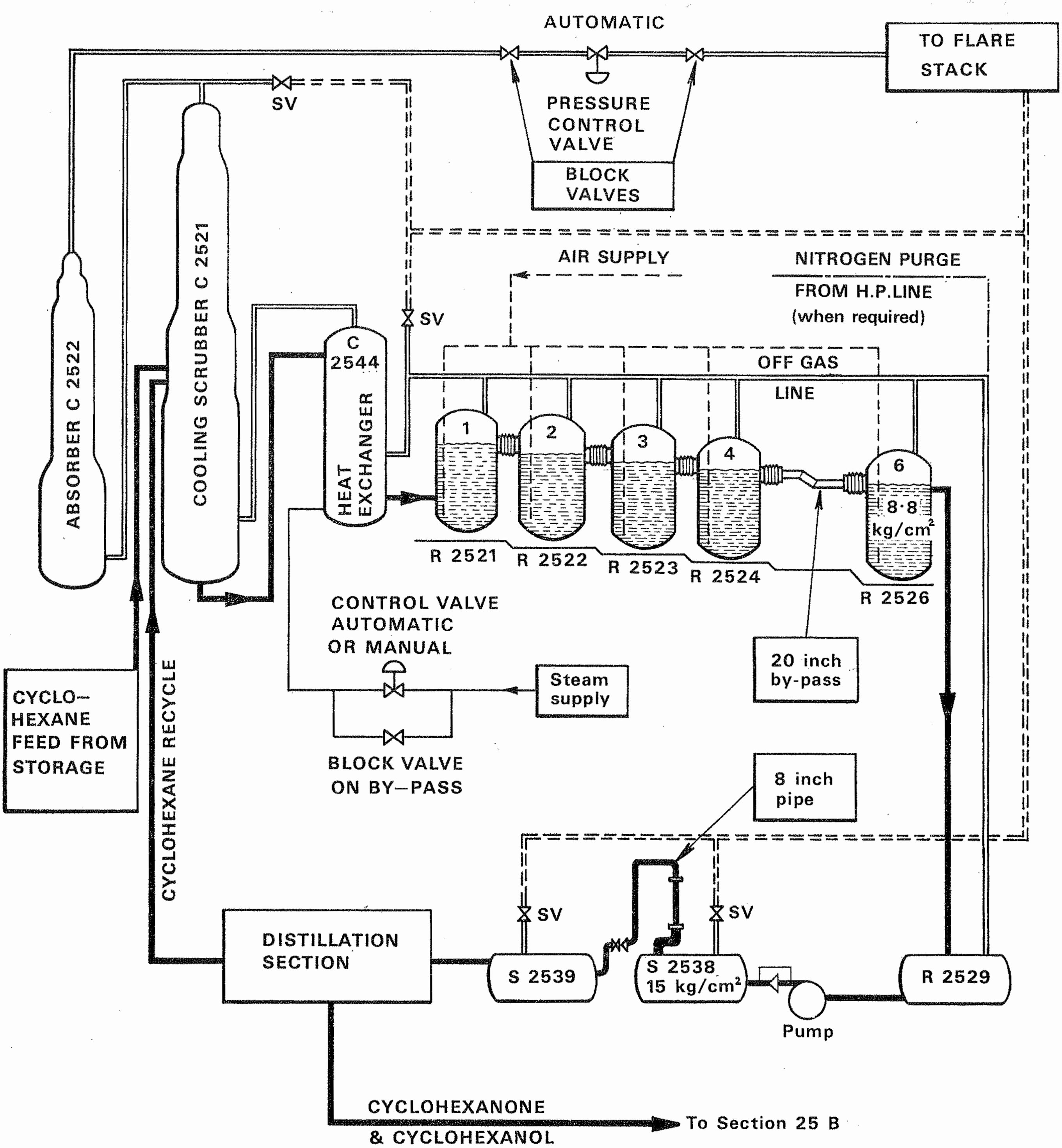
