AN ANALYTICAL ASSESSMENT OF THE SINKING OF THE M.V. *DERBYSHIRE*

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SUMMARY

The author was appointed by the UK Department of Transport as a fellow Assessor with R.A. Williams during Lord Donaldson's Assessment (1995) of the loss of the OBO ship DERBYSHIRE and in 1996 as a UK Assessor for the planning and surveys of the wreck. He relinquished his appointment in October 1997 and was not thereafter involved in the review and analysis of data gathered. This paper may be considered to be complementary to the reports of the UK and EC Assessors (Williams and Torchio, 1998a and 1998b) which followed that review and analysis. The paper deals with the history and loss of the ship, including the concept developed in 1995 of 13 possible loss scenarios in a formal safety Risk Matrix of probability and seriousness. It analyses abnormal wave effects on hatch cover collapse, on ship bending, and on flooding of bow spaces and no. 1 hold. The implosion-explosion mechanics during sinking is outlined to explain the devastation of the wreck. The 1996 and 1997 underwater surveys are outlined as are the findings of fact. Each of the final 14 loss scenarios is analysed in the light of the firm and circumstantial survey evidence, plus many other factors of service experience, analyses and experiments. The updated Risk Matrix speaks for itself and leads to the prime conclusions and major recommendations.

NOMENCLATURE

Ship dimensions, etc

B	=	Maximum beam
C	=	coaming or opening heightabove deck
C_b	=	block coefficient
D	=	moulded depth
F	=	freeboard approx. 6.9 m
GM	=	transverse metacentric height
I_θ	=	longitudinal mass moment of inertial of ship and cargo
L	=	length between perpendiculars
L_m,L_t	=	mass, trim point distance to LCF
Mct	=	moment to change trim one centimetre (tonnes metres)
Tpc	=	tonnes per centimetre immersion
T	=	mean draught
t	=	change in trim
V_c	=	compartment volume
Δ	=	displacement
δ	=	parallel sinkage
ρ	=	sea water density

Structural strength:

A_s	=	stiffener cross-section area
a	=	spacing of transverse stiffeners
b	=	spacing of longitudinal stiffeners
C_p	=	water impact coefficient
E	=	Young's modulus
L	=	length of panel, hatch cover
M_p,M_u	=	plastic, ultimate bending moment
M_t	=	tripping moment of stiffener
M_w	=	wave-induced bending moment
p_d,p_i	=	design, water impact pressures
p_u	=	ultimate pressure load
s	=	plastic shape factor
t	=	plate thickness
W	=	width of panel, hatch cover
w_o	=	permanent deflection at plate centre
Z	=	stiffener, ship minimum section modulus
z_s	=	stiffener centroid above plate
α	=	a/b plate element aspect ratio
β	=	(b/t) $(E/\sigma_y)^{0.5}$ plate slenderness
a_e , b_e	=	effective plate widths
σ,σ_o	=	direct stress, direct yield stress (min)
τ,τ_o	=	shear stress, shear yield stress

Wave environment:

A_c	=	crest peak amplitude above SWL
a	=	crest profile amplitude above opening
c	=	λ/T wave celerity = gT/2π
D	=	period used in analyses during which stationary conditions prevail
F(H)	=	cumulative distribution function of waves
f(H)	=	probability density function of wave heights
H, H_s	=	wave height, significant wave height
H_m,H_e	=	most probable, extreme wave heights
h	=	crest peak height above opening or hatch
h_b	=	maximum mean pressure head of crest profile as it passes over no. 1 hatch cover
L_o	=	horizontal crest length of an abnormal wave which passes over a small opening
m_b	=	mean back slope of abnormal wave crest
m_f	=	mean front slope of abnormal wave crest
N	=	D/T_p number of waves passing
p_e(H)	=	wave height exceedence probability p_e
T,ω	=	wave period, frequency
T_z,T_p	=	wave upcrossing, modal periods
α	=	A_c/H crest amplitude ratio
ε	=	band width parameter
γ	=	1nN-2$(H/H_s)^2$ probability parameter
λ	=	$gT^2/(2\pi)$ length of gravity waves
ζ	=	wave amplitude above SWL

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Orifice flow theory:

A_o	=	orifice (opening) area
a	=	time varying water head above orifice
c_d	=	discharge coefficient = 0.6 assumed
v	=	mean downward velocity of water column entering the orifice
V_h	=	total volume of water entering orifice during passage of wave crest of local peak height h
V_{iD}	=	total ingress volume during period D
V_i	=	V_{iD}/D ingress flow rate appropriate to D

Acronyms:

ALARP	=	As Low As Reasonably Practical
COST	=	Cooperation Scientifique et Technologique
DETR	=	Department of Environment, Transport and the Regions (previously DoT)
DFA	=	Derbyshire Family Association
DMI	=	Danish Maritime Institute
DSL	=	Deep Submergence Laboratory
EC	=	European Commission
FI	=	Formal Investigation
FPSO	=	Floating Production, Storage and Offloading
FSA	=	Formal Safety Assessment
GMT	=	Greenwich Mean Time (denoted by Z)
GoM	=	Gulf of Mexico
IACS	=	Intl Association of Classification Societies
ICLL	=	Intl Convention on Load Lines
IMO	=	Intl Maritime Organization
ISSC	=	Intl Ship and Offshore Structures Congress
ITF	=	Intl Transport Workers' Federation
ITTC	=	Intl Towing Tank Committee
JTWC	=	Joint Typhoon Warning Centre
LR	=	Lloyd's Register of Shipping
MAIB	=	Marine Accident Investigation Branch
ROV	=	Remotely Operated Vehicle
RTS	=	Revolving Tropical Storm
SOC	=	Southampton Oceanographic Centre
TD,TS	=	Time Domain, Time Series
TNT	=	Trinitrotoluene
WBT	=	Wing Ballast Tanks (topsides)
WHOI	=	Woods Hole Oceanographic Institution

1. INTRODUCTION

"I can command men and ships, but I cannot command the wind and sea" - Admiral Lord Nelson.

1.1 The Ship

The *LIVERPOOL BRIDGE* (later renamed *DERBYSHIRE*) was ship no. 57, the last of a class of six OBO carriers designed by Swan Hunter at their Wallsend Yard in 1969 and built in the period 1970-76 at the Haverton Hill Shipyard on the river Tees, which Swan Hunter acquired from the Furness Shipbuilding Company in 1968. She was classed with Lloyd's Register and delivered to Bibby Bros., Liverpool, in 1976. Her relevant principal particulars were:

L	281.94 m	Service T	17.04 m
B	44.20 m	Summer T	18.46 m
D	24.99 m	max. Δ	203,800 te
C_b	0.84	max. DWT	173,200 te

On her last voyage from Sept Isles, Canada, to Yokohama she was carrying about 158,000 te (tonnes) of ore concentrates distributed in 7 of the 9 holds, as shown in Fig. 1 (Lloyd's Register, 1987) which also depicts the oil fuel, fresh water and minimal ballast water distribution. Her estimated displacement as she approached Japan was about Δ = 194,000 te and hence mean draught T $\cong$ 18.0 m and F $\cong$ 7.0 m.

The class was of double hull construction, with double skin sides and transverse bulkheads between holds, double skin cofferdams in the aft section and between hold no. 1 and the internal spaces at the fore peak stores deck level. The only major subdivision structure that was single skin was the transverse collision bulkhead 339, as can be seen in Fig. 1.

The first ship of the class *FURNESS BRIDGE*, completed in 1971 had the thick hatch side girders (which formed the internal longitudinal boundaries of the topsides WBTs) continued from hold no. 9 through bulkheads 65 and 64 and scarfed and butt welded to the thinner longitudinal machinery space bulkhead in the same plane. This design was modified for later ships so that a cofferdam 64/65 between the hold 9 and the port and starboard slop tanks could be constructed as a unit.

As a result, the 5 later ships of the class ended these hatch side girders at bulkhead 65 with partial penetration welds forming a cruciform connection, as had been previous practice in the VLCCs which the firm had built. Although this was an approved modification, it was later to be a major cause for concern to the DFA.

The final important design and operation feature to note is that these Type B cargo ships were categorised as B-60 ships under the 1966 ICLL regulations (Murray-Smith, 1969). This relaxed (reduced) the freeboard requirements providing that a one compartment flooding standard was met when fully laden. This gave a minimum summer load F $\cong$ 6.5 m for the class. This requirement could be met by *DERBYSHIRE* but many B-60 ships cannot (Lloyd's List, 1996).

1.2 The Loss and Events up to 1986

On or about the 9th of September 1980 when the vessel was hove to in the most dangerous semi-circle of Typhoon ORCHID, the ship was lost with all hands (44, including 2 wives) at about 25.86° N and 133.53° E on the northern flank of the Daito Ridge, some 400 miles South of Shikoku Island, Japan. There was no distress signal and only two sightings of oil upwellings seen some days later gave a clue to the position of the sinking. A lifeboat from the ship was sighted but this was not recovered and subsequently sank.

As there was no available evidence, nor any established evidence of structural or other weaknesses in the six ships, the Government decided not to hold a formal investigation into the casualty. Then, 18 months later in March 1982 the *TYNE BRIDGE* experienced severe brittle fractures in the upper deck when in ballast in the North Sea. A 2.8 m crack propagated away from the port aft corner of no. 9 hatch opening, and a 4.7 m crack propagating from a weld burn aft of frame 65 but travelled inboard and forward to cross frame 65. The internal structure was subsequently modified.

This casualty led to much speculation, especially as the DFA were gathering information regarding cracking in the vicinity of frame 65 in several ships of the class. Evidence was mounting of bad alignment and workmanship either as-built or as-repaired. The DoT therefore initiated studies, including one with Bishop, Price and Partners (eventually extended and published with Temarel, 1991) and the results were incorporated in a report (DoT, 1986). Opinions on five most likely causes of the loss of the ship were offered:

- *Explosion* - less likely because she had not carried oil cargo since October 79 and had been tank cleaned
- *Shift of Cargo* - could result from an ingress of water into holds thereby causing liquifaction of the cargo
- *Failure of Hatch Covers* - deck flexing could "spring" the covers followed by water entry and rapid flooding and foundering

- *External Hull Damage* - ship struck by submerged or partially submerged object
- *Structural Hull Failure* - failure of part of the hull could lead to water ingress, etc.

This report pointed out that some of these scenarios would be apparent to the crew, and a ship message would be likely. Other points made were:

- Any misalignment at bulkhead 65 is significant only for local strength aspects; nevertheless, more consideration should be given to the alignment of this intersection
- The series of assumptions and events which would lead to a massive hull failure at or about frame 65 are contentions (and not considered further)
- Four of the five sister ships had not (as of 1986) suffered any major structural distress; the fifth, TYNE BRIDGE, also survived and its brittle cracking in 1982 in the upper deck is not considered to be relevant to the loss of the *DERBYSHIRE*.

The report ended "in the last analysis the cause of the loss of the *DERBYSHIRE"* is, and will almost certainly remain, a matter of speculation".

This final DoT report was substantially different form an earlier draft version in July 1985 which concluded that the most likely cause was "total structural failure" resulting from defective design and/or construction at the frame 65 connection. It was unfortunate that this report was not captioned "draft", was first leaked to the Press, then released by the Department and attracted very wide media attention. The builders and LR had not at that stage been consulted and the report was in fact seriously in error on several counts. This bad management by the DoT led to allegations of "cover up" and the DFA were outraged.

Shortly after in November 1986, the *KOWLOON BRIDGE* came to grief with no. 3 hold perched on the Stag Rocks off Bantry Bay following steering gear failure. There had been deck cracking aft, which had been temporarily reinforced to allow the ship to complete her voyage. Nevertheless, the stern eventually also broke off near to frame 65. As a result of this, and no doubt fuelled by the media and the pressure from the DFA, the Government ordered a FI into the loss of the *DERBYSHIRE*. It was inevitably biased toward a fuller assessment of the frame 65 loss scenario.

1.3 The FI and Events up to 1994

The Decision of the Wreck Commissioner and his three Assessors was "the Court finds that the *DERBYSHIRE* was probably overwhelmed by the forces of nature in Typhoon ORCHID, possibly after getting beam on to the wind and sea…..". The "Summary of Conclusions" of the Court (DoT, 1989) are:

1. the *DERBYSHIRE* was properly designed, properly built and constructed from material of approved standard
2. no inference can safely be drawn from the absence of any distress signal
3. the condition of the cargo when loaded and its loading were within the existing recommended parameters
4. the *DERBYSHIRE* was caught in the worst part of typhoon ORCHID and may have encountered local freak weather beyond what can be hindcast
5. the actions of her Master were not unreasonable
6. the possibility that the ship was lost as a result of torsional weakness in her hull is extremely low
7. the combination of circumstances necessary to postulate separation of the hull at frame no. 65 is very unlikely, though some element of doubt must remain
8. it is improbable that immediate or even sudden structural failure of the forward hatch covers caused rapid sinking
9. sequential flooding of holds is a possible cause of loss but not thought probable
10. if cargo liquifaction did occur, which is doubtful, it still cannot be concluded that it was the prime cause of the loss
11. if the ship got beam-on to the weather, structural failure and/or cargo shift would have become much more likely; it is quite possible that that happened, but it cannot be proved.

Again, the DFA were outraged by the lack of a firm conclusion regarding frame 65. Nevertheless, the subject had occupied about 40% of the proceedings.

The 1990 presentation and discussion in the RINA of the paper *"A theory for the loss of the m.v. DERBYSHIRE"* (Bishop, Price and Temarel, 1991) was valuable in bringing many facts together and in leading to a vigorous and beneficial discussion. The paper included a number of factual inaccuracies, and whilst the theory itself was not criticised, its application and inferences certainly were. A later debate goes into this more fully (Grigson, 1997). The charisma of Prof. Bishop in particular, had a profound effect on the DFA. They then believed absolutely that this apparent combination of poor construction and "horns of high stress" at the frame 65 connections was the final proof beyond any doubt that this was the cause of the loss. In fact these stresses were unremarkable and mainly still water cargo loading effects.

By 1994 DFA had raised sufficient funds through the ITF to mount a survey to find and examine the wreckage. This they did in May and June 1994 (ITF, 1994; Mearns, 1995). Although the emphasis and conclusions were overly subjective and extremely biased, the mission did correctly conclude that whatever happened the loss was sudden and catastrophic.

The survey was valuable in identifying and locating the bow section and in suggesting the extent of the wreck field. Evidence of unexpected and extensive fragmentation and some brittle fracture of the hull was thought to be due to substantial implosion-explosion actions during sinking (now confirmed). There was speculative evidence of excessive corrosion of the fore deck plating (later disproved) and for the possible location of the stern section as Target 9 some 600 m from the bow (later confirmed).

1.4 Lord Donaldson's Assessment 1995

The ITF survey did provide sufficient new evidence to require further formal action. The starting point was Lord Donaldson's Assessment (*DERBYSHIRE*) in 1995 whose Terms of Reference in essence were:

- to assess what further work is needed to learn more of and, if possible, make a judgement about the cause of the loss
- for each option determine the likely costs, the probability of success and the benefits to ships' safety.

The two Technical Assessors appointed to assist Lord Donaldson were Professor D. Faulkner and Mr. R.A. Williams.

The lucid report speaks for itself (Donaldson, 1995) so only the FSA aspects will be mentioned. Lord Donaldson concluded that only a further, more extensive, but final examination of the wreck site would satisfactorily resolve the mystery. He considered the likely cost of about £2M to be fully justified because of the potential benefit to ship safety.

If it were not possible to determine the reason for the loss with a reasonable degree of certainty, the secondary objective was to learn more with a view to narrowing the field of possible causes. Lord Donaldson also recommended that possible abnormal wave actions should also be considered, based on the evidence and analyses presented in his Annex (Faulkner, 1995b).

1.5 Risk Assessment of Loss Scenarios

A FSA in reverse so to speak was used when assisting Lord Donaldson (Faulkner and Williams, 1996a and 1997). This determined and ranked in relative terms the possible initiating causes for the loss. A review was therefore made of service experience for the class and of casualty data for ships generally (Faulkner, 1995a and 1995c) and specifically for bulkers (Intercargo, 1995; Jones and Roe, 1991, etc.). Discussion with those familiar with bulker operations (Spyrou, 1997, etc.) targeted reading (Ramwell and Madge, 1992; Isbester, 1993; Jubb, 1995) also helped to formulate the judgements made. For example, Table 1 (Faulkner and Williams 1996b) summarises the percentages of total loss causes of bulk carriers, excluding war losses, in the calendar years 1960-94 (LR, 1995) and 1990-94 (Bureau Veritas, 1995).

An interpretation of this data suggests that cargo shift and capsize is very rare in big ships and that 30-35% of losses are likely to be due to inadequate structure. In this period no bulkers have broken in two at sea, although at least one lost its bow. Although none have lost their stern, serious cracking at the bridge front has occurred. Table 1 also suggests that navig-ational errors account for about 35% of losses, and fire and/or explosion accounts for about 20%. More than 75% of marine casualties are attributed to human error.

Table 1: Breakdown Percentages of Loss Causes for Bulk Carriers

Attributed causes	1960-94	1990-94
Possible hull damage	29.9	28.6
Wrecked or stranded	28.3	24.1
Fire and/or explosion	18.6	20.5
Collision	9.6	8.9
Missing unexplained	4.5	9.8
Machinery damage	3.9	3.6
Engine room flooding	3.4	4.5
Cargo shift	1.1	-
Total	100%	100%

From all this, two indices were judged on a scale of 1 to 5. These were the notional probability of an initiating event (P_i) and its seriousness (S_c) in terms of subsequent consequences. They were combined as a product to define a notional Risk Numeral for each of the 13 loss scenarios: $R_n = P_i \times S_c$ for both Normal wave actions, which correspond to normal design, and for Abnormal wave actions, which would correspond to rare "Survival Design" (Faulkner, 1997a; Faulkner and Buckley, 1997). The latter are given in Table 2.

The 13 loss scenarios are in three groups, the last group has three scenarios (C9, C10, C11) where the ship would be stationary, and for each the serious consequence numeral S_c = 5 (the highest level) in abnormal seas because the ship would become beam-on to the weather. As such, she would be very vulnerable to roll-induced damage to hatch covers leading to water ingress and foundering (Faulkner and Williams, 1996b). Capsize is not likely because of the very high transverse stability.

The highest Risk Numeral was 20 for Hatch Cover Collapse (C4) and this would now be 25 (with S_c = 5 instead of 4) as it has subsequently been found from dynamic calculations that *DERBYSHIRE* could not survive the two forward holds flooded in these seas.

The second highest risk numeral was for loss scenario C1 deck cracking at Frame 65, leading to separation of the aft end and supposed rapid sinking. For this R_n = 12 which is close to the assumed "intolerable" risk level of about 16 and is certainly higher than could reasonably be implied from the FI conclusions. This increase arose from the results of the abnormal wave time stepping simulations (Faulkner, 1995b) which suggested that part at least of the stern might come out of the water, as has also been experienced in similar ships - see, for example, the RINA Colloquium discussion. This then would induce a high tensile stress at Frame 65 where any overloaded poorly constructed weld connection might crack and provide the dynamic load trigger to reduce toughness and *initiate* a brittle fracture in the hull girder. Even then, the risk of continuous *propagation* can be shown to be small.

Table 2: Risk Numeral Components

Loss Scenarios	Abnormal Wave P_i	S_c	R_n
Primary Structure			
C1 Deck cracking Frame 65	3	4	12
C2 Deck cracking mid-sections	2	3	6
C3 Torsional weakness	2	1	2
Fore End Vulnerability			
C4 Hatch cover collapse	5	4	20
C5 Hatch attachment failures	3	2	6
C6 Fore deck collapse (corrosion)	3	3	9
C7 Fore peak flooding	2	4	8
Other Scenarios			
C8 Cargo shift/liquefaction	1	2	2
C9 Propulsion loss	2	5	10
C10 Rudder loss/steering failure	2	5	10
C11 Explosion/Fire in E.R.	2	5	10
C12 Pooping – from forward waves	2	2	4
C13 Pooping – running with the sea	3	2	6
C14? The unforeseen scenario – the sea often springs surprises			

Perhaps the only loss scenarios which would not in all likelihood allow time to launch lifeboats and/or to send a distress signal are C1, C4 and perhaps C7, C13.

An *a priori* Risk Matrix for the loss scenarios is given in Fig. 2. Those scenarios in the top right corner are considered to be "intolerable" and something needs to be done about these whatever happens. There is only one in that category, which is hatch cover collapse (C4), and for this reason papers were published (Faulkner, Corlett and Romeling, 1996 and Faulkner, 1997b) without prejudice to the outcome of the *DERBYSHIRE* investigation. Whatever the final outcome, hatch cover vulnerability must be regarded as a "near miss" for several B-60 bulk carriers. Figure 2 contains some downward pointing arrows which will be explained later as *a posteriori* adjustments to R_n arising from updated information from the Phase 1 survey of the wreck.