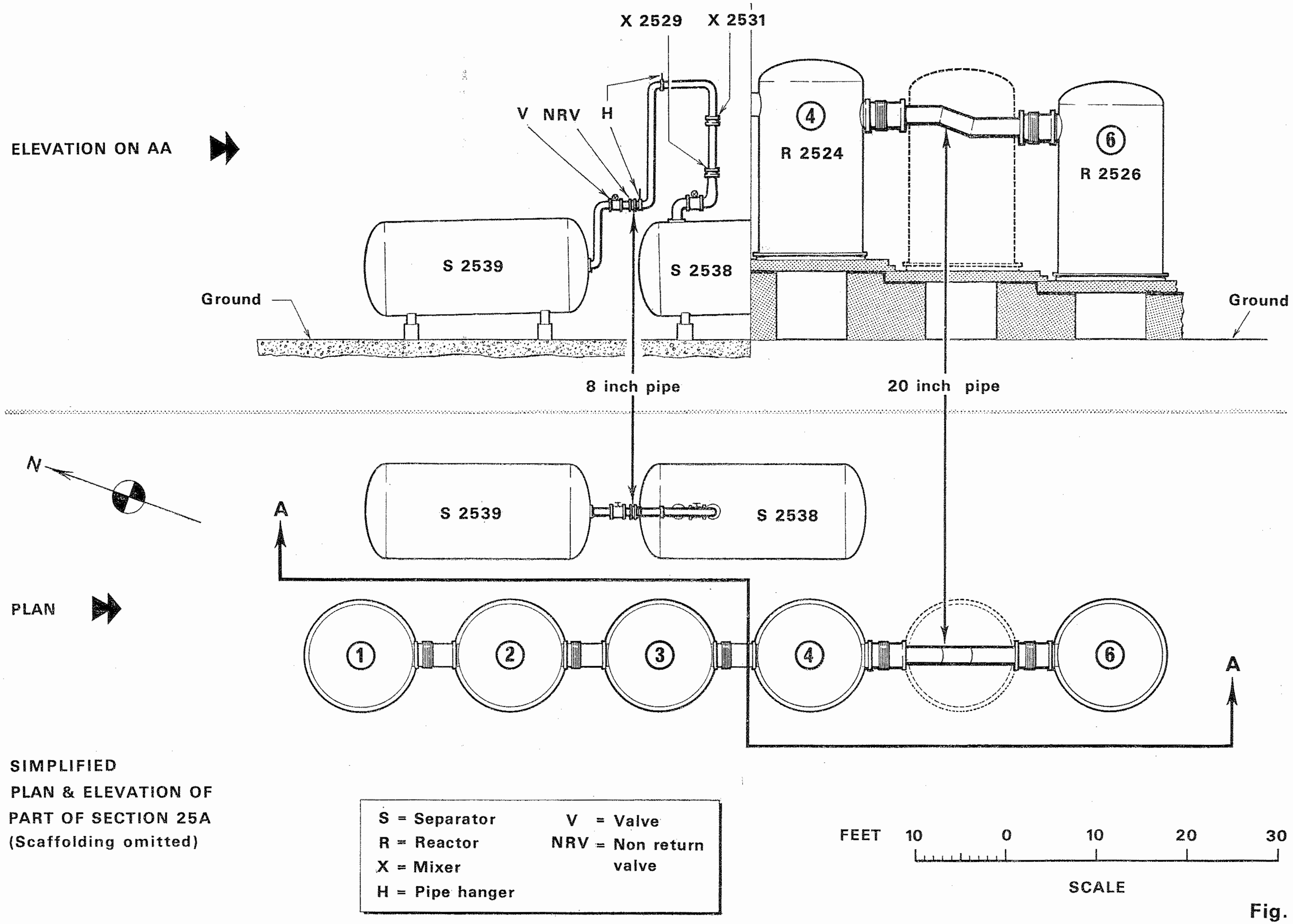


Fig. 2

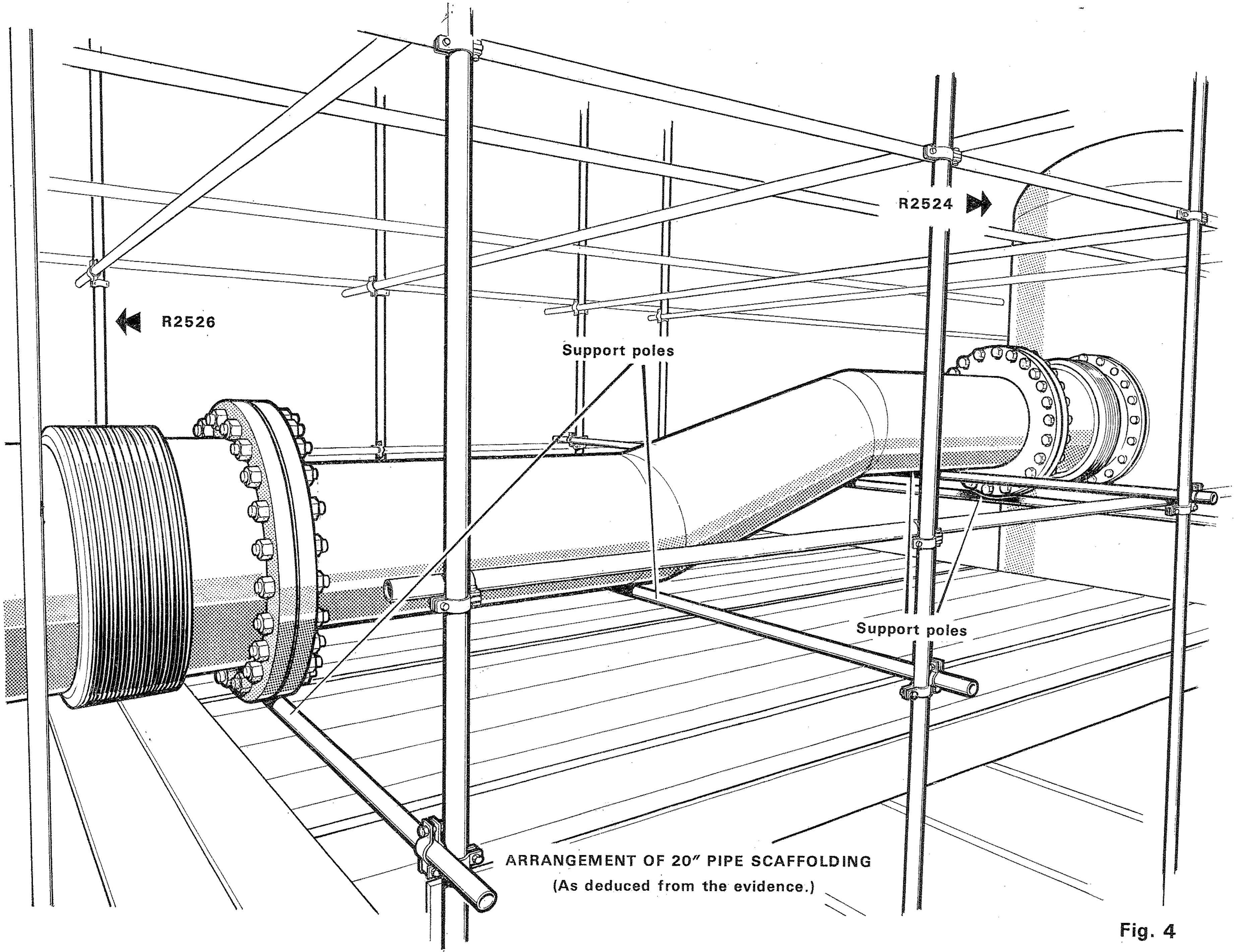
CYCLOHEXANE OXIDATION
PLANT SECTION 25 A
Simplified flow diagram (not to scale)

Cyclohexane loop	SV	=	Safety valve
Off gas	R	=	Reactor
Safety valve vent	S	=	Separator
Nitrogen (when required)	C	=	Column
Air (not operating at time of disaster).			



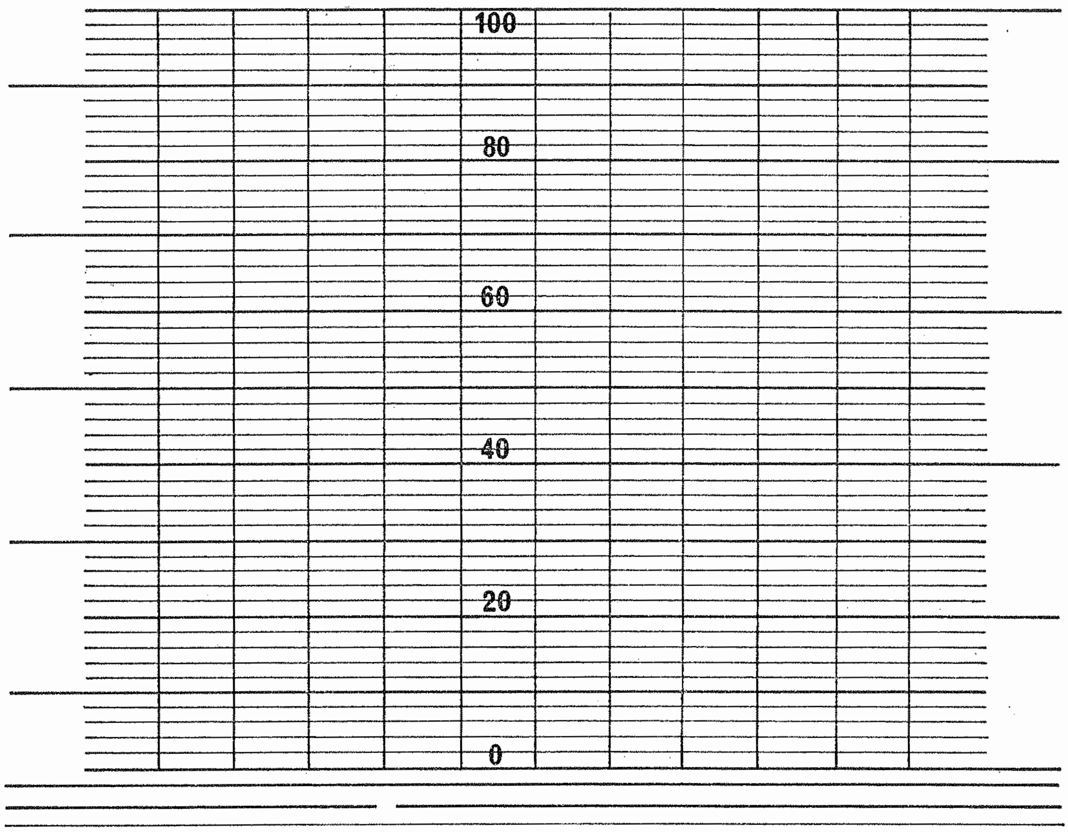
SIMPLIFIED
PLAN & ELEVATION OF
PART OF SECTION 25A
(Scaffolding omitted)