1.6 Ship Communications

The important messages to and from the *DERBYSHIRE* are presented in the FI report (1989). The last position report from the ship on 9th September at 0300Z was "vessel hove to violent storm force 11 wind NE x E seas approx 30 feet overcast continuous rain pressure 995 mb". In contrast, the m.v. *ALRAI* at about the same time and approx. 80 miles (north?) of the *DERBYSHIRE* reported "60-100 ft waves with wind force 12 and visibility nil and 962 mb".

The plot of the track of typhoon ORCHID by Ocean Routes, the weather routing agency for *DERBYSHIRE*, can be deduced from the FI report and compared with the very consistent tracks shown in its Appendix II from Tokyo, Guam and Hong Kong. In the period leading up the 8th September the Oceanroutes track was several hundred miles different from these, and might possibly have left Captain Underhill in a dilemma.

Mariners remarks at the RINA Colloquium (1996), at a recent RINA Conference (Evans et al, 1995) and from other sources do seem to suggest that weather routing may not always act in the best interests of ship safety due to economic emphasis on meeting charter dates and minimising fuel used.

2. FORENSIC ANALYSES FOR FREAK WAVE ACTIONS

2.1 Lateral Thinking

In 1995 the author was puzzled as to why, after so many man-years of intelligent effort, no loss scenario, including Frame 65, stood out as being likely. He wondered if previous investigators were restricted by applying conventional tools and thinking to explain the loss. By any standards the loss was *extraordinary* for a well found ship only four years old under the command of an experienced master.

The author's starting point therefore was to look for an *extraordinary* cause. He reasoned that nothing could be more extraordinary than the violence of a fully arisen and chaotic storm tossed sea. He therefore studied the meteorology of revolving tropical storms and freak waves (Coles, 1991; van Dorn, 1993 and Draper, 1964) and found that steep elevated waves of 25 m to 30 m or more were quite likely to have occurred during typhoon ORCHID (Faulkner, 1995b).

Later on it was found that *DERBYSHIRE* was not only trapped at about the very worst radius of the dangerous semi-circle of the typhoon, but that starting just three hours after her last message typhoon ORCHID executed three high-speed conditionally unstable cyclonic loops with increasing forward speed up to 30 knots toward the NW and North with rotational steady wind intensities reaching 75-80 knots (Cardone, 1987). This is illustrated in Fig. 3 which shows the envelope of highest rotational wind speeds and the last known and final wreck positions of the *DERBYSHIRE*.

It was therefore recognised that quite novel analyses would have to be undertaken to establish possible characteristics of the seas to which the ship might well have been subjected and then to examine the response of the ship to these seas so that the risks associated with the previously described loss scenarios could be better established.

2.2 Survival Waves

When working for the US Navy's Model Basin twenty years ago, Buckley first advocated new loading conditions for the primary structural design of ships (Buckley, 1978). This was followed by work for the Ship Structure Committee (Buckley, 1988) which advocated extreme climatic wave spectra for more general structural design, including wave impact design. In parallel, the 1979 ISSC Environmental Conditions Committee I.1 defined very similar waves of limiting steepness by:

$$T_p = 3.6 \sqrt{H_S} \qquad (1)$$

This was based on the steepest boundaries of global wave scatter diagrams and is very close to Hogben's contemporary work (see Hogben et al 1986, and Hogben 1990). Following the work for Lord Donaldson these two independent pieces of work were brought together (Faulkner and Buckley, 1997) and *survivability* and *operability* design wave envelopes of H_s vs T_p were presented - see Fig. 4.

To define the *abnormal waves* to be used for Lord Donaldson's Assessment (Faulkner, 1995b) H_s = 14 m was assumed based on FI data for typhoon ORCHID during the 24 hours following the ship's last message. For primary bending studies eq(1) was used for defining T_p, but for roll-induced actions:

$$T = 3.2 \sqrt{H_S} \qquad (2)$$

was used based on contemporary conditional probability data (Dahle and Myrhaug, 1995). The range of wave lengths considered for use with these wave periods was:

$$16\ H_s \le \lambda \le 20\ H_s \qquad (3)$$

The low probability extreme wave height for survival design (H_d) and the asymmetry parameters which are important for ship damage and flooding are:

$$H_d = 2.5\ H_s \ge 25\ \text{m} \qquad (4)$$

$$= 0.65\ ,\ m_f = 0.6 \ \text{and}\ m_b = 0.4 \qquad (5)$$

These parameters were based on wave profile data measured during hurricane CAMILLE in 1969 (Buckley, 1983 and 1991) and have recently been supported by numerical simulations (Drake, 1997) and experimentally (Clauss, 1998) where H_{max} = 2.56 H_s, very close to eq(4). See also Kjeldsen (1984), Myrhaug and Kjeldsen (1986), and Gaythwaite (1981) who explains why young RTS waves become so steep. Figure 5 shows a TS from a steep, elevated wave record and a ship encountering one (Buckley, 1983). Currents which oppose waves also steepen them. Mariners frequently referred to such waves as *"walls of water"*.

Pyramidal waves are a feature of cyclonic RTS storms. These migrate away from the tropics, sometimes drawing in further energy from other nearby depressions. The fore deck damage to QE2 in September 1995 is an excellent example arising from hurricane LUIS moving NE off Newfoundland's Grand Banks (Lloyd's List, 1995). A wave height of nearly 30 m has been confirmed (MAIB, 1997). Eilersen et al (1989) point out that the spilling breaking limit is H = 2.9 H_s. This is a vital subject related to weather deck impact damage, as shown in Appendix 1.

The above wave heights and asymmetry parameters were provisionally suggested as being appropriate for survival design of ships having L ≥ 150 m say. However, casualty data and logic suggest that smaller ships are likely to be troubled by lower height waves which occur more often. Taking note of an excellent report by Bales (1982) on designing for the (extreme) environment it is *provisionally suggested* that the above equations are considered when examining critical survival design conditions but where H_s is chosen as a function of ship length (m):

$$H_s = 15 - (3 - L/100)^{2.5} \qquad (6)$$

Over the range 75 m ≤ L ≤ 300 m H_s varies from L/10 to L/20. For smaller vessels H_s = L/10 is suggested - a little more severe than L/12 (Spencer, 1975). For larger trading ships or moored FPSOs higher values of H_s than 15 m, may need to be considered and Fig. 4 may be used as guidance. For example, for designs West of Shetland one oil company is proposing H_s = 18 m.

Probabilities

Assuming that individual wave heights in each sea state follow a Rayleigh distribution, and noting that extreme storms are reasonably narrow-banded, Longuet-Higgins (1952) derived a time dependent probability distribution for *maximum* wave heights in a short term stationary storm for which:

$$H_m = H_s\,[0.5\ \ln N]^{0.5} \qquad (7)$$

$$H_e = H_s\,[0.5(\ln N - \ln(-\ln(1-p_e)))]^{0.5} \qquad (8)$$

For D = 12 hours, H_s = 14 m and T_p = 13.5 s this gives the most probable extreme wave height H_m = 28.1 m with a 63.2% probability of being exceeded. Ochi recommends a value of p_e = 1% for design, which in this survival extreme case is H_d = 2.52 H_s which is virtually the same as eq(4). Low probability values are:

$p_e = p_e(H)$	%	25	10	5	3	1
H_e	m	30.2	31.8	32.9	33.7	35.2

It can be argued that equations (7) and (8) may not be appropriate for RTS waves because they are not narrow banded. However, until ε exceeds 0.9 for very wide banded processes the differences are no more than 5% using Ochi's band width dependent equations (1990). Moreover, as he points out, for a given period D the number of peaks for a non-narrow-banded spectrum is much larger than for a narrow-banded one. Overall, it is therefore felt that eq(8) can be used unmodified, and this was supported by Hogben (1997) who also provided evidence to support the use of eq(4) for design.

From eq(8) it is useful to derive p_e in terms of any H and N:

$$p_e(H) = 1 - \exp(-\exp\gamma) \qquad (9)$$
where $\gamma = 1nN - 2(H/H_s)^2$

The probability density function of the extreme process can then be derived by differentiating $F(H) = 1-p_e(H)$:

$$f(H) = (4\,H\,e^{\gamma}/H_s^2)\,(1-p_e(H)) \qquad (10)$$

This is shown with p_e in Fig. 6 for N = 3200 (D = 12 hours) and H_s = 14m. These equations are now used where appropriate to assist the assessment of loss scenarios C2, C4, C7 and C8-C11.

2.3 Ship Bending (C2)

Frieze et al (1991) presented a comprehensive, informative and important case study of the structural reliability of the ultimate and fatigue strengths of a FPSO having L = 194.2 m, B = 32.0 m, $C_b \cong$ 0.81 and Δ = 51,430 te over 1, 20 and 100 year exposures in the northern N. Sea. Short term storm data was used in which H_s = 15 m and T_z = 12s. Assuming $T_p \cong 1.4\,T_z$ = 16.8 s this lies close to the left hand boundary of Fig. 4 which corresponds to survival waves of limiting steepness given by eq(1).

TS simulations of the wave data derived long-crested wave profiles as the sum of 100 regular wave components having statistically independent phases. Using a non-linear strip theory and taking added mass and damping at a 12s wave period to correspond to wave lengths about the length of the ship where maximum response could be expected, a time domain simulation was performed 30 times, each covering 5 minutes = 25 wave passages. Figure 7 represents the simulated wave profile and ship position at the instant of maximum sagging moment. The wave height for maximum sagging is H = 15.8 m approximately. The results were converted to long term most probable sagging and hogging bending moments which compared well with extensive full scale measurements.

These moments have been compared (Faulkner, 1998b) with the unified IACS S11 requirements for wave-induced bending moments (Nitta et al, 1992) which are 1937 MNm in sag and 1795 MNm in hog. Over 2 storm duration's of 18 minutes (90 maxima) and 3 hours (900 maxima) the ratios of the derived wave bending moments to the IACS standard values were:

Duration	18 mins	3 hours
M sag / IACS	1.3 (1.2)	1.8 (1.5)
M hog / IACS	1.1 (1.3)	1.4 (1.6)

The values in brackets are those derived using linear strip theory transfer functions. Values such as 1.8 and 1.4 must surely be of interest if not concern.

Similar, but necessarily much more approximate extended static balance (Thomas, 1968) analyses of the m.v. *DERBYSHIRE* encountering a steep elevated wave 30 m high x 260 m long were undertaken (Faulkner, 1995b). Figure 8 shows three time-steps of the vessel, the second and third steps showing approximately the worst sagging and hogging wave-induced moments of the ship. These were judged to be more than twice the IACS standard of 6,353 MNm sag and 5,985 MNm hog values and were reported (Faulkner and Williams, 1996b) where C = 10.67 m. IACS Z min = 57.75 m3 whereas for the ship Z deck = 58.34 m3 and Z keel = 84.65 m3 amidships. The pitch attitude of the ship corresponded reasonably well with observations made during tests on a 1/50 scale model (DMI, 1985).

Some surprise was expressed in discussion (Rainey, 1997), so a slightly more sophisticated approach covering three wave lengths 0.9L, 1.0L and 1.1L was undertaken. This gave a maximum wave-induced sagging moment = 1.8 x IACS standard and a value 1.4 for hogging, which fortuitously corresponds exactly with those presented above. All that is suggested is that these type of TS studies should be undertaken more rigorously as the results are potentially important for survival analyses of ships.

2.4 Hatch Covers (C4)

2.4.1 General Features

Figure 9 from Faulkner (1997d) shows the plan view of a typical pair of hatch covers and main fittings. Most covers were 14.95m long x 11.0 m wide. The ten dotted lines parallel to the X (ship) axis are fabricated longitudinal Tee beams spaced 994 mm apart with maximum depth 635 mm at mid-length and tapered to 483 mm at the fore and aft ends for drainage and flanges 280 mm x 25 mm. They are fillet welded to the web of the centre girder.

The three dotted lines parallel to the Y (transverse) axis are "girders". The centre girder is 920 mm deep with a small 75 mm x 25 mm flange on which the drive rack is mounted. The two side girders are intercostal, of depth 560 mm and flange 100 mm x 25 mm. All webs and plating are 10.5 mm thick with fillet weld throat thickness of 3.5 mm. The oil tight covers are secured to the hatch coamings by 100 cleats, approximately 0.5 m apart attached to snugs on each fore and aft side plate. Also shown are several top side and end plate features to aid recognition between port and starboard covers. Port covers have sockets for guard rails. Unfortunately the usual hatch cover identification numbers had been painted over. The only recognition features were the helicopter roundels painted on the 2 covers of no.3 hold and on no.8 starboard cover. No.3 hatch covers were 0.23 m shorter than for those given above for covers 3 to 9; no. 2 covers were 2.29 m shorter (which did aid recognition).

2.4.2 Strength Assessments

Regulation 16 of the 1966 ICLL (Murray-Smith, 1969) required forward hatch covers (0.25L) to withstand green seas by designing for:

- uniform pressure (static) not less than 1.75 tonnes/m^2
- level of stress not to exceed minimum UTS/4.25

Along the rest of the ship the load is reduced to 1.2 tonnes/m^2. A limiting plate thickness for mild steel is b/100 or 6 mm.

For low strain collapse the UTS criterion is quite irrational, and yet still exists. For mild steel this gives a maximum design stress of 0.40 σ_o. With s ≅ 1.25 for well designed fabricated Tee sections the plastic collapse load factor for a simply supported hatch cover (no end continuity of stiffeners) is then about 1.25/0.4 = 3.1 leading to a minimum expected collapse head of p_u = 3.1 x 1.71 m = 5.3 m of sea water.

At the time of the FI two estimates were made for the inelastic collapse head p_u of the covers which were both about 4.1 m. However, the Lord Donaldson work (Faulkner, 1995b) found that, even allowing for the tripping brackets, the three deep transverse "girders" were essentially ineffective with $M_t \cong 0.2\ M_p$ and low bending strength because of their narrow flanges (face plates). Moreover, because for the main longitudinal stiffening A_s > bt (= 1.2 bt on average), the bending induced compression in the thin welded plating of the covers is substantial and reduces its effectiveness substantially as bending increases to b_e = 0.41 b at collapse. Making a more complete allowance for these adverse features and for welding stresses (Faulkner, 1975) the lowest plastic collapse load is:

$$p_u = 8\ Mp/L^2 b \qquad (11)$$

which is = 3.7 m with plastic hinge collapse of the longitudinals at their mid-lengths (centre girder) referred to as Y mode failure. This compares with values of 4.0 m to 4.3 m of sea water head from non-conservative analyses at the time of the FI. The analyses also showed that there were several modes of plastic collapse quite close to the 3.7 m arising from the taper in the longitudinals.

Although this is only 0.75 of the strength of a well designed cover (5.1 m as above) this should not be taken as an indication of bad design 30 years ago. Regrettably, although much was then known and practised in other disciplines about compression strength of thin plating and inelastic collapse of structures, this was not the case in marine design. Moreover, although the stiffeners in the covers in *DERBYSHIRE* comfortably met the maximum stress requirements of the 1966 ICLL, they were, like many others of their time no doubt, quite inefficient load bearers. This is a good example of the weakness of working stress design methods which make no reference to any consideration of real collapse. Maximum stress is often a very poor indicator of collapse capability, and yet its influence is still dominant. It is of course vital in fatigue considerations.

2.4.3 Dynamic Collapse

6 or 7 of the 18 wrecked covers showed unexpected X mode bending or tearing between longitudinals (Williams and Torchio, 1998a). This may have been caused by plunging green sea wave actions over part of the covers. A necessarily crude elasto-plastic analysis was therefore undertaken during the final survey assuming three different uniform load imprints of 30 m^2 (18%) spread along the central spans of longitudinals 2, 3 and 6, as illustrated in the port cover of Fig. 9. This gave an average p_u = 5.0 m from:

Imprint size (m x m)	15x2	7.5x4	5x6
Pressure head (m)	4.4	4.9	5.6

But the assumptions made were so approximate that a more rigorous inelastic FE analysis was recommended with dynamic load signatures and several imprints, such as the circles shown in Fig. 9. It is understood this was initiated but the results were inconclusive.

An unfortunate feature of A grade mild steel is that if the initial pressure pulse is steep (milliseconds) then brittle fractures are possible. This may arise from the *gifle* peak associated with water impact, and there is evidence of such cracking and tearing in the wrecked hatch covers. However, the implosion-explosion actions during sinking (see later) promote such fractures and makes interpretation less certain.

2.4.4 Wave Profile Loads

The first TS step in Fig. 8 shows the fore end of the ship about to be swamped by a simulated steep elevated wave. Figure 10 shows the quasi-static wave profile loading on the forward hatch covers. No allowance is made for the usual bows down attitude induced by the long troughs which leads heavily laden large low freeboard ships to plunge into the oncoming steep crests rather than rising to the sea. Nor is the dynamics of sea waves considered (see Appendix). With these non-conservative assumptions a simple model for the peak and average pressure heads are:

$$h = \alpha H - (F + C) \qquad (12)$$

$$h_b = h[1 - mL/4h]\ ,\ L \leq 2\ h/m \qquad (13)$$

where α = 0.5, m = 0 for linear waves and α = 0.65, m = 0.5 was assumed for the steep elevated waves of typhoon ORCHID, and is the mean of the crest face and back slopes. F = 6.9 m and average C = 2 m. Surprisingly good agreement was found for waves up to 25 m high between eq(12) and the mean hatch cover peak pressure measurements on a 1/50 scale model of the *DERBYSHIRE* during seakeeping tests (DMI, 1985 and Faulkner, Corlett and Romeling, 1996).

We are now in a position to evaluate peak and average pressure heads acting on no. 1 hatch cover which has L = 14.72 m. These are shown for a range of wave heights together with the probabilities of exceedence for D = 1 hour, 3, 6 and 12 hours.

Table 3: Hatch Cover Pressures and Probabilities

H	(m)	20	22	24	26	28	30
Linear waves:							
$h = h_b$ (m)		1.1	2.1	3.1	<u>4.1</u>	5.1	6.1
Non-linear waves:							
h	(m)	4.1	5.4	6.7	8.0	9.3	10.6
h_b	(m)	2.3	3.6	<u>4.9</u>	6.2	7.5	8.8
Probabilities p_e							
D =	1 hr	0.99	0.85	0.53	0.23	0.09	0.03
	3 hrs	1.00	1.00	0.89	0.55	0.24	0.08
	6 hrs	1.00	1.00	0.99	0.80	0.42	0.15
	12 hrs	1.00	1.00	1.00	0.96	0.66	0.28

<u>Notes</u>:
(1) mean pressures above $p_u \cong$ 4 m are underlined
(2) for non-linear wave H = 22.7 m, h_b = <u>4</u> m
(3) in practice the waves will be a mix of nearly linear and clearly non-linear form. On advice for RTS storms the probabilities should be biased toward the non-linear values. A bias of 75:25 is suggested.
(4) for H = 35 m p_e = 1% over 12 hours and h_b = 8.6 m for linear waves, h_b = 12.0 m for non-linear waves.

2.4.5 Beam Sea Risks

Loss scenarios C8 to C13 in table 2 all lead to loss of propulsion or steering and the ship falling beam on to the sea. The laden *DERBYSHIRE* was very stable with GM ≅ 8 m and a natural roll period of about 11 s. With H_s = 14 m eq(2) gives a steep fronted wave period of T = 12.0 s. Although the capsize risks are small, these figures do suggest that vigorous rolling might well occur in beam seas, especially with the possibility of tandem waves.

The more serious risks then for low freeboard stable cargo carriers are likely to be the effects of steep, elevated breaking or near-breaking waves on the side structure, deck and hatches (see Appendix 1). For *DERBYSHIRE*, the sides are of double skin and the deck strength varies from about 40 m to about 80 m sea water head. However, as we have seen, and in complete contrast, a 4 m pressure head would burst the hatch covers. It is therefore recommended that apart from the forward hatch covers, those along the length of the ship should be designed to a substantially higher uniform sea water pressure head of say 3.0 m, not the 1.2 m as at present. This suggestion is supported by many mariners.

2.4.6 Hatch Coaming Risks

Green sea impact on flat vertical structure is a subject badly in need of more research (Meyerhoff et al, 1994). Damage to bridge fronts, bulwarks, coamings and seals, deck fittings, etc. continues to occur. Faulkner and Buckley (1997) reviewed earlier Japanese research (Kawakami, 1969; Suhara et al, 1973), more recent model tests (Graham, 1988; Zhu, 1995) and theoretical research (Korobkin, A, 1994; Buchner, B., 1995) and for the present favour for design pressure (p_d) the results of reputable experiments represented by:

$$P_d = C_p \ 0.5 \rho v^2 \qquad (14)$$

where v is the relative velocity. For the *gifle* peak pressures which typically have a 2 to 10 millisecond duration, the results from essentially normal flat impacts provide C_p values from about 10 to nearly 400, with 65 perhaps being a reasonable average. This phase of the impact is certainly relevant for brittle materials. However, it is the longer follow on *bourage* or momentum transfer phase which is more relevant for structural damage in ductile materials and mean C_p values are lower, varying from about 1 to 10. See Fig. 14(b). Present provisional recommendations are:

- C_p = 9 for normal plating design where plate widths b are around 1 m, or a 5% upper bound value of C_p = 15 could be considered for plastic hinge collapse design with a low safety factor
- C_p = 3 for stiffened panel design loads
- Higher values of both are expected close to bulwarks and other re-entrant corners.