Fig. 3



ARRANGEMENT OF 20″ PIPE SCAFFOLDING
(As deduced from the evidence.)

Fig. 4



Note that:

(a) The pressure is recorded on the chart by a red ink pen.

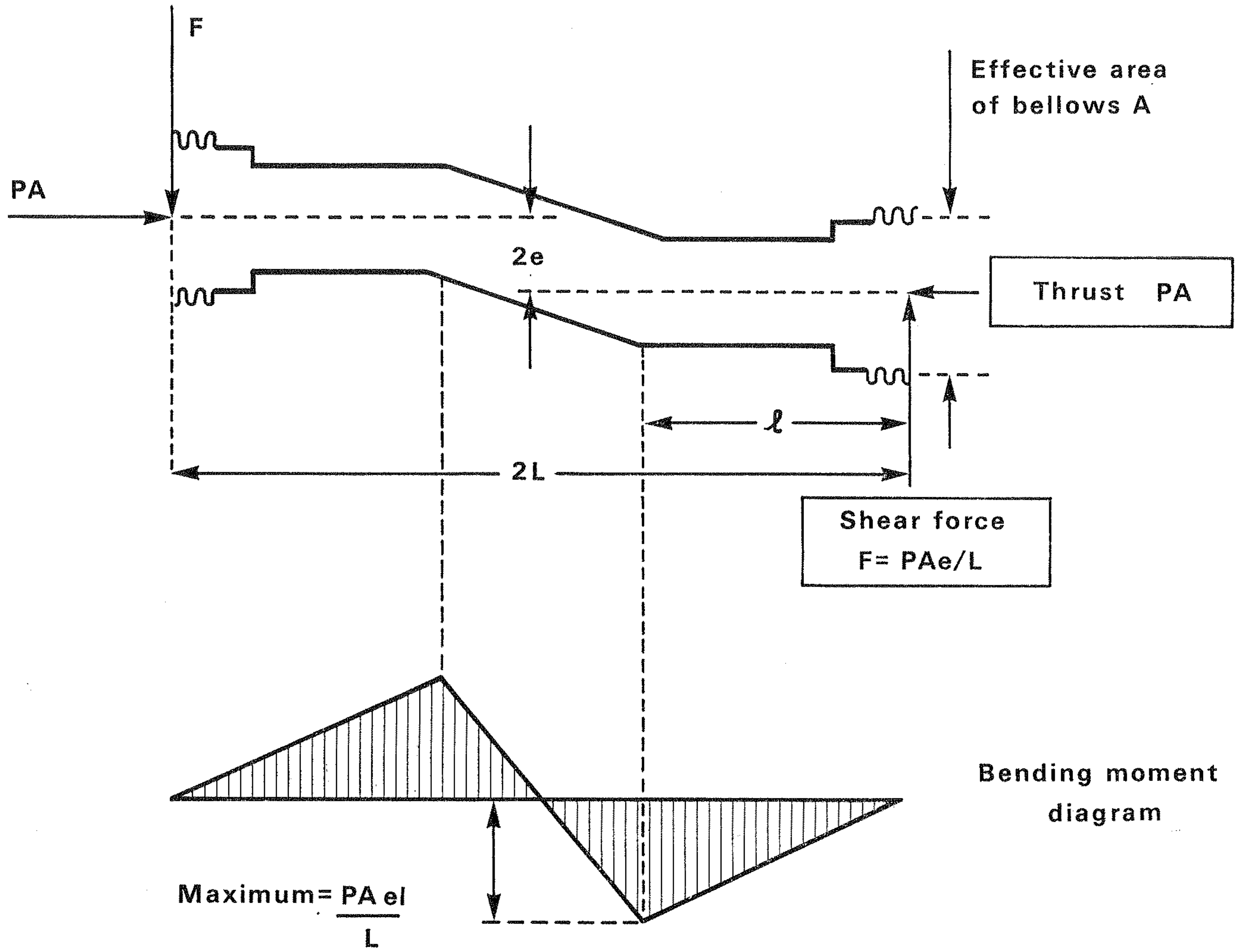
(b) On the right hand side of the instrument there was a red mark to indicate 8.5 kg/cm^2

(c) On the left hand side of the instrument there was a variable red set point indicator.

SECTION 25A –

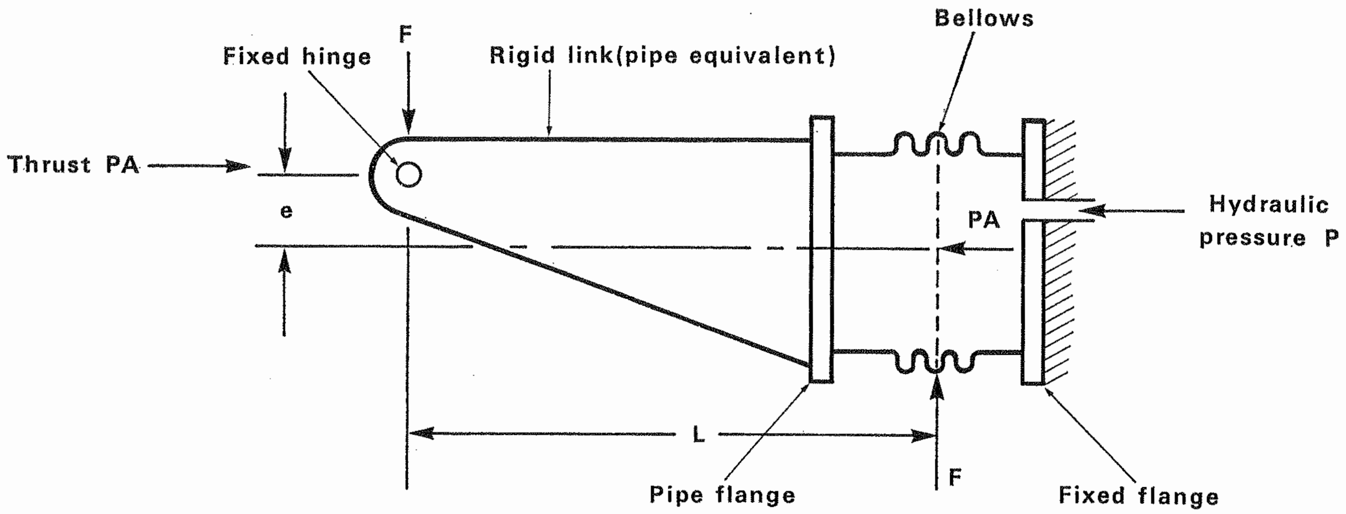
CHART USED FOR RECORDING OXIDATION SYSTEM PRESSURE

Fig. 5



SKETCH OF PIPE AND BELLOWS ASSEMBLY SHOWING SHEAR FORCES
ON BELLOWS AND BENDING MOMENTS IN PIPE
(DUE TO INTERNAL PRESSURE ONLY)

Fig. 6



PRINCIPLE OF NOTTINGHAM TEST RIG

LENGTHS L AND e WERE THE SAME AS ON
THE ACTUAL PIPE AND BELLOWS

Fig. 7

SKETCH OF 8-INCH LINE AS VIEWED FROM THE NORTH WEST.