Recent Norwegian research (Kvasvold et al, 1997) provides a somewhat different more analytical approach which looks promising for predicting peak stresses in stiffeners.

The C_p approach was used for predicting the pressures on coamings where v = 1.2 c + ship speed. The 1.2 is to allow for flow augment from wind and channelling. Average derived values for the waves used in the DMI tests on the stationary *DERBYSHIRE* model were 226 kN/m^2 which agreed reasonably well with the 203 kN/m^2 measured, but only when C_p = 1.

The maximum pressures estimated for the *DERBYSHIRE* were 327 kN/m^2 = 32.5 m head. This is very close to the coaming plate plastic hinge collapse between stiffeners as estimated by LR. The stiffeners themselves are also vulnerable, so there is a real risk of substantial deformation of the coamings in such storms, especially from near breaking waves (see Appendix 1).

2.4.7 Casualty Evidence

Some evidence, mainly from LR casualty reports, of hatch cover weaknesses was presented at the FI and later by Byrne (1995). It can be seen from Table 1 of the Faulkner, Corlett and Romeling reference (1996) that in the period 1969-87:

- 8 OBO and bulk carriers were almost certainly lost directly due to heavy weather breaching the hatch covers and/or coamings, or possibly to the loss of the covers
- 12 other vessels were lost by mostly forward flooding in heavy weather, caused potentially by, or by contribution of, the breach or loss of hatch covers
- 6 of these 20 ships were lost in the W. Pacific in the winter 1980/81
- the average age of these 20 bulk carriers was about 14 years, their averaged deadweight was 35,700 t, and average lives lost 23
- these 20 ships represent 16% of the 128 bulk carriers lost over the period.

It must be stressed that the evidence is far from complete. Two cases of coaming failure are cited in the Table. It is of interest to note that from the wreck of the *KOWLOON BRIDGE*, sister ship of the *DERBYSHIRE*, it seems that coaming failure also occurred, which could perhaps explain her noticeable trim down at the bow on completion of her Atlantic crossing.

Over the last eight years 108 bulk carriers and combination carriers of average age 19.2 years have been lost (LR, 1998). Nearly 30% of 87,500 DWT average were in iron ore and sank in heavy weather and 11% of 94,375 DWT average sank with no details, as with the *DERBYSHIRE*. These losses continue at an intolerable rate.

It is quite possible that some of the many unexplained heavy weather losses may have been caused by hatch cover or coaming failures because fore end plunging due to flooding of large holds can be rapid (Brown, 1997). Jones and Roe (1991) claim that 70% of bulkers are lost in very heavy weather.

To these losses would have to be added the well documented loss of the *CHRISTINAKI* in 1994, and perhaps the *DERBYSHIRE* and *LEROS STRENGTH* when their formal investigations are complete (Aftenposten, 1997).

2.4.8. Improved Design of Covers

The simple beam equation (11) is unusual for a grillage and arises because the three cross girders are ineffective. It is then much more efficient to place the load bearing stiffeners across the shorter span. By eliminating all three girders and replacing the 10 longitudinals by 14 similar cross-section transverse beams it can be shown that this simpler structure is slightly lighter, certainly cheaper to construct, and yet is 85% stronger. The collapse head is then p_u = 7.0 m which is 33% better than the minimum (5.3 m) which could reasonably be expected from the ICLL requirements!

However much of an improvement this may seem, it still leaves an inefficient stiffener-plate cross-section. Taking p_u = 12 m as the minimum collapse head recommended for no. 1 hatch cover, a much more efficient design has been generated which is about 12% heavier than the original but is significantly cheaper to construct because it is a beams only design. The weight estimate applies only to the top plate and its stiffeners. The weight of the four end plates and associated stiffening could remain unchanged.

This design has 15 mm plating and nine beams, eight of 680 mm total depth and the centre one incorporating the drive rack would be deeper to provide the drainage taper. Because of the uncertainty as to where a plunging breaker might act, a simple beams only design would appear to be attractive.

2.4.9. New Strength Criterion

Table 3 data and casualty evidence provide a compelling case to make hatch covers much stronger, and it is gratifying to note that some class societies have offered increased requirements.

Following a more complete review (Faulkner, Corlett and Romeling, 1996) it was recommended that with the existing stress-based criterion, hatch covers for nos. 1, 2 and 3 holds should be designed for sea water heads of 4.5 m, 4 m and 3.5 m respectively.

It is demonstrably more rational to base the pressure design criterion on an ultimate collapse approach which should be demonstrated for approval. On this basis it is now provisionally suggested that with a load factor against collapse of 1.5 the design heads of sea water for hatch covers 1 and 2 are set at 9m and 7.5m, and at 6m for no. 3 and all other covers. These requirements should be mandatory and not optional.

These values imply less than 1% probability of collapse during a 12 hour exposure to the dangerous semi-circle of a severe typhoon like ORCHID, but includes some allowance for in-service corrosion.

2.5 Fore End Flooding (C7)

The *SIR ALEXANDER GLEN*, a sister ship, experienced severe weather damage to fore deck fittings and moderate flooding of fore peak spaces. *DERBYSHIRE* herself had lost one ventilator head. The RINA Colloquium discussion brought to light several other similar cases for other ships. Statistics also show that by the middle 80s the annual incidence of heavy weather damage forward to bulk carriers had increased tenfold as compared with the incidence immediately following the 1966 ICLL when freeboard was reduced.

Because of all this a middle level risk numeral $R_n = 8$ was allocated to C7 loss scenario for the *DERBYSHIRE* as an initiating event. The final survey found that 3 or 4 of the 0.5 m diameter mushroom vents (MVs) to some of the fore peak ballast tanks (2,869 m^3 total) were damaged and open to green seas. The 0.9 m x 1.2 m hatch cover giving access to the Bosun's store (686 m^3) is missing. The official report also refers to the possibility of an engineers' spaces having a damaged ventilator. These spaces are not watertight so the whole stores flat is included (1200m^3)

2.5.1. Philosophy

Figure 11 shows a quasi-static conservative idealisation of a sinusoidal wave passing over an orifice on the fore deck. This is similar to the Fig. 10 approach adopted for hatch cover pressures. Most important, it uses a similar pressure head and probability modelling so that the results may be compared even though one may argue over the numbers. Non-linear waves are also considered to provide a weighted probability solution (Faulkner, 1997e).

The second important point to note is the fundamental difference between the two events. The first is the bursting of the hatch covers by the first occurrence of a single sufficiently high almost certainly non-linear wave, and the second event is the slow ingress of water from hundreds of linear and non-linear waves passing along the ship at about 267 per hour (1 hour/T_p). In reliability theory the first is referred to as a *first passage* or *out-crossing* from a safe region single event, and is analogous to ultimate back breaking of a ship. The second phenomenon is referred to as *up-crossings* of a *threshold level* event, more analogous to fatigue damage. Truncation at critical wave height threshold levels is then required, as will be seen. Figure 12 is an attempt to illustrate these differences.

2.5.2. Moving Wave-Orifice Theory

Orifice theory is surprisingly complex. For small sharp edged orifices (and there's the catch) under gravity head flow c_d is usually in the range 0.6 to 0.65 (Massey, 1970). No experimental data could be found for parallel flow through vertical tubes (MVs) or rectangular (hatch) coamings. Although it was recognised that at low orifice Reynolds numbers c_d would reduce (for a < $5\sqrt{A_o}$? (Marks, 1979)) this was ignored for simplicity and to be conservative and a value $c_d = 0.6$ was used. Under gravity flow the mean downward velocity through the orifice is:

$$v = c_d\sqrt{2ga} \qquad (15)$$

where a = ζ - (F+C) is the time varying water head from the crest profile over the period t_1 when water ingress starts to t_2 when it finishes. The total volume passing through the orifice during the passage of a single wave crest of peak height h above the orifice is then:

$$\begin{aligned} V_h &= \int vA_o\, dt \\ &= c_d A_o\sqrt{2g}\int_{t_1}^{t_2}\sqrt{a}\, dt \end{aligned} \qquad (16)$$

2.5.3. Linear Waves

For a linear sinusoidal wave of amplitude:

$$\varsigma = (H/2)\sin(2\pi/Tp) \qquad (17)$$

it can be shown from symmetry that eq(16) becomes:

$$V_h = 2c_d A_o\sqrt{2g}\int_{t_1}^{T_p/4}\sqrt{\varsigma-(F+C)}dt \qquad (18)$$

where:

$$t_1 = \left[\sin^{-1}\left(\frac{F+C}{H/2}\right)\right]\frac{T_p}{2\pi} \qquad (19)$$

C = 1.0m was taken as the mean height above the deck of the MV and other orifices considered so that F+C = 6.9 + 1.3 = 8.2. The integration of eq(18) was the performed numerically over a truncated wave height range from H_1 to H_2 where:

- H_1 provides the minimum wave height for steady water ingress to take place so H_1 = 2 (F+C) = 18 m
- H_2 relates to the peak head of water h ≅ 4 m above which hatch cover no. 1 would certainly burst; from Table 3 for linear waves an upper threshold of H_2 = 26 m is taken.

For these extreme waves f(H) is zero at H = 21 and below so the limits of integration were taken from H = 22 m to 26 m and the results are:

H	(m)	22	23	24	25	26
Integral	(√m s)	2.04	2.37	2.67	2.97	3.29
N_H=f(H) N		7	36	93	143	153

During any required period D the most probable number of waves passing along the ship are N = D/T_p. For example, for D = 3 hours N = 800 and if this is multiplied by the pdf f(H) from eq(10) this provides an estimate of the waves N_H in each of the δH = 1 m wave bands as illustrated in the Table above. The total sum of these waves is 432, a little more than half, and allowing for round off errors this is reasonably consistent with the most probable wave height for D = 3 hours from eq(7) being 25.6 m with a 63.2% probability of being exceeded. It follows that we can combine the integration with f(H) to give the total volume of water ingress from N waves as:

$$V_{iD} = 2c_d A_o N\sqrt{2g}\sum_{H_1}^{H_2}\int_{t_1}^{T_p/4}\sqrt{a}\quad dt.f(H)\delta H \qquad (20)$$

Then the flow rate $V_i = V_{iD}/D$ where D is related to N.

2.5.4. Abnormal Waves

If the crest of pyramidal and other steep, elevated waves are idealised as a triangle, as in Fig. 10, then local crest amplitude a is a linear function of time up to the passage time t_o for the crest to pass over the orifice. The integration of eq(16) then reduces to a simple closed form:

$$\int_{t_1}^{t_2} \sqrt{a}\quad dt = \tfrac{2}{3}\sqrt{h\quad t_o} \qquad (21)$$

where $t_o = L_o/c$ and by geometry $L_o = h[m^{-1}_f + m^{-1}_b]$, c is wave celerity $gT_p/2\pi$ and h is given by eq(12) where $\alpha = 0.65$ and (F+C) = 8.2 m. Based on Dahle and Myrhaug (1995) an average value of m = 0.25 is taken for m_f and m_b so $L_o = 8$ h. Applying all this to eq(16) the volume of water ingress from one crest is:

$$V_h = 47.4 \frac{c_d A_o}{T_p \sqrt{g}} h^{3/2} \qquad (22)$$

Then, summing this ingress for the number of crests in each $\delta H = 1$ m wave height band the total volume entering the ship in time $D = NT_p$ for each orifice area A_o is:

$$V_{iD} = 47.4 \frac{c_d A_o N}{T_p \sqrt{g}} \sum_{H_1}^{H_2} h^{1.5} f(H) \delta H \qquad (23)$$

This can be compared with eq(20) for linear waves.

2.5.5. Mixed Wave Calculations

For D = 3 hours (N = 800) equations (20) and (23) lead to water ingress rates of $V_i = V_{iD}/D$ of:

V_i = 1802 A_o m^3/hr for Linear waves
= 164 A_o m^3/hr for Abnormal waves

The much higher V_i values for normal linear waves than for abnormal waves is due to two factors. For example, for H = 25 m:

- the longer flatter crest gives an area over the orifices 3.0 times greater
- the area under the pdf(H) from H = 21 m to H = 26 m is nearly 17 times greater, as can be appreciated from Fig. 6.

The decision for a 75:25 probability mix of abnormal and normal waves is because for non-narrow-banded spectra, such as occur in RTS storms, the number of elevated peaks (maxima) is much larger than for narrow-band spectra (Ochi, 1990). One other justification is mentioned later. With this 75:25 mix:

V_i = $\underline{574\ A_o}$ m^3/hr (24)

It is important to understand that since f(H) varies with the number of waves considered then V_i will also vary with D the time considered. Table 4 illustrates the filling rates and times (T_f) for the two fore peak spaces considered and the resulting loss of freeboard (δF) at no. 1 hatch cover given by:

$$\delta F = m[Tpc^{-1} + (L_t / L) L_m Mct^{-1}] \qquad (25)$$

where m is the flooded mass in tonnes and for T = 18 m level draught Tpc = 116 te and Mct = 2330 te m.

Table 4: Flooding of Fore Peak Spaces (D = 3 hrs)

Spaces and Tanks	Ballast Tank	Bosun's Stores Flat
Volume (m^3)	2869	1200
Orifice no./diam & type	3/12" pipes	4/20" MVs
A_o Area of orifice (m^2)	0.219	0.811
V_i (m^3/hr)	126	465
T_f (hours)	22.8	2.58
% full in 3 hours	13%	100%
δf No.1 hold when full (cm)	91.3	37.9
δf in 3 hours (cm)	12.0	37.9

Note: Table 4 & 5 have been updated since original printing and Table 6 has been added.

It follows that in 3 hours the loss of freeboard is about 50 cm if all spaces are open to the sea. In terms of the already very high hatch cover collapse risks (Table 3) such effects are secondary, and are not essential to cause no. 1 hatch cover to collapse (see Appendix 2).

2.5.6. Truncation Effects

The upper truncations of H_2 = 26 m and 23 m adopted in these analyses are somewhat artificial because they ignore the possibility of higher waves which would certainly breach no. 1 hatch cover. Nevertheless, their probabilities of occurrence (= p_e) are quite real and unacceptable as can be seen from the lower part of Table 3. The situation is even worse because the quasi-static approach adopted completely ignores the adverse effects on hatch cover loads of ship motion and the dynamics of plunging waves and other green sea effects.

2.5.7. Flooding the Forward Fuel Tank

Section 4 of the UK/EC Assessors' report makes much of the almost total lack of implosion effects in the bow section. In 4.58 it therefore presumes that the bow became almost full at the time the ship sank. This is followed by much unsupported speculation which attempts to explain how the necessary flooding could have happened. In this assessor's judgement, the most unconvincing of these speculations relates to the filling of the deep fuel oil tank in the bow, which is discussed in the section 2.7.

The UK/EC Assessors' report gave a fore perpendicular (FP) trim of 2.5 m from bow flooding, but this has been criticised, no doubt because it is unrealistic and is badly defined. It is now checked. Taking the Assessors' judgement that about 2000 tonnes of fuel oil remained in the forward tank this would lead to a maximum water entry mass of 3240 tonnes. Using eq(25) this leads to a reduction ΔF at the FP of $\underline{1.15}$ m. Adding this to the summation from complete flooding of the fore peak spaces (mentioned in Table 4) gives a total reduction of bow freeboard of 2.3 m which is close to the Assessors' value. This draught reduction would be about 2.0 m at no. 1 hold.

2.5.8. Ship Motion Effects

The effects of water ingress at the bow on ship motions was briefly investigated. The total volume of water ingress into the two fore peak spaces in three hours is estimated from Table 4 to be about 1580 tonnes. This increases I_θ by about 3.4% which would reduce the maximum pitch motions to about 1%, equivalent to a reduction in pitch induced-bow trim of about 10 cm for a $\pm$ 2.5° normal pitch. This would change to about 25 cm reduction if the two spaces were completely filled and 41 cm if the deep fuel oil tank was also flooded.

Because of their high inertias and natural pitch periods, these large ships do not rise to the waves, as appropriately experienced masters have also confirmed. They tend to bury into them.

2.6 Cargo Shift (C8)

Table 2 shows a very low risk numeral of 2 for cargo shift because:

- there was negligible supporting evidence
- the ship was very stable with very low likelihood of capsize
- the FI considered a 6° list from progressive movement of moist ore to be doubtful and not likely to be the prime cause of the loss
- any significant cargo shift would have taken time and would almost certainly have been reported in the circumstances.

To support the first point, Table 1 shows about 1% loss in 35 years. In support of the last point, four ships reported cargo shift in the years 1978-87 and sent distress messages. Three were lost, one was successfully towed into harbour, and all crew on all ships were safely evacuated (Faulkner and Williams, 1996a).

During the Lord Donaldson work it was postulated that through damaged coamings and or seals it was possible that the top layer of ore in the hold could become saturated and mobile. Assuming an ore density of 5,100 kg/m^3 and a ± 20° harmonic roll at 11 and 12 s periods side impact "punching through" calculations based on eq(14) and C_p = 3 gave rise to a side pressure of 122 kN/m^2 which was no danger to the double skin *DERBYSHIRE*. Older, single hull vessels would be vulnerable to this type of loading which should be investigated.

Later, progressive cargo shift calculations based on earlier work by Skinner (1987) showed that for untrimmed cargoes in partly loaded hulls, a list of 8° or a little more might develop. Again, in discussion with mariners, it was felt that such lists would have been reported. A review of the UK research on the topic (summarised for the IMO by Kruzewski, 1992) was undertaken for the DoT (Faulkner, 1997c).

It would appear from these calculations that the current trend toward homogeneous loading of less height cargo in all holds could lead to a greater chance of cargo shift. This would be aided also by creating even stiffer ships as a result of the lower cargo heights, and hence brisker rolling. Using high WBTs to lower GM reduces available deadweight which would be unpopular.

2.7 Sinking Actions

Following the most likely loss scenario C4 of breaching of no. 1 hold and plunging by the bow, there are four actions which require some examination. Chronologically they are taken in order here.

2.7.1. Cargo Shift Actions in No. 1 Hold

Figure 13 shows a sketch by Robin Williams of the bow. There is a very long horizontal split in the single skin collision bulkhead about 8m above the hold floor level which follows a butt weld right across the ship. Unfortunately, it was not possible to examine the fracture surfaces in any detail. But it is a straight line fracture and is likely to be brittle in parts at least. Bulkhead 339 is "substantially bowed inward" toward the deep fuel tank in places and above this fracture.

A likely explanation for this, which could also go a long way toward explaining why the forward deep fuel oil tank has not imploded, is associated with a ship motion-induced dynamic slide forward of saturated ore concentrate following the collapse of no. 1 hatch cover. The possible collapse of the forward hatch coaming, as described in C14? and in Appendix 1, could also lead to ore mobility. The resulting dynamic impact could well cause the straight line fracture, especially if, as is likely, the stress front has a sharp rise.

Figure 14(a) shows diagrammatically an idealisation of a layer of ore sliding in slurry form down the untrimmed forward slope of partially saturated ore during the first few minutes of flooding of hold no. 1. Trimming of ore was not widely practised in 1980. Calculation assumptions are:

- initial forward acceleration at the top of the slope f = 2.5 m/s^2 taken from published data on storm induced pitch and surge motions
- this acceleration of about 0.25g is maintained by the ore slurry on the downslope which is taken as 33° and s ≅ 5 m for the slope length
- the initial velocity at the top of the slope is zero, the final impact velocity is v
- bulk ore mass density ρ = 5,100 kg/m^3 and eq(14) is assumed to apply.

From Newton v = $\sqrt{2fs}$ = 5 m/s, and from eq(14) assuming C_p = 3 the mean structural impact pressure is p_i = 191 kN/m^2 equivalent to a 19 m head of sea water acting over about 6m^2.

The two modes of static failure considered for plating were three-hinge plastic collapse (p_u) and edge shear yield (p_τ). Assuming $\tau_o = \sigma_o/\sqrt{3}$ these pressures for mild steel having σ_o = 235 N/mm^2 are respectively:

$$p_u = 4.5\sigma_o(t/b)^2 = 200 \text{ kN/m}^2$$
$$p_\tau = 2\tau_o(t/b) = 3730 \text{ kN/m}^2$$

whilst there is clearly no danger of shear yielding, the idealised local impact load could be on the verge of deforming the plate. No such deformation was seen, but in view of the uncertainties no conclusion can be drawn.

However, and potentially far more damaging, is the initial *gifle* pressure spike (see Fig. 14(b)). Even taking C_p = 15, this gives p_i = 956 kN/m^2 over about 1m^2 and this could be much higher. Such impacts would, almost certainly, induce a brittle fracture in A grade mild steel, particularly along a weld run. This is seen in the evidence and rapid flooding of the FO deep tank would ensue. This could also explain the plating "bow out" seen towards the top of bulkhead 339 (Fig 13).