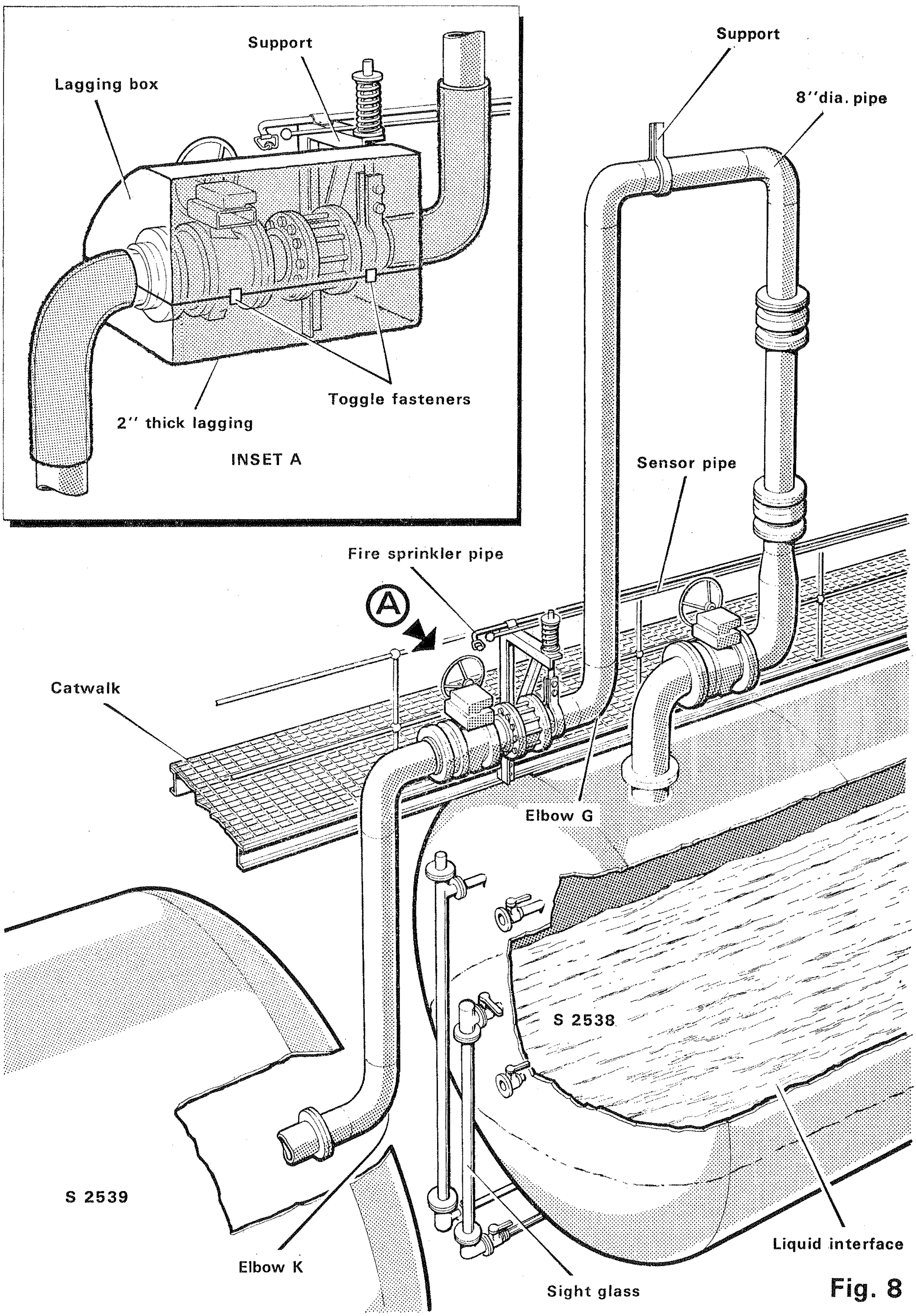
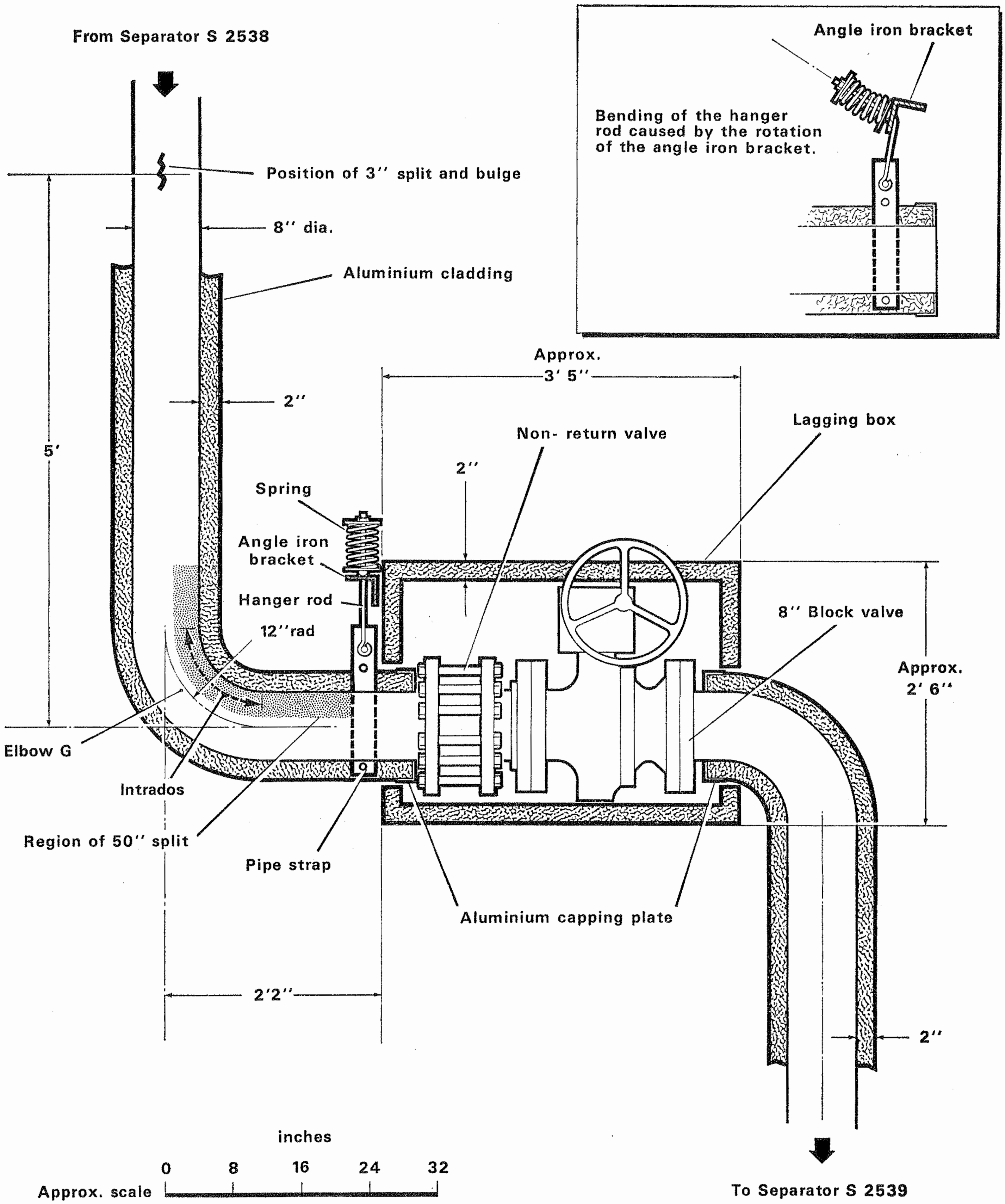