In summary, whilst this hypothesis is uncertain it does have two circumstantial evidences plus analysis to support it. Also, no other plausible evidence has been advanced.

2.7.2. Bow Flooding During Sinking

Once no. 1 hold starts flooding and the fore deck becomes permanently immersed, then the rate of water ingress through all orifices becomes:

$$V_i = c_d \; A_o \sqrt{2ga} \tag{26}$$

where c_d may be taken as 0.6 on average and a is the time varying local head of water which naturally increases as the bow trim continuously increases. Beyond $a_o \cong$ 25 m implosion actions would probably have started in any bow space still at atmospheric pressure. As a first, probably conservative approximation, assume that (a) increases linearly with time (t) until a = a_o when the implosion depth is reached in time T_o. Then, a time integration of eq(26) leads to V_T the volume entered in any compartment over time T as:

$$V_T = (2/3)c_d \, A_o \sqrt{2ga_o / T_o} \; T^{3/2} \tag{27}$$

Or, in metric units, when a_o = 25 m (this estimate assumes no pressure build up in the tanks) the time taken to fill a compartment of volume V in seconds is:

$$T = ((V\sqrt{T_o})/8.86\,A_o)^{2/3} \qquad (28)$$

This then leads to Table 5 which shows the flooding time in minutes to completely fill the 2 fore peak spaces of Table 4 for a credible range of times T_o for the ship's deck to reach its first bow implosion depth of about 25 m.

Table 5: Filling Times (T minutes) for the fore peak spaces for a range of times to reach implosion depth

T_o(mins)	4	6	8	10
Ballast Tank	13.4	15.4	16.9	18.2
Bosun's Stores Flat	2.2	2.5	2.7	2.9
Ballast Tank*	2.8	9.3	10.3	11.1

*after 12 hours flooding

Tables 4 and 5 use updated orifice sizes since the paper was first published (see Footnote to Appendix 2). Table 5 shows the stores flat spaces would be in no danger of imploding during sinking because T > T_o, that is, the filling times are all appreciably greater than the credible implosion times (by factors between 1.8 to 3.4).

The fore peak ballast tank on the other hand is clearly in danger of imploding within the assumptions made and assuming there had been no penetration (splits, etc.) of the tank's boundary prior to final plunging of the ship. However, since no significant implosions occurred in the bow spaces (confirmed by Phases 1 and 2 surveys) this can only be explained by one of, or a combination of, two factors:

(a) a significant penetration of the tank boundaries occurred prior to, or in the early stages of sinking

(b) a build up of internal air pressure in the ballast tank takes place as it fills through the three broken air pipes without any significant air escape

In an attempt to quantify scenario (b) the filling theory was refined to approximately allow for both adiabatic and isothermal compression. This alone was not sufficient to explain the absence of implosion. It follows that scenario (a), probably in combination with (b), is likely to provide the answer. Very probably flooding occurred during sinking through the long split in bulkhead 33, as suggested in the previous section. This would also explain why there was no significant implosion-explosion of the fuel tank.

The Assessors' suggestion that the manhole covers to the ballast and fuel tanks had been removed prior to arrival in port is quite unacceptable and really is clutching at straws. No experienced mariner would contemplate such folly. There is absolutely no good reason for doing so, and opening the top of the fuel tank would present a fire and explosion risk.

2.7.3. Ship Bending During Plunging

The recent forensic investigations into the loss of the *TITANIC* (Garzke et al, 1996; Hacket and Bedford, 1996) have aroused great interest. The finding that the *TITANIC* started to break her back when the stern was out of the water and completed the process during sinking was approximately examined for the *DERBYSHIRE* during the final survey. The most severe bending moment would appear to be more than twice that to cause deck yield (about 15 GNm) probably toward the end of no. 6 hold. This has not been confirmed, but it does suggest that excessive yielding and crack extensions in the upper deck and side structure could well have initiated. But, the sequence of sinking at that stage would have been very quick and final separation of this (and other) sections is then much more likely to have been caused by the numerous implosion-explosion actions around the cross-section.

It is interesting here to reflect on Fig. 15 which shows the remains of the inboard section of the starboard WBT which runs through holds 8 and 9. It is substantially intact but folded and twisted at its centre. Because of the absence of any significant implosion-explosion actions (see next section), this assessor first wondered if the fore end fracture (at about frame 124 fore end of no. 8 hold) was initiated by an overload tearing of the deck as the stern lifts further out of the water. As the vessel then plunged the water would enter the WBT through these splits, thus reducing any implosion effects. This must remain as a speculative possibility. The UK/EC Assessors' report examines this target C230 in detail and decides (from video stills 265-267) that the fracture at about frame 124 is "exploded and ragged".

2.7.4. Implosion-Explosion Actions

This phenomenon has recently been discussed for the *TITANIC*, *LUSITANIA* (Garzke et al, 1996) and other ships, but is not widely expected and therefore understood. The mechanics have been explained (Faulkner, 1997c - now in Williams and Torchio, 1998a) and so only the essentials are summarised.

Description

Like all double skin OBO ships *DERBYSHIRE* had many empty void spaces (see Fig. 16). Her hatch covers would burst at the very early stage of sinking and the void spaces would be squeezed until at pressure (p_u) their weakest surface would collapse inward compressing the air to some higher pressure (p_e) like a spring. This internal air then explodes outward causing the devastation seen in the wreckage. This outward *shock wave* type pressure pulse has a steep rise like the *gifle* phase of water impact in Fig. 14(b). This explains why many of the fractures are brittle because grade A mild steel quickly loses what little notch toughness it has in the presence of such dynamic loads.

The second *bubble migration* phase immediately follows in which the expanding air escapes as a bubble or bubbles which in bursting out will oscillate in volume and then continue the damaging process. Any structure in its way, or to which it is attracted, can receive successive expanding bubble pressure "thumps" which have been known to permanently deform the shell plating of submarines, for example. This phase therefore continues to tear open the structure in a more ductile manner.

Mechanics

Void spaces are complex but, as a first approximation, the compression actions of the weakest boundary is treated as a constant load (p_u) piston compressing air in a cylinder till it reaches a highest maximum pressure (p_e) allowing for spring overshoot.

By making various assumptions it is shown that the maximum potential energy (PE) that could be released is:

$$PE \cong k\,V_o\,(p_u + p_o) \qquad (29)$$

where p_o is the atmospheric pressure, V_o is the initial void volume and k < 1 is a function of γ in pV^γ = constant which controls the implosion characteristics.

Potential Energy

Table 1 in Faulkner (1997c) shows the details for all cargo space holds from which the total available potential energy from all of these cargo space voids assuming a value of k = 0.9 is:

$$PE = 47.9 \text{ GJ} \equiv 16.0 \text{ tons TNT}$$

This assumes 1 giga joule is equivalent to 340 kg of TNT, or about 1000 sticks of dynamite. Not all of this potential was released as the wreckage shows that most transverse hold bulkheads and double bottoms are mainly un-imploded.
This is thought to be due to their high implosion pressures approaching 100 m head and to their unit type construction. For example, the PE from the connected WBTs, sides and hopper tanks alone are 49% of the total, and their implosion pressures average around 50 m head. Such energy would break the connecting fillet welds of the double skin transverse bulkheads exposing their open ends to an in-rush of lowish pressure water broken up by their internal egg-box construction. The air would therefore mostly escape before their side skins could implode at the greater depth.

However, on top of the PE calculated for the initiating implosion-explosion would have to be added the energies from the follow on bubble phase. A calculation of this for one WBT alone shows that the first bubble maximum radius would be about 6.4 m pulsating at a 0.6s period (Kendrick) and releasing in total an energy = 640 MJ ≡ 218 kg TNT. It is now obvious that even allowing for partial implosions the total process energy release is substantial.

Kendrick's assessment of the initial blast energy and first bubble radius and periods for the hold void spaces is summarised in a table at page 1:197 of Appendix 9 of Williams and Torchio (1998a). However, it is pointed out that Kendrick's energy equivalence is 1 GJ = 238 kg TNT, 30% lower than that above. Moreover, his table omits to include p_o = 10 m which is required using his equation. When these two adjustments are made, Kendrick's energy results agree within about 10% with those obtained from eq(29).

Prediction of Collapse Pressures (p_u)

Because of the extensive use of strong deep frames the implosion pressures were largely determined by three-hinge collapse of the rolled or fabricated continuous longitudinals given by:

$$p_u = \frac{k16\sigma_0 A_S(z_S + t/2)}{b(1-1/2\alpha)L^2} \qquad (30)$$

where $A_s < b_e t$ and k is an arbitrary factor set at 1.2 to allow for some measure of large deflection membrane actions. For heavy stiffeners where $A_s > b_e t$ then the plastic moment is approximately $\sigma_o Z$. The bracket term in the denominator allows for load shedding to the transverse boundaries. Shear strengths at grillage boundaries were examined, but were never the weak link.

Plating seldom fails before stiffeners because its continuous nature allows excessive membrane actions to develop at increasing pressures. However, the following failure criteria was developed based on the long-plate plastic membrane approach and a limiting central deformation w = b/8 which approximately corresponds to shear yield at the boundaries:

$$p_u = k_1 k_2 8\sigma_0 (t/b) \qquad (31)$$

where k_1 is a plate slenderness parameter to bridge the stocky 3-hinge collapse criterion to the slender membrane yield at about β = 2.5 , k_2 is to allow for the substantial membrane "shape hardening" effects for plate aspect ratios $\alpha < 3.0$ say (Faulkner, 1997c)

Finally, the lateral plate implosion-explosion pressure to cause weld pull-out by shear yield in the fillet welds of leg length ℓ is $p_e = 2\,\tau_o\,(\ell/b)$. But this was reduced by 0.5 for low penetration welds. Taking $\ell = 0.6\,t_w$ and $\tau_o = \sigma_o/\sqrt{3}$ then gives:

$$p_\tau = 0.35\,\sigma_o(t_w/b) \qquad (32)$$

These varied between about 2 to 4 times the predicted collapse pressure p_u and a lot of weld pull-outs were seen. This is not to be taken as evidence of bad workmanship as such connections are not designed to withstand implosion-explosion actions.

3. PHASE 1 SURVEY

The final survey was split into two; phase 1 in July 1996 and phase 2 in March and April 1997. Phase 1 was a limited budget reconnaissance "survey of opportunity" undertaken by Oceaneering Technologies Inc (OTECH) of Maryland operating out of Okinawa. The firm had undertaken the 1994 ITF survey and were very keen to please.

3.1 Aims of Survey

In order of priority the objectives were to:

- find and identify Target 9 (supposed stern)
- if this is not the stern, extend the sonar survey until it is located
- visually check the status of the stern spaces, propeller, rudder, the frame 65 region
- re-confirm that Target 63 was the bow and check its status, deck fittings, etc.
- time permitting, investigate other major targets
- determine the water clarity.

3.2 Equipment and Conduct

OTECH's survey vessel the *PERFORMER* was 5,575 ton displacement 10 knot, dynamically positioned, DSV. It was equipped with LBL acoustic transponders for the seabed, side scan sonar and the MAGELLAN 725 ROV using differential GPS for accurate positioning and Mesotech forward looking scanning sonar for navigating. Camera equipment was:

- a wide angle SIT zoom video of range about 30 m
- high resolution CCD camera with about 10m range
- 35 mm still colour camera with 750 frame capacity and 300 watt dual head strobes.

Lighting was by a 400 watt HMI gas arc system, and all images, except the 35 mm stills, were relayed in real time to the control van and to the three Assessors in the conference room (2 UK and 1 EC Assessor).

With an intensifying tropical storm HERB approaching, the underwater survey was limited to about 10 hours. After some 1-1/2 hours the ROV found the bow, but it took much longer to find Target 9, which was the stern. The remaining 1-1/2 hours was spent slowly surveying it before hastily retrieving the equipment and heading back to Okinawa.

3.3 Main Findings

3.3.1. The Stern

- is about 600 m from the bow at a bearing of 310° and lies at perhaps 60°-70° to starboard
- considerable implosion-explosion damage
- very little of the superstructure remains in way of the bridge and accommodation
- bulkhead 65 is missing
- rudder is in place and secured to the palm plates
- engine room is lying open with little signs of equipment, fire or explosion

- a suggestion that the propeller is in place was the "scrolling" of the seabed around the stern frame
- transom deck has extensive damage, including to some ventilators.

3.3.2. The Bow

- has few signs of implosion-explosion and lies at 20°-30° to port
- the deck is fractured over the whole width just aft of the collision bulkhead 339; it is mainly ductile but with some signs of straight line brittle fracture
- the supposed excessive corrosion (C6) is not confirmed
- the starboard windlass is missing and other equipment is damaged.

3.3.3 More Generally

- only incomplete views of two hatch covers were seen, one broken in two; no ID markings or numbers were seen
- widespread devastation of the wreckage, with evidence of fillet welds "unplugged"
- iron ore appears to be widely distributed
- seabed penetration is light
- the seabed slopes down about 16° to N x NE and is of average depth 4250 m
- water clarity is excellent
- video quality is generally very good, but more lighting is needed for still photographs.

3.4 Loss Scenario Deductions

C1 *Deck cracking Frame 65:* positional evidence goes strongly against this scenario, as do some of the fracture lines; P_i reduced from 3 to 1
C2 *Deck cracking elsewhere:* no evidence, no change
C3 *Torsional weakness:* no evidence, no change
C4 *Hatch cover collapse:* no evidence (but revised casualty and survivability analyses suggests S_c should increase from 4 to 5)
C5 *Hatch attachments:* no conclusions can be drawn
C6 *Fore deck corrosion*: nil, so C6 ruled out
C7 *Fore peak flooding*: no implosion might suggest bow was flooded before sinking; inconclusive; perhaps increase P_i because of deck damage
C8 *Cargo shift/liquifaction*: no evidence, no change
C9 *Propulsion loss:* propeller very probably in place; reduce P_i from 2 to 1
C10 *Rudder loss/steering failure:* rudder is in place, reduce P_i from 2 to 1
C11 *Explosion/fire in ER:* no sign of charring, but evidence is inconclusive so no change
C12 *Pooping - from forward waves:* evidence is inconclusive so no change
C13 *Pooping - running with the sea:* inconclusive, no change.

3.5 A Posteriori Updating

From above, each of the loss scenarios C1, C9 and C10 have been reduced to 1 and C6 is reduced to zero. For C4 it is suggested S_c increases to 5, for C7 P_i might perhaps be increased. There changes are shown as dotted lines in Fig. 2 (Faulkner and Williams, 1997).

In spite of the typhoon, the survey was regarded as being successful, and well worth the outlay.

4. PHASE 2 SURVEY

4.1 Overview Summary

This overview summarises the choice of contractor, the survey objectives, statistical information and the scope of sections 4 and 5 of this paper.

4.1.1. Contractor

The two UK Assessors advised the DoT that the Deep Submergence Laboratory (DSL) of the Woods Hole Oceanographic Institution (WHOI), Cape Cod, be engaged for the task. The three main advantages over a commercial contractor were:

- quality of equipment, staff and archiving
- the scientific non-commercial approach
- experience with *TITANIC*, *BISMARK*, etc.

There were also technology transfer benefits in the final Memorandum of Agreement between the UK DoT and the US NSF.

4.1.2. Objectives

The stated single objective was "to investigate the 13 loss scenarios identified in Lord Donaldson's Assessment" - with a view to determining the cause or the most probable cause of the loss of the m.v. *DERBYSHIRE* insofar as this was possible. If this was not possible then it is important to avoid yet more speculation by demonstrating that there is nothing more which could reasonably be done to establish the cause. A secondary unstated objective was to demonstrate that the technology now exists to successfully undertake a mission of this complexity for future important losses.

4.1.3. Statistics

These are drawn from the main report:

- 43 days were spent on site, 6 days mainly evading super typhoon ISA and 6 days in transit from Guam to wreck site to Yokohama; some days were lost replacing from WHOI the P-code navigation system which failed
- over 137,000 Electronic Still Camera images were captured digitally on tapes and disks
- from these 119 major contacts were mosaiced
- over 2500 contacts were classified by *DERBYSHIRE* hull location in a data base
- over 1800 hours of video recordings were made.
-

4.1.4. Scope

It is not intended here to dwell in detail on the objectives, planning, equipment, conduct of the survey or its many findings of fact. They are covered in great detail elsewhere (Williams and Torchio, 1998a) and many of the findings have more to do with the imploded-exploded wreckage than with the loss. Section 5 will mention the more important findings which are possibly related to the 13 loss scenarios when deductions are drawn for each of them.

4.2 Equipment, Team and Organisation

4.2.1. Equipment

The R/V *THOMAS G. THOMSON* (AGOR-23) was available for the survey. She is 83.5 m long survey vessel, displaces over 3000 tonnes, has a transit speed of about 15 knots, has high accuracy GPS (P-code) navigation and excellent station keeping with Z-drive propulsion and a bow thruster. The underwater vehicles deployed were:

- DSL-120 kHz split-beam high resolution SWATH bathymetric towed sonar
- ARGO II towed platform with heading control propulsors
- JASON and MEDEA self propelled ROV platform system with a 5 dof manipulator

and each has a 6000 m depth capability. ARGO II had an array of advanced imaging sensors configured specifically for photo-mosaicing of the wreck field in parallel 5 m to 7 m track intervals to give 30-50% overlap. MEDEA serves as a transition point from the main tow cable via a neutral 30 m umbilical to the self propelled ROV JASON. It provides an "eye in the sky" with its own lighting and SIT video camera to help guide JASON to targets of interest. JASON is specifically designed to support a wide variety of science operations with a variety of cameras and sensors. With its 7 thrusters it has fine positioning control with 3 dimensional speed capabilities of about 1 knot. A hydraulic drive rotary metal sample cutter and a coring tool were deployed.

A dazzling array of high resolution and high definition video and still cameras (including stereo for target depth definition) were deployed on ARGO II and JASON. Some were forward looking, some downward looking, and some with zoom and 50 magnification macro capability. A bank of powerful HMI, incandescent and strobe lights, both forward and down looking, ensured that excellent photo images were obtained, including high quality mosaics of important wreckage features.

4.2.2. The Team and Organisation

Andy Bowen, Senior Engineer of the DSL, was the NSF/WHOI Expedition Leader. 11 other WHOI staff included pilots, navigators and engineers for sonar, imaging, instruments and data handling. Robin Williams, UK Assessor, was nominated by the DoT as Chief Scientist to "decide any questions related to the survey plan and specifications in consultation with the NSF/WHOI Expedition Leader".

The UK/EC team of fifteen on board were grouped:

- 3 Assessors: Williams and Faulkner (UK) and Torchio (EC)
- 1 medical doctor from the MOD
- 4 Interpretation Group of 1 Master, 1 Chief Engineer and 1 Second Engineer who had served on sister ships and a Master Mariner from MOD Salvage who co-ordinated the group
- 4 Oceanographic/Survey experts, 2 from SOC Southampton and 2 from IFREMER France (the Institut Francais de Recherche pour l'Exploitation de la Mer).
- 3 PhD students, naval architect, sonar imaging, marine biology, to assist with data processing

The Assessors were to direct the investigative aspects of the survey. The Chief Scientist was empowered to "decide any questions related to the survey plan and specifications in consultation with the NSF/WHOI Expedition Leader". He also reported progress and discussed matters on a regular basis with the DoT.

All of the team, except the doctor and the EC Assessor, undertook watchkeeping groups in the control van on the transom deck on a daily 2 x 4 hours basis. The team also put in substantial additional hours each day on data processing, instrumentation, reviewing and interpreting data. Faulkner undertook the necessary analytical work in relation to the loss scenarios.

4.2.3. Main Technical Activities

The five main activities of Phase 2 (Williams and Torchio, 1998a; Faulkner, 1998a) were to establish:

- via a high resolution sonar survey of the site a "road map" of the area for later imaging
- a photo-mosaic survey of the whole wreck field, with later processing of key wreckage targets for photo-mosaic images
- close up pictures at several angles of key targets using colour cameras
- macro-photographs of key fracture edges at high resolution for failure to be defined
- cutting some metal samples to validate conclusions from the macro-photographs.

All but the last were undertaken, plus some seabed/iron ore coring. The navigation repeatability is said to be better than 5 m, but this was never tested.

The macro-photography was primarily to aid the investigation of the C1 scenario (which over influenced the survey) and was limited in time as the end of the survey was approached. No macro-photographs were taken of the fractures in the hatch covers. These were potentially more interesting because some, at least, may have occurred while the ship was fighting the storm. Metal cutting was programmed for the last day or two of the survey and was aborted because of technical difficulties.

5. EVIDENCE AND DEDUCTIONS

As stated earlier, in making deductions this vital section draws only from the important and relevant evidence from the findings of fact, together with other external evidence and analyses. This itself requires judgement, and so some guidelines are first offered.

5.1 What is Truth?

Scientific truth does not depend on human opinion. However, with marine casualties there is usually no absolute certainty and this applies to the *DERBYSHIRE*. This assessment relies on combined intelligence and wisdom to perceive the most probable truth beyond any reasonable doubt. This required an assessment of external information from various sources, as outlined earlier in Sections 1 and 2. In some cases this is augmented by further information not already given where this is judged to be possibly relevant.