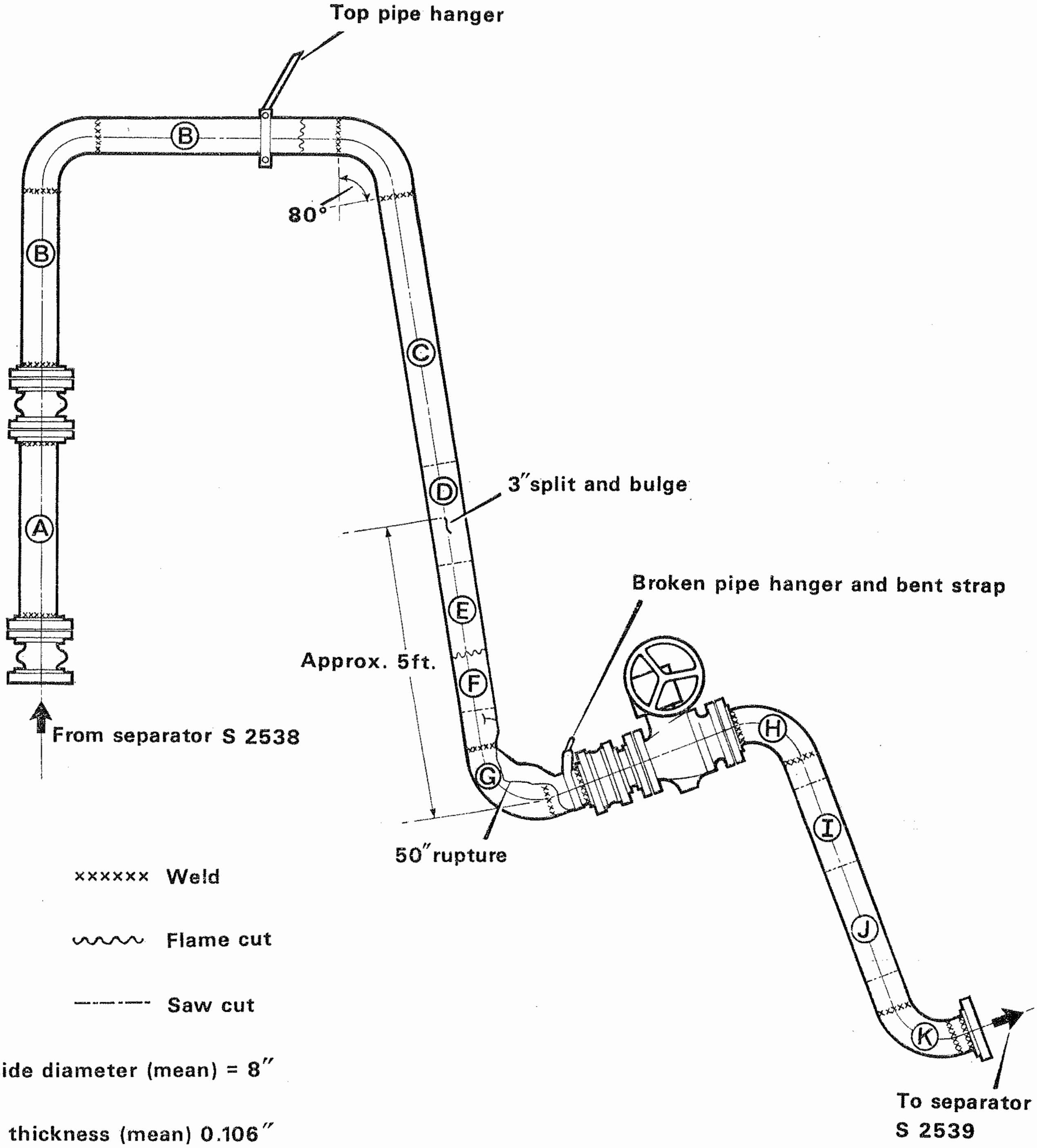