5.1.1. Circumstantial Evidence

Much of the evidence from the survey findings of fact when used in arguing for or against establishing any particular possible cause or causes of the loss of the *DERBYSHIRE* is *circumstantial*. In law this means it does not bear directly on the fact in dispute, but on various attendant circumstances from which the judge or jury might infer the occurrence of a fact in dispute. The interpretation used here is similar.

The strength of such circumstantial evidence is its contributory potential. That is, whilst each piece of evidence is inconclusive by itself, collectively with other such facts or external data it may lead to a most probable result beyond any reasonable doubt. It is recognised that some circumstantial evidence may work for and some may work against any particular loss scenario.

One item of circumstantial evidence which has been widely used, and will be used here, is the absence of a distress message. However, the fact that none was received (by the Owners at least) is not a proof that none were sent. The FI outlined the radio transmission difficulties which can arise in such extreme weather conditions. The absence of any 3 hourly weather reports required by SOLAS is very apparent, and may indicate that *DERBYSHIRE* was experiencing such difficulties.

5.1.2. Basic Premises

Since implosion-explosion actions have affected the wreckage so much, it is important to state the three basic propositions which relate to structural failure:

- *Lemma 1:*
  Any compartment which has imploded must of necessity have been intact at the time of sinking
- *Lemma 2:*
  Conversely any compartment found fairly intact will have been completely, or nearly completely, flooded before sinking, or will have been flooded in the early stages of sinking before reaching its implosion depth.
  If an incomplete compartment has more or less kept its shape, then there must have been a rent or break in the structure which has permitted flooding at depths less than implosion depth
- *Lemma 3:*
  If a hull has separated into two parts before sinking, it is most unlikely that the two parts will then sink simultaneously. Therefore, it is nearly certain that the two parts will lie far apart on the seabed.
  There is then a reasonable expectation that the time needed for the sea to destroy watertight integrity will allow partial flooding above implosion depth, and consequently lead to less extensive implosion-explosion damage of the wreck.

It will be seen that these premises are important for loss scenarios C1, C2, C3 and C7. *Lemma 5* (in Section 5.5) is also important.

5.1.3. The Logic of Formal Safety Assessment

Section 1.5 outlined the FSA logic adopted in this investigation. A fourth basic premise is offered here because it is important and has been disregarded in the UK/EC Assessors' reports:

- *Lemma 4:*
  FSA logic requires that all possible scenarios be considered in the final assessment, unless there is direct evidence which proves that a particular scenario was the unique cause of the loss.

It therefore follows that for those scenarios which cannot be ruled out beyond reasonable doubt, the three considerations that need to be considered for each are relevant: Survey Evidence; Casualty and Service data; and, Theory and/or Test data.

The logical approach adopted to bring these final judgements together is the updated Risk Matrix. It is hoped that this will reduce further speculation to those "near miss" scenarios having risk numerals higher than 8 say, out of a possible 25. It is suggested that the recommendations should also be guided by this approach.

5.2 Main General Survey Evidence

Just a few of the more interesting general findings which have no bearing on the loss are mentioned here, with additional clarifying comments as necessary:

- correcting longitude, the wreck of the *DERBYSHIRE* is about 34 nautical miles from her last known position and at a bearing of about 24° from it (NE x N)
- all but one piece of the wreckage lies within a rectangle 1200 m x 833 m = 1km^2 oriented with its main axis SE to NW, as is the orientation of the bow to stern whose centres are 620 m apart
- about 70% of the wreckage lies NE of the bow to stern axis and 30% SW; which may be due to the influence of the local Kuro Siwo current
- the one piece outside this wreckage rectangle is 880 m SW x W from its centre, and is a double skin ship side unit; some of the other hydrodynamically slender foil type structures have also glided to the remoter parts of the wreck field
- other similar double skin units, such as transverse hold bulkheads, cofferdams and double bottom units, are reasonably intact with implosion-explosion induced separation at their boundaries with other structure; some are bent, probably by bubble forces
- most of the remaining structure is severely mangled; the superstructure from about the second level upwards, including the wheelhouse, top mast and funnel, is upside down and severely crushed
- some quite dense items of main and auxiliary machinery were more widely dispersed from their source than, e.g., were lighter wreckage items like hatch covers; this may be due to the implosion-explosion of the two large air reservoirs in the engine room which are estimated to generate at least 486 MJ of energy, equivalent to 165 kg of TNT
- the main engine itself was not seen but could be hidden in a hollow below the upturned aft end double bottom structure
- about 100% of the double bottom structures, and 80% to 85% of the ship deck and sides were identified
- there are no clues from the wreckage as to the specific time of the sinking; this will be discussed again in relation to loss scenario C13
- the findings from phase 1 are confirmed, except the location of the bow and hence the wreck field (about 500 m difference).

5.3 Main Relevant Evidence and Deductions

The important factual findings from both surveys are discussed in relation to the loss scenarios. Where scenarios are absolutely ruled out, the more important circumstantial evidence, external data and arguments which also support this deduction are nevertheless identified. References to the Assessors' report are to (Williams & Torchio, 1998a).

C1 Deck Cracking Frame 65

- Port and starboard slop tanks aft of bulkhead 65 imploded-exploded, as did the wing and hopper tanks froward of bkd 65 in no. 9 hold. By *Lemma 1*, the ship must have been intact in this region at the time of sinking. This scenario is therefore ruled out.
- *Other arguments:* complex deck fracture path which meanders across the ship and fore and aft of Fr.65; no casualty support; fracture mechanics considerations; wind and sea would have driven the wreck NW of the stern; *Lemma 3* applies.
- A further probability based argument which is also relevant to C2 and C3 is that none of the hatch covers are essentially intact and attached to their coamings, as they probably would be if the ship had broken in two at any cross-section.

-

C2 Deck Cracking Elsewhere

- This scenario is only fatal if it leads to extensive deck cracking and hull separation. Most of the structure of the hold compartments has been severely damaged, and no wing or hopper tanks remain intact. By *Lemma 1* this scenario is ruled out. *Lemma 3* also strongly supports this.
- *Other arguments*: no life saving equipment was launched (see later), no distress message was received, no hatches intact and very low incidence of such losses.

-

C3 Torsional Weakness

- This manifests itself as sprung hatch covers or fatigue cracks at hatch corners, which, if they extend beyond the coaming into the deck, can lead to very minor water ingress, or eventually to unstable crack extension across the deck and a C2 type scenario. All hatch covers were found close by;

there were fatigue cracks, but they did not extend into the deck. By *Lemmas 1* and *3* this scenario is ruled out.

- *Other arguments:* as for C2, but in addition double skin ships are torsionally very strong.
-

C4 Hatch Cover Collapse

Evidence with Comments:

- All 18 hatch covers were found in the wreck field, based on 13 complete covers and 10 part covers to make up the remaining 5 (Faulkner, 1997d); the Assessors' report uses 13 or 14 complete and 7 part covers (see sketch 26 page 2:66).
- Only for 3 of the complete covers can their location in the ship be established for certain (see 2.4 re identification clues); using the main report notation these are HS as no. 2 port cover, HI as stbd no. 2 and HE/AD as port no. 3; complete cover HO is either stbd 3 or stbd 8 and port covers HAE and HMG taken together would be about 80% of the companion cover to HO; the Assessors' report gives other possible allocations, including HR as starboard no. 1 cover (see below).
- All covers suffered external pressure as their initial or primary failure mode, but 3 for certain and 3 less certain had evidence of subsequently being blown outward; Figure 17 shows HD, half of a hatch cover which has split between centre longitudinals and shows it has been bent diagonally outward.
- Y type bending or tearing primary failures across the centres of the longitudinals were seen in 7 of the 13 complete covers, and X type bending or tearing occurs in the remaining 6 (see 2.4); in some cases there is a mix of both types of failure, again some being outward failures.
- A few of the Y bend failures are located about 0.35L to 0.4L from the fore end of the cover, rather than at the centre of the longitudinals; in two or three cases the bends are bulges rather than straight hinges, and some of the hinges are skewed across the longitudinals.
- About half of the covers were badly distorted, some with extensive tearing; most had heavily distorted and torn end plates.
- About half the covers were upside down; of those that could be seen, 14 had access/ventilator opening lids missing, and there is evidence of 5 covers with one or more of their three heavy duty securing catches being left in the open position (refer to Fig. 9).

Assessors' Deductions with Comments:

- The evidence of inward hinge lines and bulges being about 0.6L to 0.65L from the aft ends of covers led the Assessors to suggest this may be evidence of wave actions in the process of sinking by the bow. On this evidence they show a possible layout of the covers, referred to earlier, and a sequential description of hatch failure.
- For the static component of pressure, simple beam theory shows that for a plunging ship at inclination θ the linearly diminishing load leads to the maximum bending moment occurring at αL from the least loaded end where:
-

$$\alpha = \left(\sqrt{(t^2/3)+t+1}-1\right)t^{-1} \qquad (33)$$

where t = tan θ. For θ = 15°, 30° and 45° this defines the position of the plastic collapse hinge as α = 0.51, 0.52 and 0.53 respectively. This is so near the centre as to make no difference to the collapse position or pressure. The predicted "55-65% of the length" put forward by the Assessors to support their contention is pure guesswork (in 3.732 page 1:93). Nor would the taper of the beams help their assertion.

- There is the possible action of breaking waves as the ship sinks, but the probabilities of a hit at any particular position are out of anyone's control in the cauldron of typhoon ORCHID.
- A potentially more important deduction from the Assessors is their firm assertion that hatch cover HR is no. 1 starboard, and that "it was initially destroyed by the dynamic pressure loading of a plunging wave". They have attempted to seek support for this from two eminent metallurgists, even though there are no macro photographs of the fractured surfaces. This assessor would like to believe the assertion, but feels that there is no firm evidence to support such speculation.
- For example, it is noted that the Y mode of failure for cover HR at midlength is as expected from <u>uniform</u> pressure. Because these longitudinals are fillet welded to the central girder at that position some may have "pulled out" as bending approaches the collapse level. In so doing their release of energy would also be dynamic and could fracture the plating exactly as seen. Several of the hatch covers have similar fractures to that on cover HR. The other complication is the unknown effects on any cover of the subsequent implosion-explosion actions from within the hold.
- The Assessors' emphatic, but nevertheless specious statement (in 5.9 page 1:120) ruling out hatch cover weakness as the primary initiating cause of the loss (and indeed ruling out the six scenarios C8-C13 in the process) will be dealt with in C7.
- The Assessors in 3.732 page 1:93 refer to static and dynamic inelastic finite element (FE) calculations and they deduce:
  (a) The impact from a plunging wave could fracture a cover in half at the 3.5 mm fillet weld connections of the longitudinals to the centre transverse girder. The Assessors apply this only to cover HR (which they claim is no. 1 stbd) and conclude that it "was initially destroyed by the dynamic pressure of loading of a plunging wave" (4.133 page 1:114).
  (b) Under a static uniform pressure, plasticity starts at 3.8 m head and would collapse the cover at about 4.8 m head, thus confirming "that the design was in accordance with the requirements of the ICLL of 1966 which required the covers to withstand only 1.75 m".

Author's Deductions:

- Regarding the Assessors' deductions (a) and (b):
  (a) As mentioned above, several other covers fractured completely or partly along the centre transverse girder. If the Assessors' deduction is correct, then one must assume that these covers were also struck and breached by plunging waves, and this has been ignored. Two more serious criticisms arise directly from the evidence. Many of these longitudinals in other covers carried the full plastic moment at or near these 3.5 mm fillet welds well into the plastic stretching regime. This hardly seems consistent with the implied weak fillets. Secondly, the Assessors have also ignored the 6 covers which failed through X type bending <u>between</u> longitudinals. It was this difficulty to explain behaviour which gave rise to the suggestion for more rigorous FE calculations in the first place. So there are several reasons why the Assessors' deduction is incomplete and unconvincing.
  (b) Deduction (b) of the Assessors is demonstrably absurd. It implies that their interpretation of the 1966 ICLL is that covers are only required to withstand 1.75 m of sea water head. It will be seen from 2.4 Strength Assessments that in a well designed mild steel cover, stiffener yielding should start at about 4.3 m head, and plastic collapse at about 5.1 m – not 1.75 m. The Assessors have overlooked the safety factor! Incidentally, in the absence of stated assumptions regarding effective plate widths and boundary restraints, both of which are critical, the two pressure heads quoted by the Assessors in (b) are meaningless. The severe weld pull outs, tearing and distortion of the end plates suggests there was little restraint at the hatch cover boundaries. Loss of water-tightness would be well developed at a 4m head.

From these observations it would appear that the FE calculations and the Assessors' interpretations are open to serious questions and are unconvincing and inconclusive.

- The evidence of X type failures, local bending and some straight line fractures, does suggest that at least 6 of the 18 hatch covers failed due to dynamic wave actions as there really is no other explanation.
- It is ironic that no firm deductions can be made from the survey evidence for this most likely of all the loss scenarios, and indeed as the final event for loss scenarios C8 to C13 as will be seen.
- Recourse to other external arguments and data is therefore essential.

Other Arguments:

Most of the other arguments have already been made in section 2.4 but they are summarised here :

- Model tests at DMI measured pressure on hatch covers and coamings. Even for simulated steep elevated waves no more than 26 m high, these pressures correlated well with the theory advanced for predicting them in the Annex to Lord Donaldson's report. This theory ignores wave and ship dynamics and is expected to become more non-conservative with higher waves.
- In the hove-to position the *DERBYSHIRE* only requires one steep elevated wave of 23m height or more to collapse no.1 hatch cover, or one linear wave higher than about 26 m to do so. The notional probability of exceeding these values has been estimated for the sea conditions prevailing up to the early morning of the 9th September 1980, and are given in Table 3. They are high and Fig. 5 shows one such wave.
- Bow flooding reduces freeboard in way of no. 1 hold and inevitably increases the probability of hatch cover collapse. This was examined in 2.5 for realistic and unrealistic degrees of bow flooding and found to be negligible compared with the probability of collapse with no bow flooding.
- These probabilities are unacceptably high, and would have become higher during the afternoon and the night of 9th September and into the 10th when typhoon ORCHID executed three high-speed conditionally unstable cyclonic loops, with intensifying winds, as described in 2.1. The conditions would be ferocious.
- Casualty data (in 2.4) suggests that every third month a bulk carrier in dense ore is lost in rough weather and that every eighth month the loss is likely to have been due to breaching the forward hatch covers.
- Some Classification Societies have already implemented substantial increases in hatch cover strength. There can now be no doubt that the 1996 ICLL requirements are totally inadequate as regards hatch cover strength, especially so for heavily laden B-60 bulkers where buoyancy loss is greatest due to flooding.

Conclusions (C4):

- This scenario cannot be proved absolutely. But, on the collective basis of limited circumstantial evidence, experiments, theory and casualty data, it must be put at R_n = 25 in the extreme corner of the *"intolerable"* zone of the FSA risk matrix.
- It is also reiterated that it is not just no. 1 hatch cover which is vulnerable, they all are, and failure of covers anywhere along the ship's length is the likely end event for all of the other loss scenarios.

C5 Hatch Attachments

- The evidence shows that all 18 hatch covers are within a closely defined area of the wreckage field and were driven into the holds by sea actions. None were lost, so this scenario is ruled out.
- It is interesting to mote in passing that one bulker of the 108 lost in the last eight years (LR, 1998) *"sank after loss of hatch cover"*.

C6 Fore Deck Corrosion was previously ruled out.

C7 Fore Peak Flooding

Evidence at the Bow:

- The alignment from the bow to the stern is SE to NW and the bow is inclined to port by about 25°.
- The bow has suffered only minor implosion-explosion actions and appears to be attached to the remaining lower levels of no. 1 hold structure and below the mud line.
- Four broken ventilators are missing on the fore deck, as is the cover for the access hatch to the Bosun's Store (both assessed in 2.5).
- The aft coaming to the stores hatch is stove in with vertical splits along its edge which are also bent inward (video still 77) and the hinge pins are missing. In contrast, the side coamings are only slightly distorted at their aft corners.
- The starboard windlass, mast and other heavy fittings are missing and considerable bodily impact damage exists on the fore deck (Richardson, 1998)
- Collision bulkhead 339 has a major split across the ship at a weld line about 8 m above the hold floor. This was described in 2.7. Para 3.40, page 1:48 of the UK/EC Assessors' report suggests that the top and bottom edges are bent outward indicating "internal pressure". This is not agreed and conflicts with the very clear video stills 12 and 13 of the report, and with the Assessors' own statement in 4.74 page 1:109 *"The section of bulkhead 339 in way of the fuel tank was substantially bowed inward by external pressure on the hold side indicating that this tank was not completely filled with fluid during the initial stages of sinking"*.
- The port side shell has a crack and bulge below the ship's name running downward at about 45° from aft to forward; other lesser cracks are reported port and starboard.

Deductions From Evidence

Sections 2.5 and 2.7 address the more important possibilities quite fully and are summarised:

- From *Lemma 2* the absence of major implosion-explosion means the bow was mainly flooded before the external to internal pressure difference on any of its boundaries reached their implosion level (of about 45 m). The alternative explanation that the bow broke away from the vessel and was eventually breached by the sea and sank is untenable in the light of evidence just described.
- There can be little doubt that some level of green sea flooding would have occurred through damaged openings before sinking actions started. But, there is no evidence whatever of this or of the extent of the flooding.
- It certainly does not follow from lack of implosion-explosion damage that major flooding took place before the ship started to sink. As the conservative calculations in 2.7 and Table 5 show, after the filling of no. 1 hold, the time to flood ballast spaces and stores in the bow is a matter of minutes.
- Even then, irrespective of any doubt about the calculations, the logical arguments which follow these calculations show that there would really be no risk of damaging implosions anyway. This is because there would be insufficient difference between the water and air pressure inside ballast tanks and the sea outside, for the reasons given in 2.7.
- The explanation offered by the Assessors as to why the deep fuel oil tank in the centre of the bow is intact is that the manhole access covers at the top had been left open to ventilate the tank before reaching port. This is quite contrary to normal practice since the fuel tank has several permanent vents. If this fuel tank was fairly intact this would be explained by *Lemma 2* and by the arguments in 2.5 and 2.7 in Cargo Shift Actions in No. 1 Hold which <u>is</u> supported by evidence.
- The physical damage to the aft coaming of the Stores hatch has almost certainly been caused by the unseated windlass

or other heavy object. In doing so, this would certainly have distorted, and very probably sprung loose any cover from its butterfly clips and sheared the hinge pins (see Richardson, 1998). Down flooding to the Bosun's store would start and the reduction in freeboard when full is 21 cm at no. 1 hold.

- In 3.71 page 1:50 the two Assessors refer to the two toggles each side of the aft port corner of the coamings being tightly roped together around their threaded shanks and this prevented the proper use of the wing nuts in securing the hatch (video still 75). They then refer to this "unsecured fore deck hatch" in the second of their four conclusions. This implication of crew negligence has been challenged on the basis of the Summary report (1998b), notably by Grigson (1998) and by Richardson (1998). It is also understood that P&O have issued instructions for butterfly nuts to access hatches to be lashed together with cord for greater security when the hatch is closed.
- This assessor suggests that because the side coamings of the stores hatch are essentially straight and upright, with just local bending at the two corners, the hatch lid must have been in place and secure at the time it was unseated. The reason for this is that the gross inward bulge of the aft coaming with vertical splits along its top edge, indicates large horizontal membrane stretching actions and these could not develop unless such forces can be reacted. The two side coamings are not strong enough to do this and would bend inward if no lid was present to resist this. On the other hand, the lid and its own inside coaming would initially be strong enough to resist these membrane actions.
- This lends strong support to Captain Richardson's belief that the aft end of the hatch was probably struck by the freed windlass behind it, distorting the coaming and lid, shearing the hinge pins and springing the lid free from its butterflies.
- No significance is attached to the splits in the side shell of the bow. They are unlikely to have been caused by hitting a semi-submerged object, for the reasons given earlier and later in C14. They are more likely to have been caused by bottom impact, for which there is evidence. The 45° bulge crack port side is oriented as for shear from a forward impact.
- The bow to stern orientation aligns approximately with the likely orientation of a ship hove to at that time in typhoon ORCHID. This evidence is not conclusive, but it is backed up by evidence from other wrecks and suggests that the vessel at the time of the loss was more probably hove to than beam-on.
-

Other Arguments

- The "GLEN" and other ships have experienced bow flooding from broken ventilators, air pipes, etc. But, this appears not to have led to any serious consequences.
- In effect the Assessors argue that the collapse of no. 1 hatch cover would only occur if the bow spaces were flooded. This assessor regards the likely extent of bow flooding to be a quite secondary effect and is not essential to cause no. 1 hatch cover to be breached by the sea. Both issues are of course linked by seas over the bow actions.
- Sections 2.4 and 2.5 go into both topics in some detail, which is not repeated here. In essence, these argue from analyses and notional probabilities, that the ship would not survive long enough for the fore peak spaces to fill before no. 1 hatch cover, or some others, first failed from the dynamic actions of a single high wave. Using the same notional probability modelling for both events, the risk of hatch cover collapse is a higher order of magnitude, even if one were to allow for <u>unlikely</u> flooding of the fore peak spaces. The reduction in freeboard is trivial in the context of gross hatch cover overloads from just one wave of 26 m or a 23 m non-linear wave.
- It will no doubt be argued that for the flooding cases examined in 2.5 the 75:25 probability mix is arbitrary and biased toward slow fore peak flooding. This is agreed. But, as well as Ochi's reasoning, there is another physical factor to justify this judgement. Extremely ferocious, turbulent and highly elevated seas, which would prevail at the time, are less likely to fill openings than are more stable green seas. As an added comment, they are also much more damaging to structure as the Appendix shows.

Conclusions (C7):

- This assessor concludes unequivocally that the breaching of no. 1 hatch cover to flood the hold does <u>not</u> depend on the prior flooding of the fore peak spaces in the context of typhoon ORCHID (see 2.5).
- It follows that, whilst fore end flooding does occur and should be prevented, it is a secondary factor in the context of the loss of the m.v. *DERBYSHIRE*. It is not an essential initiating event.
- This conclusion applies to the grossly weak hatches designed to ICLL 1966, and would also apply to properly designed hatches 2.5 to 3 times stronger than this present requirement. Paradoxically, for intermediate strength hatches (say 50:50 chance of no. 1 hatch cover surviving typhoon ORCHID), the UK/EC Assessors' conclusion would become more valid; that is, extensive fore end flooding could then be the last straw.
- Because of the evidence, analyses, and other arguments made, the risk matrix notional probability of this initiating event occurring is increased from P_i = 2 to 4 (high probability). But because the trim consequences are less serious than first thought S_c is reduced from 4 to 2. Then R_n = 8 which is on the middle line of the ALARP zone, and suggests that safety related improvements should be made.

Other Loss Scenarios C8 to C13

Two general points are stressed to save repetition before considering this last group of six possible loss causes:

- Based on the supposed "slow filling of the bow prior to sinking" the two Assessors have ruled out all of these six other scenarios (but strangely, not any of the remaining scenarios). Under *Lemma 4* this would require the slow filling of the bow to be proven absolutely. Demonstrably this is not the case, nor is there any evidence for slow filling (over many hours). The circumstantial evidence of damaged ventilators and the missing stores hatch merely suggests that some unspecified water ingress is likely to have occurred. The Assessors dismissal of the remaining six scenarios is therefore quite invalid. Rather strangely, the Assessors seem to have had rapid capsize in mind for these other loss scenarios (see 4.162 page 1:117) rather than hatch cover damage.
- Section 1.5 pointed out that all of these six scenarios would result in the ship becoming beam-on to the weather, three of them with the ship stationary (C9, C10 and C11). Whilst it is conceivable that the final loss event for C8 could include capsize, because of *DERBYSHIRE*'s high stability it is considered that by far the most likely terminating event for all six scenarios is brisk rolling leading to collapse of any or several of the eighteen hatch covers. They occupy about 30% of the cargo deck area and are an order of magnitude weaker than the rest of the deck or the ship sides.

It follows from these points that all of these six scenarios will be considered here, and should any remain as a non temporary scenario their seriousness of consequence indices S_c must be high, and 5 is suggested if the ship is stationary.

It also follows that evidence of storm-induced hatch cover failures cannot help to distinguish which of these six other loss scenarios is most likely.

C8 Cargo Shift/Liquifaction

Evidence and Deductions:

- This scenario has two possible end events under beam seas: capsize or hatch cover failure. If the vessel had capsized all of the 14 hatch covers over the 7 laden holds (all except holds 2 and 6) would have been forced *outward* by the ore, and they would be easily recognised. Therefore, the positive evidence of inward initial failure of all hatch covers (para 5.8 page 1:120 of the main report) rules out capsize.
- No further deductions can be made from the survey evidence

Other Arguments:
- This scenario shows a very low incidence in casualty data over many years for bulkers over 20,000 DWT (see Table 1 in section 1.5). Of 4 that reported cargo shift, all sent distress messages, 3 were lost, 1 towed to harbour and all 4 crews were safely evacuated. This is mentioned in section 2.6 which reviewed the whole topic. The fact that no distress message was received or lifeboats launched is circumstantial evidence against this scenario.

Conclusion (C8):
- The scenario cannot be absolutely ruled out, so must remain at the lowest probability $P_i = 1$; because of the severity of typhoon ORCHID S_c is increased from 2 to 3.
-

C9 Propulsion Loss
- The engine and tail shaft could not be examined. No lifeboats were launched or distress message received, so with no new evidence Phase 1 conclusion remains at $P_i = 1$, $S_c = 5$.

C10 Rudder Loss/Steering Gear Failure
- The steering gear could not be seen but their high redundancy and reliability makes failure very unlikely. Also, no new evidence, no lifeboats launched or distress message received.
- Because the scenario cannot be absolutely ruled out Phase 1 conclusion remains at $P_i = 1$, $S_c = 5$.

C11 Explosion/Fire in Engine Room
- The machinery items mentioned in section 5.2 showed no signs of being damaged by explosion, fire or smoke. This is not conclusive, as the absence of charring after 17 years could be due to the actions of current, or even marine life.
- The possibility of nearly simultaneous explosions from hydrocarbon residues in the two slop tanks was ruled out on grounds of very low probability and no scorch or burn marks.
- No lifeboats launched or distress message received so P_i is reduced from 2 to 1, and S_c remains at 5.

C12 Pooping Actions From Forward Waves
- There is some evidence of pooping damage but not from forward waves. Retaining this always very improbable scenario is therefore unjustified and it is ruled out.

C13 Pooping Actions

As a preliminary comment, the UK/EC Assessors' report seems to have limited its definition of Pooping to damage to the Winnel ventilators allowing water to enter and contaminate the fuel tanks and causing engine stoppage (BRAER type). This paper considers all damage from being pooped (waves over the stern) so the title of C13 has been generalised. It is assumed that, in the prevailing very confused multi-directional sea conditions, being pooped is <u>not</u> limited to running with the sea or to the associated involuntary course changes.

Evidence with Comments:
- The main deck plating between frames 15 to 40 over the port fuel tank is severely collapsed downwards showing clear upstanding ridges over the underdeck longitudinals, which are also bent downwards.
- A long split exists in the nearby shear strake which is bent inward at 3 deck level; its straightish line suggests brittle fracture.
- The transom deck aft of frame 23 is severely collapsed downward at the centre with diagonal hinges leading to the transom corners; bollard tops are missing (probably imploded) and one circular manhole cover (or 500 mm MV?) is missing
- The port corner of the transom is severely damaged and the deck roller fairleader is bent inboard.
- The Winnel vents to the fuel tanks appear to be undamaged, at frames 17 and 26.
- Various ventilators on the aft deck quarters are missing their mushroom heads; one (at least) has a wad of material pushed inside it as if to prevent water ingress (video still 131).
- All guard rails at the stern are missing.

Deductions from Evidence:
- The damage to fittings on the transom and port side deck and the two depressions in the deck suggest damage either from pooping or from the early stages of implosion arrested from flooding elsewhere.
- The Assessors also suggested the deck depression on the transom may have been caused by inertia forces as the stern struck the bottom, but this is not agreed as the safety factor should cope with 3 or 4 g forces which are inconceivable in the likely bottom contact circumstances.
- The extensive split and inward depression of the port side shear strake appears more likely to be caused by pooping wave actions (see the Appendix) which could also account for the damage to the fairleader and structure at the aft port corner of the transom.
- Any ventilators stuffed with wadding suggest damage was from earlier pooping actions.
- The missing guard rails are likely to have been swept overboard at sea, or dislodged from the various implosion-explosion actions during sinking.

Other Arguments and Data:
- The probability that some of the loss of watertight integrity damage seen could be due to pooping would imply that water ingress occurred into the space below the transom.
- A lifeboat was sighted shortly after the loss. It is thought to have come from the ship's starboard side and could have been lost as the Master attempted to alter course to port, the obvious way. The predominant sea would then have been on the ship's starboard side. The starboard lifeboat, only one deck up, would then be very vulnerable to the mountainous seas and might have been torn from its davits. It is very unlikely that it was lost before the last message received from the ship because this would surely have been reported then. This is, of course, speculative and circumstantial.
- It would also be very difficult to maintain a hove-to heading in the conditions prevailing and the ship could have fallen off wind and ended up more or less beam-on (FI, 1989). She would then certainly have been in very serious difficulty, with a greater risk of being pooped.
- At the time of *DERBYSHIRE*'s last position report (0300Z/9/80) the Chief Officer of the m.v. *ALRAI* sent a message (referred to in 1.6) in which he felt *"*that it should not be ruled out that the *DERBYSHIRE* broke down and broached to*"*. The cause and need for this speculation is perhaps surprising and implies that the *DERBYSHIRE* might have been in following or quartering seas and in some difficulty. The FI appear to have ignored this strange message. Nor did they consider why the ship reduced speed on 8th September.
- The FI report (1989) points out that a later coded message (2000/9/80) was sent and received. The time was probably local time as the message refers to Tokyo, in which case this is 8 hours after *DERBYSHIRE*'s last position report. It would then just about coincide with the beginning of the first of the three conditionally unstable cyclonic loops (see 2.1 and Fig. 3). These intensifying conditions persisted over the next 24 hours as typhoon ORCHID recurved northwards.
- The wreck of the *DERBYSHIRE*, allowing for the earth's curvature, is estimated to be about 34 nautical miles (nm) NExE from her last known position, at 0300Z Assuming a

linear progression of the ship between these two positions, her tracks over 9 and 18 hour periods were plotted. These were then compared with a median plot of typhoon ORCHID's progress over the same 18 hour period, as reported from Guam, Tokyo and Hong Kong. Within an accuracy of $\pm$10 nm it was found that initially the *DERBYSHIRE* was about 100 nm from the typhoon's instantaneous position and after 18 hours about 135 nm from it.

- This assessor's approximate estimate of the radius of maximum rotating wind speed at that time was about 100 nm which agreed surprisingly well with an earlier DMI value of about 110 nm radius (Faulkner and Williams, 1996b). It therefore follows when these two sets of calculations are put together that the *DERBYSHIRE* during her last hours was very close to the most damaging radius of the dangerous semi-circle as it progressed along the track of the typhoon, as Fig. 3 demonstrates.
- It is noted that because of low freeboard the life saving equipment on the *DERBYSHIRE* was extremely vulnerable to boarding seas. In particular, Richardson (1998) suggests such seas would trip the hydraulic releases of the liferafts which would be washed overboard.
- Among her many problems *KOWLOON BRIDGE* suffered pooping damage.

Conclusions (C13):

- The evidence suggests the likelihood of pooping damage, but is inconclusive.
- However, taken together with the external factors just mentioned, and noting the potential forces involved from the Appendix 1, it would seem probable rather than possible that pooping occurred and caused at least some of the damage seen. What cannot be said is that this was a consequence of the Master attempting to run with the sea or to veer away from the typhoon track. These must remain only as possibilities.
- As a consequence of all these considerations, P_i might reasonably be increased, and an increase in S_c might be considered due to the possibility of water ingress into the steering flat. However, the evidence is not firm so no change is proposed.

C14 Hatch Coaming Collapse

C14? was retained as an "unforeseen" scenario following the Lord Donaldson work because the sea often springs surprises. Three were considered, but only one retained for serious consideration. The other two were:

- Striking a semi-submerged object like a container. This had previously been suggested (DoT, 1986) and was re-examined because of the splits found in the sides of the bow. However, these splits were not felt to be consistent with striking a container. Moreover, such a container would surely have been smashed by the turbulent waves in typhoon ORCHID.
- A huge wave or sequence of waves, sweeping away the accommodation and bridge super-structure. This was suggested by the DoT because after Phase 1 very little superstructure could be seen on the stern. However, there is no modern casualty data on such an event, and calculations showed that although the accommodation block walls might be badly deformed, there is massive shear strength in the internal transverse bulkheads and deep frames to resist this scenario.

However, hatch coaming collapse remains even though there is no direct evidence for it from the surveys. Specifically, no. 1 hatch coaming is regarded as the most vulnerable and the consequence is certainly serious and is not adequately considered in design.

Other considerations:

- The casualty data mentioned in 2.4 under Hatch Coamings does suggest that coaming damage does occur, and this would lead to water ingress into the hold.
- It has been suggested (Richardson, 1998) that the dislodged starboard windlass could have been swept aft at some stage before it left the ship, and hit the forward coaming of no. 1 hatch severely damaging it.
- A more likely source of extensive damage and substantial water ingress is from a spilling breaking wave, as described in Appendix 1. Calculations in 2.4 show that this coaming would be vulnerable even to high normal waves sweeping over the bow. But a spilling breaker, or a near-breaking steep elevated wave crest, has at least 4 times more damaging potential, as eq(vi) in Appendix 1 demonstrates.
- Using an exceedance probability reduction factor of 0.4 as suggested by recent data (Eilersen et al, 1989) and applying the equations of 2.2, it can be shown that over a 12 hour period of typhoon ORCHID the notional probability of a spilling breaker occurring for different wave heights is:

H	(m)	25	27.5	30
p	(%)	40	30	11

The crest tops of lower or higher waves would miss the coaming. Although the results are notional and untested, they do indicate significant possibilities of a breaking or near-breaking wave sweeping over the windlasses and on to no. 1 hatch coaming.

- The sequence of events would then be a steady and substantial increase in bow trim as no. 1 hold filled up, followed by the collapse of the hatch covers for holds 1 and 2 and plunging by the bow.
- It is a scenario that can arise from head or beam seas, and is potentially terminal because of flooding which could worsen subsequently if the damaged hatch cover is lifted by continuing sea actions.
- The 1966 ICLL makes no provision for hatch coaming strength, and the classification society rules are also quite inadequate.

Conclusion (C14):

- In the absence of corroborative data a cautious risk numeral 6 is suggested made up of P_i = 2 and S_c = 3. This new scenario is in the "ALARP" zone of the risk matrix and clearly needs to be examined further as R_n could increase (Appendix 1).

5.4 Updated Risk Matrix with Comments

Figure 18 shows the final risk matrix for the seven remaining loss scenarios C4, C7, C8, C9, C10, C11 and C13, the dotted lines showing their changes from the initial 1996 risk matrix. Also shown is the additional possible scenario C14 hatch coaming collapse.

The six scenarios which have been removed on the basis of evidence and/or other arguments and data are shown in the bottom left corner of their original position.. The cluster of C9, C10 and C11 in the right hand bottom corner very nearly also came into the ruled out category because they are extremely unlikely. But they are retained because of their maximum seriousness of consequence rating S_c = 5 as the ship would be stationary and very vulnerable if any of these events did occur.

This assessor has found this novel approach to evaluating risks to be very helpful for assessing and comparing the various loss possibilities. The numbers of course are notional, and other assessors will doubtless have different views. This does not really matter. What does matter is establishing the most probable cause for the loss (C4)and doing something about it and the "near miss" scenarios C7 and C13 which certainly require attention.

Although C1 has been ruled out, it initially had the second highest risk numeral. Improvements in the structural design of such connections should therefore be considered, as they also should be for hatch coamings (C14).

5.5 Initiating and Terminal Events

As the conclusions are approached it is appropriate to clarify a common confusion which often distracts attention from the true *cause* of ship losses, as it does in the UK/EC Assessors' report. The recent LR update (1998) has several examples.

- *Lemma 5* - The true cause of the loss of a ship is not necessarily or even usually the *initiating event* in a chain of events. The true cause may be a *serious defect* which the chain of events revealed. The initiating chain of circumstances exposed the defect to a dangerous test, but it is the defect which is the *cause* of the loss.

An example is a large B-60 OBO ship in dense ore lost in a severe storm. The initiating event might have been shearing of vents to the fuel day tank, leading to salt-water in the fuel, causing main engine stoppage, leading to the ship coming beam-on to abnormal waves, which caused weak hatch covers to collapse, which led to loss of buoyancy and foundering. The true cause of the loss is not loss of vents, nor loss of power, but the deficiency in the hatch cover strength.

Of the 14 loss scenarios considered here for the m.v. *DERBYSHIRE,* 9 are *initiating* events only, and the remaining 5 are *terminal* events because their initiating event is inevitably final. That is, there are no other necessary ship events in the chain before the ship is lost. These 5 are 2 primary structure scenarios C1 and C2, and the 3 hatch related ones C4, C5 and C14.

6. CONCLUSIONS

One can be certain beyond reasonable doubt that the m.v. *DERBYSHIRE* was finally overwhelmed by typhoon ORCHID during the night or early morning of 9th/10th September 1980. To determine what event, or combination of events which, beyond reasonable doubt, caused her to sink, we turn first to the survey evidence.

6.1 Deductions from the Underwater Survey

The survey eliminates some scenarios: the three *Primary Structure* ones C1, C2 and C3 and two of the four *Fore End Vulnerability* scenarios C5 and C6. Of the remaining *Other* scenarios C9, C10 and C11 have had their notional probabilities reduced to $P_i = 1$ and are discounted. So also is C8 ($P_i = 1$ throughout). The improbable scenario C12 is subsumed in C13. These are major achievements.

Does the survey evidence lead to changes in the risk numerals? The consequence seriousness index is not influenced by the evidence, so only possible revisions to P_i are considered:

C4 Hatch Cover Collapse:
The survey leads to no firm conclusion but a $P_i = 3$ is suggested (medium likelihood) because the mosaic images show different failure modes, some of which may be caused by wave actions.

C7 Fore Peak Flooding:
Video images show damage to vents and the stores hatch which would cause slow flooding. P_i is raised from 2 to 4, but certainly no more as there is no evidence for the extent of flooding.

C13 Pooping Actions:
Circumstantial evidence suggests the likelihood that some transom deck and side damage was caused by pooping actions, but the evidence is inconclusive so P_i remains at 3.

It follows that the underwater survey does not by itself reveal the sequence of key events in the loss and hence it does not explain the loss with a reasonable level of certainty. Nevertheless, the two Assessors attempted to do so, but their description of the series of events is unproven and speculative. Note that their UK/EC' report contains little numerical data, no relevant quantitative analysis, nor does it use FSA logic.

6.2 Deductions Based on Facts and Analyses

Since the seabed evidence is inconclusive, it is essential to consult other evidence and analyses. For this reason these independent factors govern this assessment. The final values of P_i and S_c are given below and shown in Fig. 18.

C4 Hatch Cover Collapse:
Analyses of wave heights during typhoon ORCHID show, beyond reasonable doubt, that waves able to collapse the forward covers pass over the bow section of the ship. This is shown without including the effects of downward pitching into the oncoming waves. Pressure measurements at the DMI also confirm that a single steep elevated wave of height 23 m would burst no. 1 hatch cover. Casualty data for laden bulkers supports this scenario. P_i therefore remains at its original 5 and S_c is set at 5, so $R_n = 25$.

C7 Fore Peak Flooding:
C7 is linked to C4 because the same waves do the damage to both. However, there is a fundamental difference which is ignored by the two Assessors. In C4 a single elevated wave above 23 m high is terminal; in C7 about 2,000 wave passages are required to fill the fore peak ballast tanks and stores. In fact, C4 and C7 are in effect mutually exclusive because analysis of typhoon ORCHID's waves shows that C4 will happen long before C7 has lead to significant flooding (see Table 6 in Appendix 2). The Assessors' suggestion of flooding into the forward fuel oil tanks is dismissed (Sections 2.7 and 5.3).
Hence, it is concluded that the breaching of no. 1 hatch cover(s) does not depend on the prior flooding of fore peak spaces. P_i remains at 4 from the survey evidence but S_c is reduced from 4 to 2 because of the limited flooding.

C13 Pooping Actions:
The very confused, steep elevated 3-dimensional waves of typhoon ORCHID might suggest that S_c be increased from 2 to 3 because of the possibility of significant water ingress. However, it is left at 2 with a question mark, mainly because it is an initiating event, not a terminal one.

C14 Hatch Coaming Collapse:
This cause of loss was introduced because of suspected weakness of the hatch coamings (section 2.4) and because the analysis in Appendix 1 now quantifies the large forces caused by breaking waves over the bow or from the beam. It is also potentially terminal due to substantial water ingress which could worsen if the damaged hatch cover was also lifted or detached by the continuing sea actions. A cautious $P_i = 2$ and $S_c = 3$ is judged.

6.3 The Cause of the Loss

- Beyond any reasonable doubt, the direct cause of the loss of the m.v. *DERBYSHIRE* was the quite inadequate strength of her cargo hatch covers to withstand the forces of typhoon ORCHID. This weakness to resist substantial water ingress is gross when compared with other major elements of the watertight boundaries of the ship's hull.
- These hatch covers did meet the acceptable stress criterion of the 1966 ICLL. It then follows that the fundamental fault and cause of this tragic loss lies fairly and squarely in the altogether inadequate value and inappropriate nature of the loading and safety factor implicit in these Rules.
- It is not possible to say which of the eighteen covers failed first, or from which direction the waves came; but evidence and other arguments suggest that the no. 1 hatch covers were probably the first to yield, probably from waves over the bow with the ship hove-to.