Fig. 8



VIEW OF 8-INCH LINE SHOWING TYPICAL LAGGING BOX AROUND VALVES
AS VIEWED FROM THE EAST

Fig. 9



KEY ×××××× Weld

∿∿∿∿ Flame cut

—·—·— Saw cut

Outside diameter (mean) = 8″

Wall thickness (mean) 0.106″

THE DAMAGED 8″ PIPE

Fig. 10

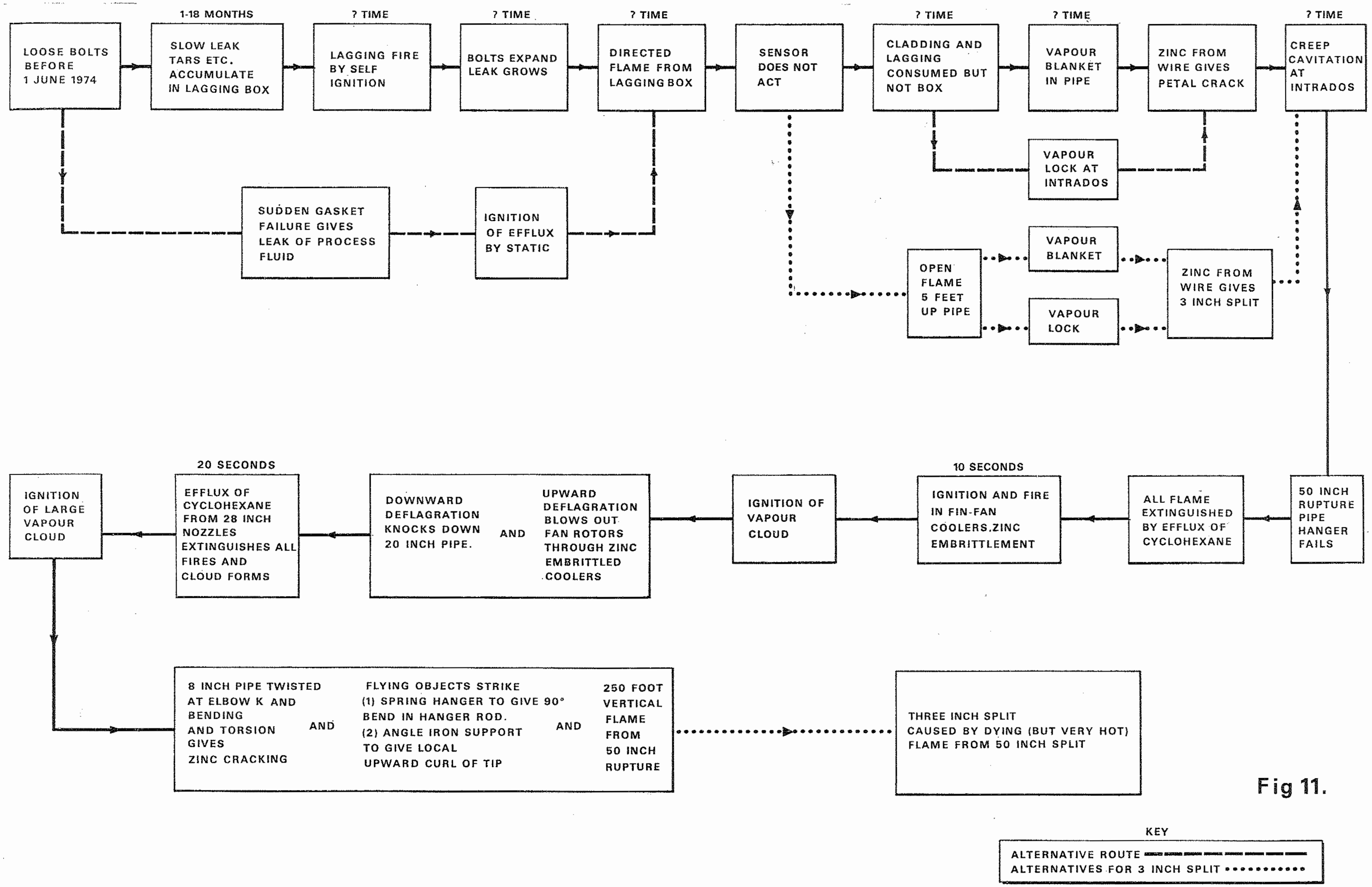


Fig 11.

STEPS IN THE '8 INCH PIPE' HYPOTHESIS

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