- The prime conclusion does not depend on the likely extent of flooding of the bow spaces through damaged openings or missing cover (Appendix 2).

6.4 Other Important Conclusions

- It will be apparent that this assessment differs in many details and in its prime conclusion from Williams and Torchio's assessment. Their most likely cause of the loss (in Chapter 6) is almost pure fiction in places, full of assertions which are seldom backed by evidence and never by appropriate analyses. Most assertions are non sequitors. This is the clue to the fundamental difference between the two assessments.
- Nevertheless, this assessor agrees totally with their most sensible paragraph in the whole report (8.69 page 1:142*): "Regardless of the actual initiating event, the DERBYSHIRE case illustrates quite clearly how the hatch covers are a front line of defence against water ingress. Their failure inevitably would lead to the loss of such vessels and must be treated in the same manner as the main fabric of the hull structure".*
- However, and with respect, it should be understood that the hatch cover survey evidence is inconclusive, with a medium rating. It is only the quality of the DMI test data of 1986 and the conservative theory advanced in 1995 for Lord Donaldson's Assessment which, when matched together (Faulkner, Corlett and Romeling, 1996), provide the real justification and confidence for such statements which were made very clearly at the time.
- This is stressed simply to emphasise that advanced analytical thinking is an essential prerequisite for complex endeavours of this nature if a beyond reasonable doubt conclusion is to be reached. The independence of the survey and its deductions from sponsor interests is also vital once the objectives have been set.
- The question has been asked in the Assessors' report: *"Why did the DERBYSHIRE find herself in the most dangerous sector of typhoon ORCHID?"* The last para. In 1.6 touched on a common theme among master mariners who generally have little confidence in the safety aspects of weather routing. It is also very clear from Appendix II of the FI report that Ocean Routes got it wrong as far as the plot of typhoon ORCHID was concerned. Had that been accurate Captain Underhill would have incurred little risk in attempting, as he did, to run ahead of the storm. But, had he been more influenced by the consistent median plot from Tokyo, Guam and Hong Kong, and allowed for the well known vagaries of typhoons and taken the approved avoidance action (The Mariners' Handbook, 1979), he would not have put his ship at such risk. But, he would also have been anxious to meet the Charter arrangements and would, no doubt, have confidence in the size and capability of his nearly new ship, especially before the more recent spate of bulk carrier losses were known.
- Section 2.3 describes TD simulations which suggest that high non-linear waves can give rise to wave induced bending moments which may be about 80% higher in sag than those given by the unified IACS standard (Nitta et al, 1992).
- The three remaining loss scenarios C7, C13 and C14 all have high enough risk numerals to suggest that they should be treated as "near misses", and methods to reduce these risks should be devised.
- Freak or abnormal waves do occur and have sunk many ships. They are not curious and unexplained quirks of nature. This assessment suggests that their occurrence can be predicted with sufficient accuracy for survival design as advocated recently by Faulkner and Buckley (1997) and others.
- The underwater technology now exists such that no ship need now remain unlocated or its loss not investigated if the will to look for it exists and the necessary resources are made available (Lang, 1998).

7. RECOMMENDATIONS

We should not only react to disasters, but design and operate to prevent them. The Assessors' recommendations, like their conclusions, cloud rather than clarify the main issues. Most have little if any link with the underwater survey evidence.

7.1 Prime Recommendations

- Revise substantially the 1966 Loadline Convention requirements as regards hatch cover strength for all covers. Detailed suggestions for this are given in the last two sections of 2.4 which also show the weight and cost penalties can be small. The analysis there also shows how important it is to abandon the present archaic allowable stress criterion based on ultimate stress. It should be replaced with a more logical and safer ultimate strength criterion.
- All Type B freeboard ships should have a raised forecastle head with high bulwarks and a substantial breakwater to protect the forward hatches and deck machinery and fittings.
- Consider an increase in freeboard and/or deck sheer forward. This is not essential if the first two recommendations are adopted.
- Existing ships should have their covers replaced now. This breaks with tradition but the situation in lost lives is far too serious to delay.
- Hatch coaming design loads are inadequate.
- Review the present status and effectiveness of the ship safety aspects of weather routing.

7.2 Other Recommendations

The following recommendations arise specifically from *DERBYSHIR*E related investigations, but are also thought to be important to consider for other ships.

- Designs for "near miss" scenarios C7, C13 and C14 should be improved No rules exist for coamings and their collapse (C14) can be terminal.
- The Frame 65 scenario (C1) initially had a high risk numeral. The design of such connections can and should be improved to eliminate cruciform "through the thickness" loading and alignment problems, and to reduce the direct and shear transfer loads.
- A *Survival Wave* approach to design (Faulkner and Buckley, 1997) should now be considered seriously as an addition to the normal design process. Section 2.2 introduces the topic and loss scenario C14 would be an excellent one for testing the method and to illustrate the first principles approach required. Also see Appendix.1.
- The suggestion that ship bending moments from abnormal waves may substantially exceed the present IACS unified standard should be examined.
- The inelastic FE calculations of partially loaded hatch cover responses to dynamic waves are felt to be well worth repeating to see if failure modes corresponding to those seen in the survey can be explained. This would need an interactive well specified and monitored contract.
- The use of grade A mild steel clearly can promote brittle type fractures in structure and fittings under dynamic wave actions. Previous proposals that its use be abandoned for all hull and weather deck structures and fittings (Jubb, 1995) are suported.
- Dynamic impact of side shell from the mobility of saturated ore cargoes in holds should be considered in design of single hull bulkers.

- Because it is now evident that even large ships can sink very rapidly, the wider use of ramp mounted gravity launched lifeboats should be considered. Life saving equipment should not be vulnerable to pooping wave actions.
- The cargo hold flooding dangers are notably higher for ships laden in dense ore. This suggests that Floodability requirements may need to be revised.
- Fore peak spaces should be capable of being pumped out with controls operated from the Engine Room or Pump Room.
- The design and protection of weather deck ventilators and access hatch covers must be improved.
- The FSA approach should be beneficial when considering design and operational improvements.
- Guiding principles and practices for forensic analyses of shipwrecks should now be established. This must include other evidence and analyses.

Several more detailed recommendations arising from the *DERBYSHIRE* work can be found in Faulkner and Williams (1996a and b), in Faulkner and Buckley (1997) and in Faulkner (1998). These deal with environmental and oceanographic needs, design, construction and operation, and feedback of service experience.

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APPENDIX 1

Breaking Wave Impact Forces in Wind Driven Seas

Adlard Coles (1991) describes the "supreme violence of breaking waves". These are not confined to shallow waters or shelving beaches. In open ocean extreme conditions, if the wind rapidly intensifies, younger steeper waves are generated (see Fig. 6 of Faulkner and Buckley, 1997). In the *overshoot* phase of wind wave growth some of these waves become oversteep (crest peak slopes m > 0.58) or unstable and their crest particle velocity (u_o) exceeds their celerity (c) and they dissipate their excess energy into breaking waves. There are two forms (Bacon, 1991):

- *Spilling Breakers* occur when the crest "topples" down the front face of the wave. It is assumed that the maximum impact velocities are at about $v = 2c = 2\lambda/T$ and that the maximum incident wave heights cannot exceed $H = 2.9\ H_s$ (Eilersen et al, 1989)
- *Plunging Breakers* are less common in open oceans, but they can occur when the wind wave growth has been so rapid that the overshoot energy is unusually high from ferocious winds, or, in multi-directional intensifying wind conditions which create 3-dimensional seas of *pyramidal* form whose crests can interact with each other creating what Adlard-Coles described as the *"*seething sea like a bubbling cauldron*"*.

The energy from spilling breaker wave fronts has destroyed sea walls and dislodged and moved breakwaters. Figure 5 shows an elevated wave crest recorded during hurricane CAMILLE. Such waves are clearly important when considering wave forces sweeping along the upper deck of ships, or, even impacting on the topsides of ships holds. Present advice for vertical hatch coamings (Faulkner, 1995b) of taking relative impact velocity v_i = (1.2c + ship speed v) when hove to, as in 2.4, should be reconsidered for a higher value for spilling breakers:

$$v_i = 2c + v \qquad \text{(i)}$$

used in conjunction with design pressures defined by eq(14) in section 2.4.

However, although rarer in deep water, the horizontal forces that can arise from the plunging breaker can be significantly higher. A *plunging jet* of water forms in front of the wave crest whose velocity can reach up to 3 or 4 times the wave speed (Bacon, 1991). This is thought to be due to the presence of air trapped under the curl of a plunging breaker (Bagnold, 1939). The initial relative horizontal water velocity would then be:

$$v_i = kc + v\cos\alpha \ , \ 2 \le k \le 4 \qquad \text{(ii)}$$

where v is ship speed and α is heading angle relative to the predominant waves. Assuming a gravitational fall of the initially horizontal jet, and that v_i remains unchanged during the second or so before impact, it follows that for an initial height (h) of the jet above the impacted structure:

$$u = \sqrt{2gh} \quad , \quad t = \sqrt{2h/g} \qquad \text{(iii)}$$

$$v_{i\theta} = \sqrt{v_i^2 + 2gh} \qquad \text{(iv)}$$

$$\theta = \tan^{-1}\left(\frac{u}{v_i}\right) = \tan^{-1}\left(\sqrt{\frac{2gh}{v_i}}\right) \qquad \text{(v)}$$

where u is the vertical component of $v_{i\theta}$ the impact velocity, whose direction is angle θ to the horizon, and t is the fall time. For example, with T_p = 13 s, λ = 260 m, c = 20 m/s to correspond with the H = 30 m wave shown for the *DERBYSHIRE* in Fig. 8, and assuming v = 0, equations (ii) to (v) with k = 4 and (14) with C_p = 1 have been evaluated for varying heights (h) of the plunging breaker above the deck to derive $p_{i\theta}$ the reflected wave impact pressure head of sea water acting on a flat surface normal to the jet (90° - θ to the deck):

h	(m)	0	5	10	15
u	(m/s)	0	9.90	14.01	17.15
$v_{i\theta}$	(m/s)	80	80.6	81.2	81.8
θ	(deg)	0	7.1	9.8	11.8
$p_{i\theta}$	(≡m)	326	331	336	341

The corresponding much lower vertical pressure component acting on a horizontal surface under these assumptions is C_p 0.5 ρu^2 = C_ph. However, in section 2.4 the C_p factor was ignored for green sea pressures on hatch covers.

These nearly horizontal *gifle* shock impact equivalent pressure heads may seem unbelievably high, but they are of the same order as those determined experimentally by Denny (1951):

$$p_m \equiv 28\,H \;,\; p_e \equiv 100\,H$$

where H is the incident wave height, p_m is the most frequently occurring instantaneous green sea impact pressure head and p_e is the maximum extreme pressure head. The duration of these gifle peaks was on the order of 0.01 seconds and these pressures are local – see Fig. 14(b). Taking H = 30 m as for the *DERBYSHIRE* calculations gives:

$$p_m \equiv 840\text{ m} \;,\; p_e \equiv 3000\text{ m}$$

Dividing by the 0.5 ρv^2 = 326 m for θ = 0° in the above calculations gives C_p values of <u>2.6</u> and <u>9.2</u> respectively. Although such comparisons can be fortuitous, it will be seen these values are very close to the <u>C_p = 3</u> and <u>9</u> derived for design from more recent data in section 2.4 (Faulkner and Buckley, 1997).

For interest, it can be shown that ignoring ship speed the ratio (R) of the square of the horizontal water speeds at the crests of breaking and near breaking waves having the same celerity c = λ/T is approximately:

$$R = \left[\frac{k}{1 + \pi H/\lambda}\right]^2, 2 \le k \le 4 \qquad \text{(vi)}$$

Taking values of k = 2, for spilling breakers and 3 and 4 for plunging breakers and a limiting steepness of H/λ = 0.14 from the *DERBYSHIRE* calculations leads respectively to R = 2.7, 6.1 and 10.9. This illustrates how much more damaging are the crest-induced forces from breaking waves than from linear waves. Ratios greater than 5 have been confirmed from water tank experiments on vertical piles (Kjeldsen et al, 1986).

In passing, it can be noted that whereas water particle motions execute *oscillatory* closed loops in linear waves, in higher order deep water waves they are *translatory* or *progressive* in nature and have higher forward particle velocities.

It is suggested that naval architects should design vertical surfaces and fittings to withstand breaking or near-breaking actions from *spilling* breakers, and leave the *plunging* breakers to coastal engineers. Load criteria should be derived using Buckley's First Principles Methodology (outlined in Buckley, 1997). Data from Eilersen et al (1989) suggests that the probability of encountering these breaking waves might be taken as 0.4 p_e(H) as derived, for example, in eq(9).

Further work is required. A good starting point for researchers is the following references: Longuet-Higgins (1974, 1982) and Dommermuth et al (1988) based on two excellent doctoral theses from MIT (Chan, 1985 and Rapp, 1986). For aerated seawater, density is less and the gifle decay is longer.

APPENDIX 2

During discussion of the similar 1998 SNAME paper a verbal wish was expressed to see a more direct comparison of the notional probabilities of fore peak flooding and collapse of no. 1 hatch cover from bow waves. For the same assumptions this would provide valid comparisons to illustrate the difference in emphasis between this paper and the UK/EC Assessors' report.

To this end Tables 3 and 4 in the main text help (pages 7 and 10), but Table 6 is now included in which the same notional density function is used for the two events. However, a 50:50 mix of linear and non linear waves is now assumed for comparison purposes. This change is because the original 25:75 mix was considered to be too large a bias toward the abnormal steep elevated waves. As before, the analysis ignores the downward pitching of the vessel into wave troughs, but the effect of flooding-induced static bow trim on hatch cover collapse is now explicitly included in the last line of Table 6.

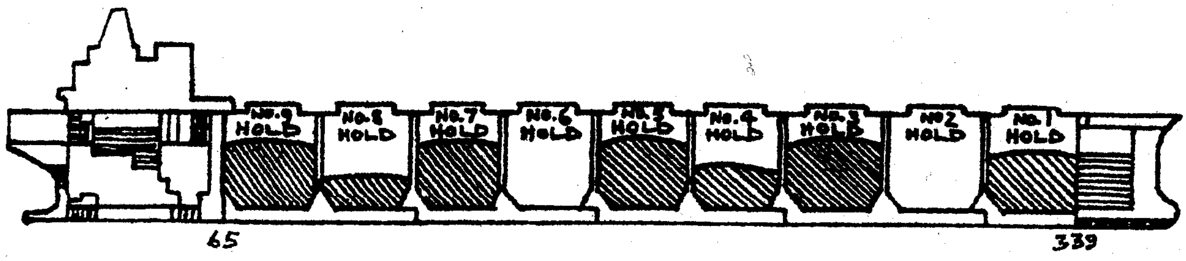
Table 6: Relative Notional Probabilities for Flooding of Fore Peak Spaces and Collapse of No. 1 Hatch Cover (p_f = p_e) for Various Storm Periods and H_s = 14 m

Scenario and Duration D	1Hr	3Hrs	6Hrs	12Hrs
Fullness of FP Spaces(%):				
- Ballast Tanks	4.4	13	26	43
- Bosun's Stores Flat	39	100	100	100
Total Freeboard Reduction at No. 1 Hatch ΔF(cm):	19	50	62	86
HC Collapse Probabilities (%):				
- Linear Waves and Trim	34	78	97	100
- Non-Linear Waves & Trim	82	100	100	100
- 50:50 Mix of Waves & Trim	59	89	98	100
- 50:50 Mix of Waves & No Trim	51	79	92	99

It follows from Table 6 that allowing for trim changes from fore end flooding only increases the notional p_f for no. 1 cover by a maximum of no more than 10% in three hours. One concludes that the probabilities of hatch cover collapse do not depend in any significant way on the likely extent of bow flooding. Furthermore, the flooding of the bow spaces is not a terminal event, whereas bursting one hatch cover is.

Flooding of the deep fuel oil tank is ruled out for the reasons given in section 2.7. Even if this were remotely possible it would take two days or more through the known orifices and the Captain would surely know something was amiss.

Footnote: *Since the original printing of the paper the opportunity has been taken to correct the previously assumed orifice sizes in Tables 4, 5 and 6. This now also excludes the Engineers Store as it is not in the fore peak. The author was prevented from checking such details earlier, but has subsequently been provided with full details.*



Note:- Cargo centroids assume level cargo within confines of hatchway opening with 33° angle of repose beyond hatch boundaries.

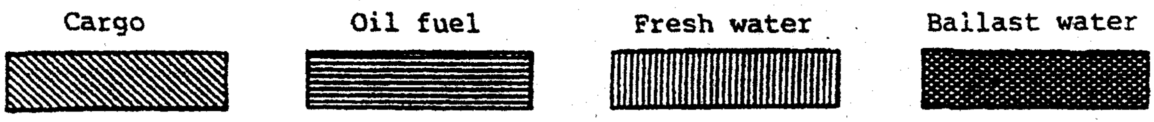


Fig. 1 : last known Loading Condition of m.v. DERBYSHIRE

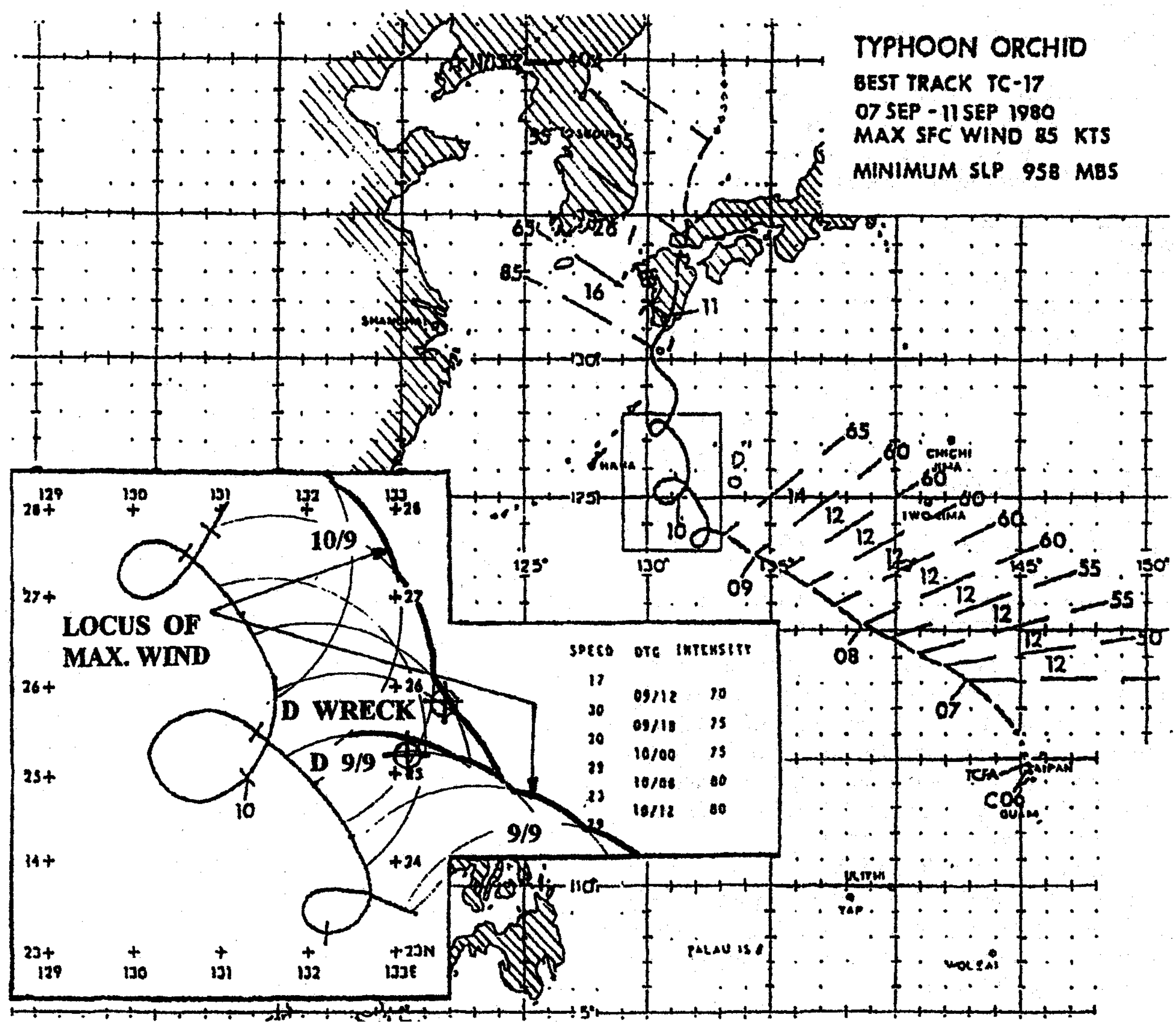
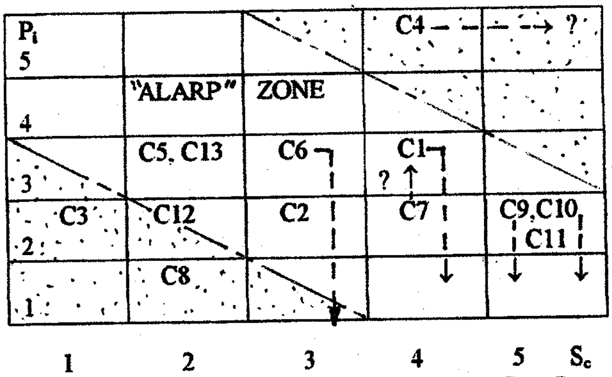


Fig. 3: Typhoon ORCHID Track and Last Known position of m.v. DERBYSHIRE



		$P_i x S_c = R_n$
C1	Deck Cracking Fr.65	3 x 4 = 12
C2	Deck Cracking Mid-Sections	2 x 3 = 6
C3	Torsional Weakness	2 x 1 = 2
C4	Hatch Cover Collapse	5 x 4 = 20
C5	Hatch Attachment Failures	3 x 2 = 6
C6	Fore Deck Corrosion Collapse	3 x 3 = 9
C7	Fore Peak Flooding	2 x 4 = 8
C8	Cargo Shift/Liquifaction	1 x 2 = 2
C9	Propulsion Loss	2 x 5 = 10
C10	Rudder Loss/Steering Failure	2 x 5 = 10
C11	Explosion/Fire in Engine Room	2 x 5 = 10
C12	Pooping – From Forward Waves	2 x 2 = 4
C13	Pooping – Running with the Sea	3 x 2 = 6

FIG. 2: Risk Matrix for Abnormal Waves (1996)

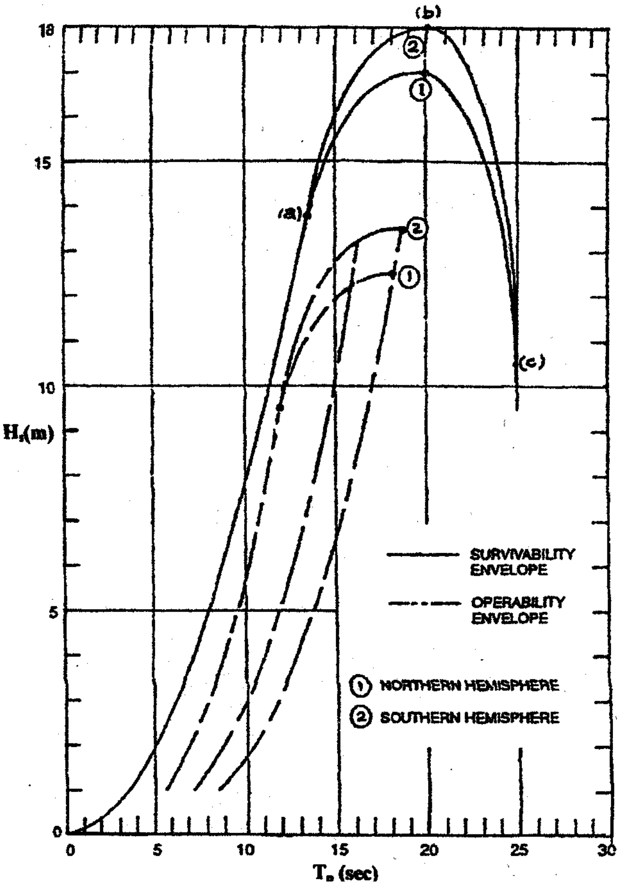


FIG. 4: Global Survivability and Operability Envelopes

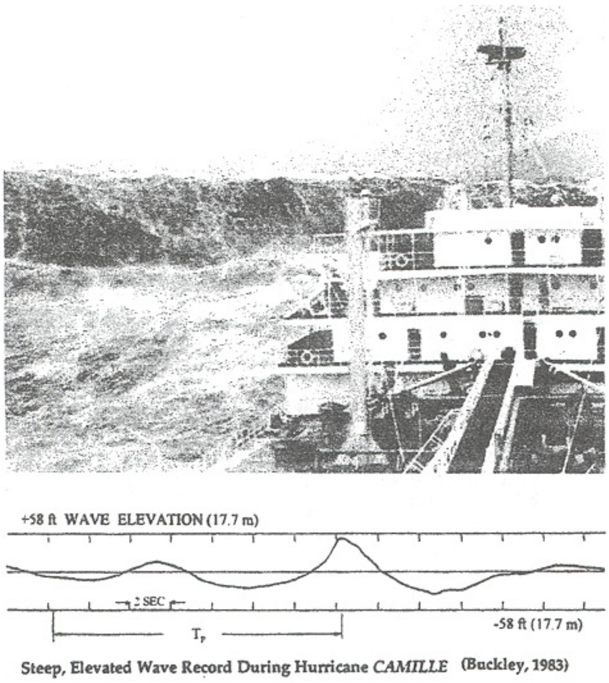


FIG. 5: Steep Elevated Wave Record and Ship Encountering One

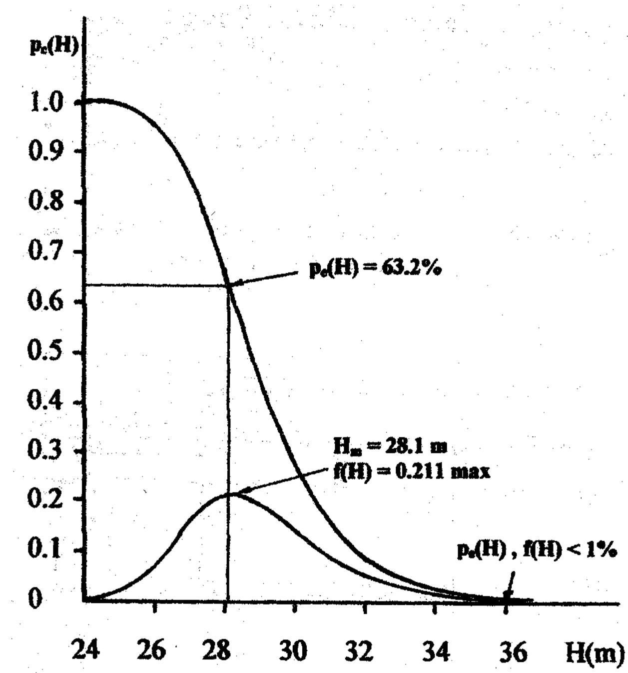


FIG. 6: Probability Density and Exceedence Plots

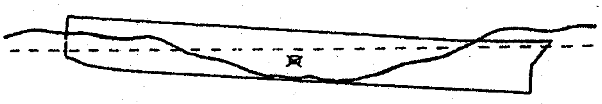


FIG. 7: FPSO Maximum Sagging in Non-Linear Waves

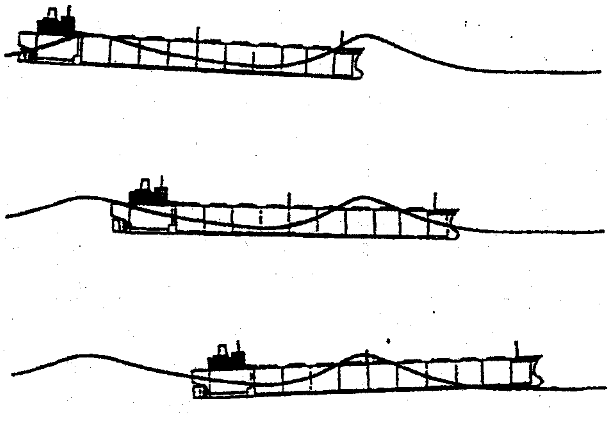
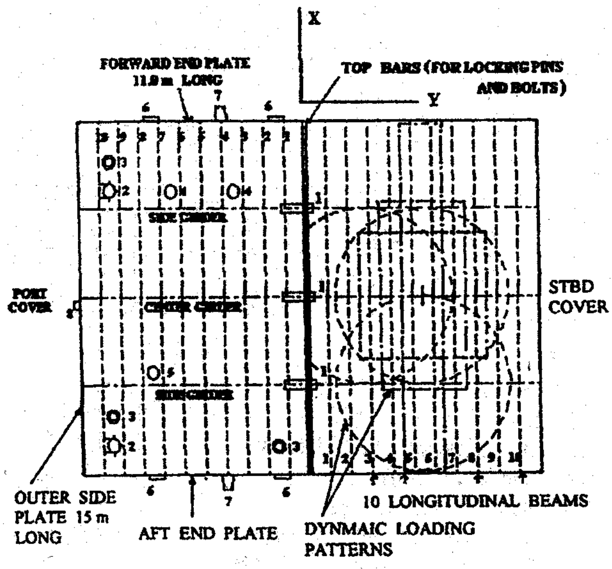


FIG. 8: m.v. *DERBYSHIRE* Encountering a 30m x 260 m Steep Elevated Wave



1. Heavy duty securing catches (3)
2. Access/Ventilator openings (2)
3. Butterworth openings (3)
4. Openings for heating coils (2)
5. Hatch clamp slot (1)
6. Wheels (4)
7. Emergency towing brackets (2)

FIG. 9: Hatch Cover Details and Loading Patterns

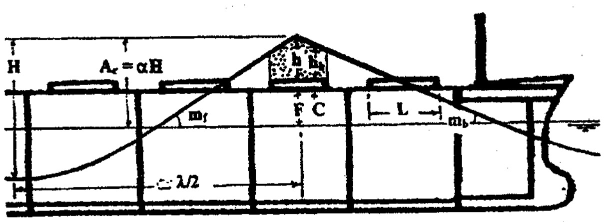


FIG. 10: Hatch Cover Design Head for Extreme Steep Elevated Waves

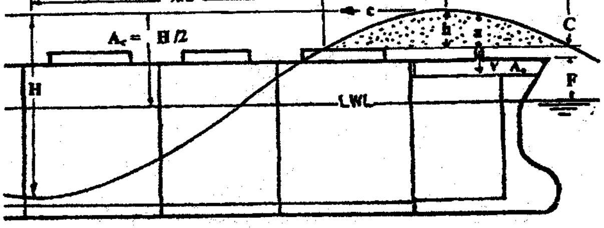


FIG. 11: Linear Wave Crest Passing Over An Orifice

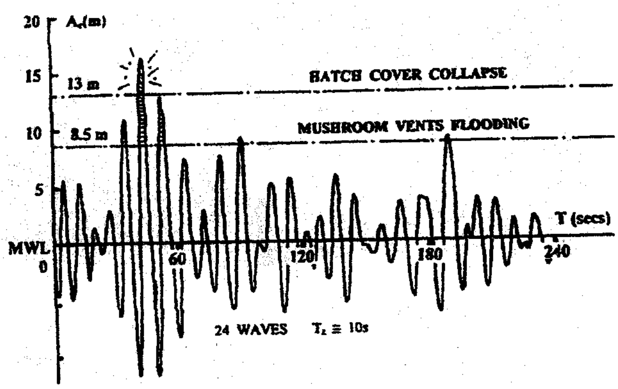


FIG. 12: Truncation of Waves for First Passage Events and Up-Coming Threshold Events

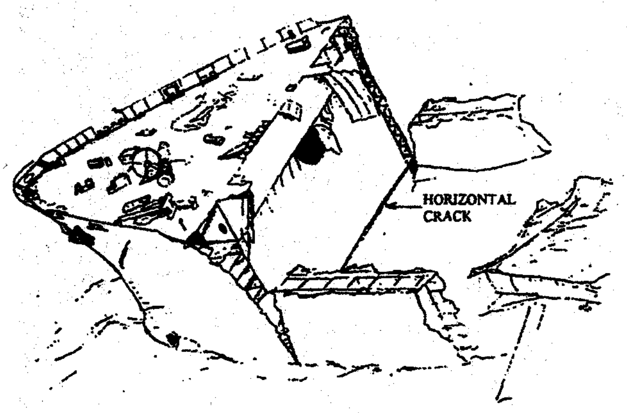


FIG. 13: Sketch of the Bow

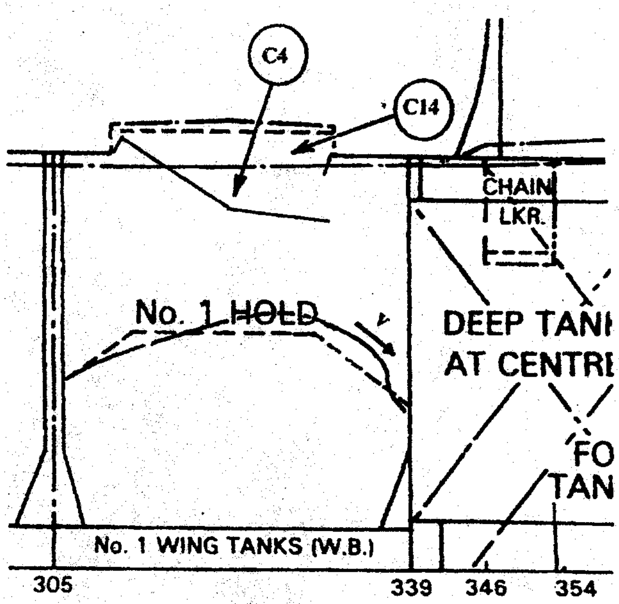


FIG. 14(a): Sketch of No. 1 Hold and Ore Slide

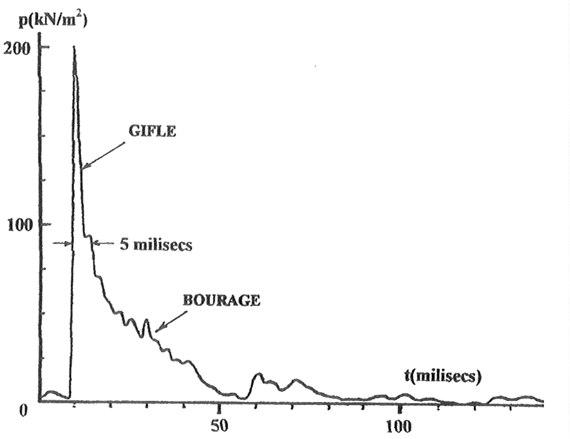


FIG. 14(b): Pressure vs Time Loading

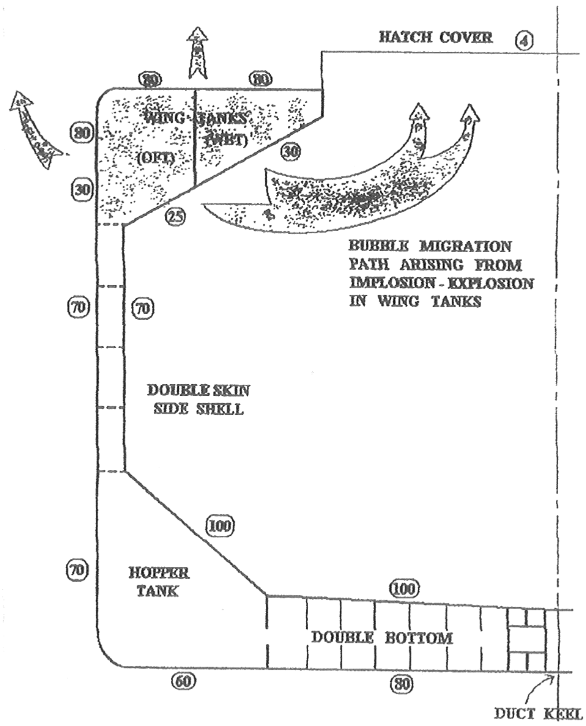
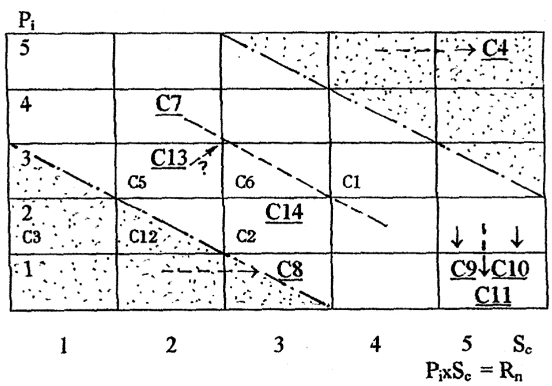


FIG. 16: Cargo Space Voids and Estimated Implosion Pressure Heads P_H (m) of Seawater



FE VULNERABILITY:

C4	Hatch Cover Collapse	5 x 5 = 25
C7	Fore Peak Flooding	4 x 2 = 8

OTHER SCENARIOS:

C8	Cargo Shift/Liquifaction	1 x 3 = 3
C9	Propulsion Loss	1 x 5 = 5
C10	Rudder Loss/Steering Gear Failure	1 x 5 = 5
C11	Explosion/Fire in Engine Room	1 x 5 = 5
C13	Pooping Actions	3 x 2 = 6
C14	Hatch Coaming Failure	2 x 3 = 6

FIG. 18: Final Risk Matrix for m.v. *DERBYSHIRE*

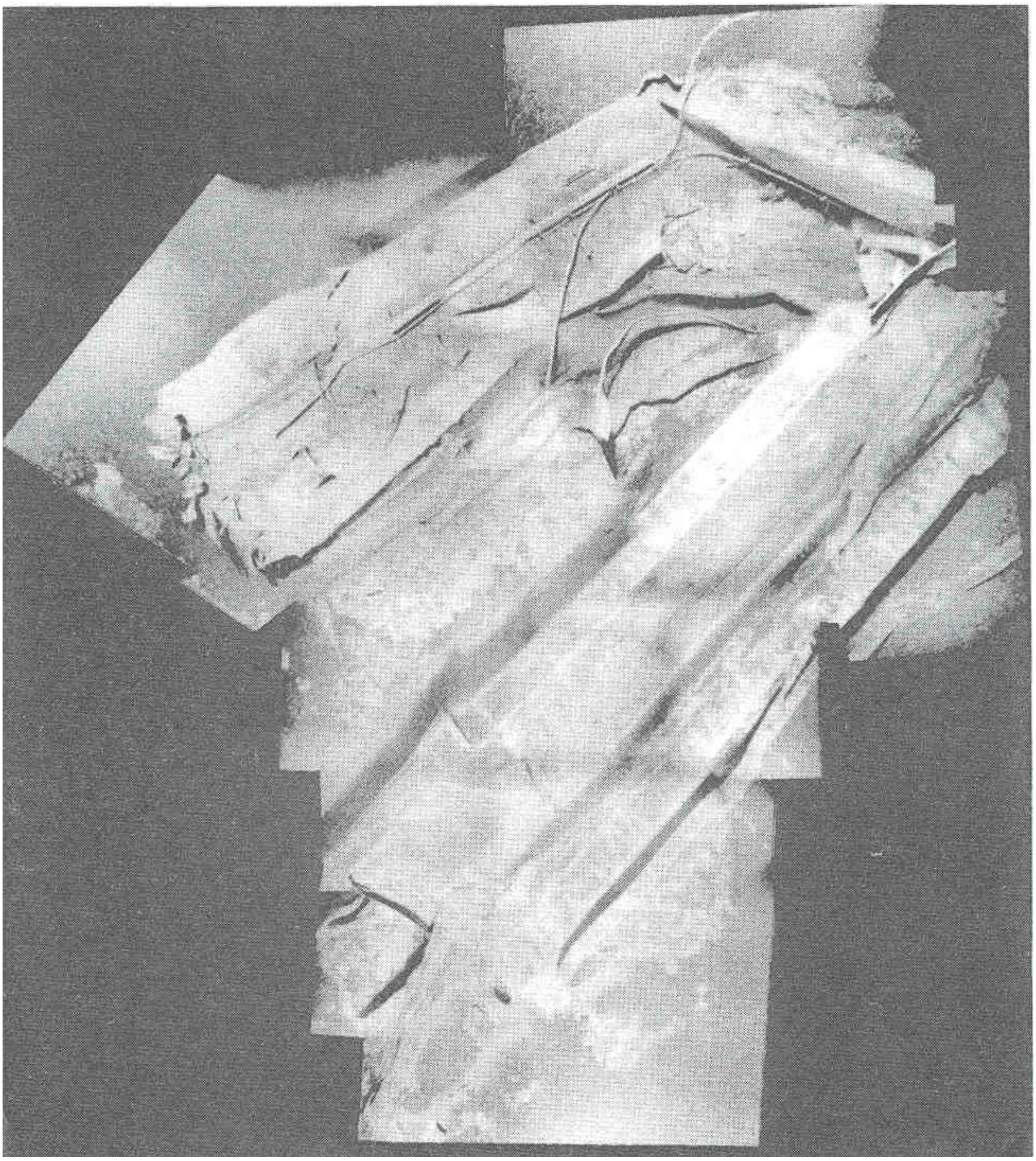


FIG. 15: The Remains of the Inboard section of the Wing Ballast Tanks for Holds 8 to 9 Starboard (target C230)

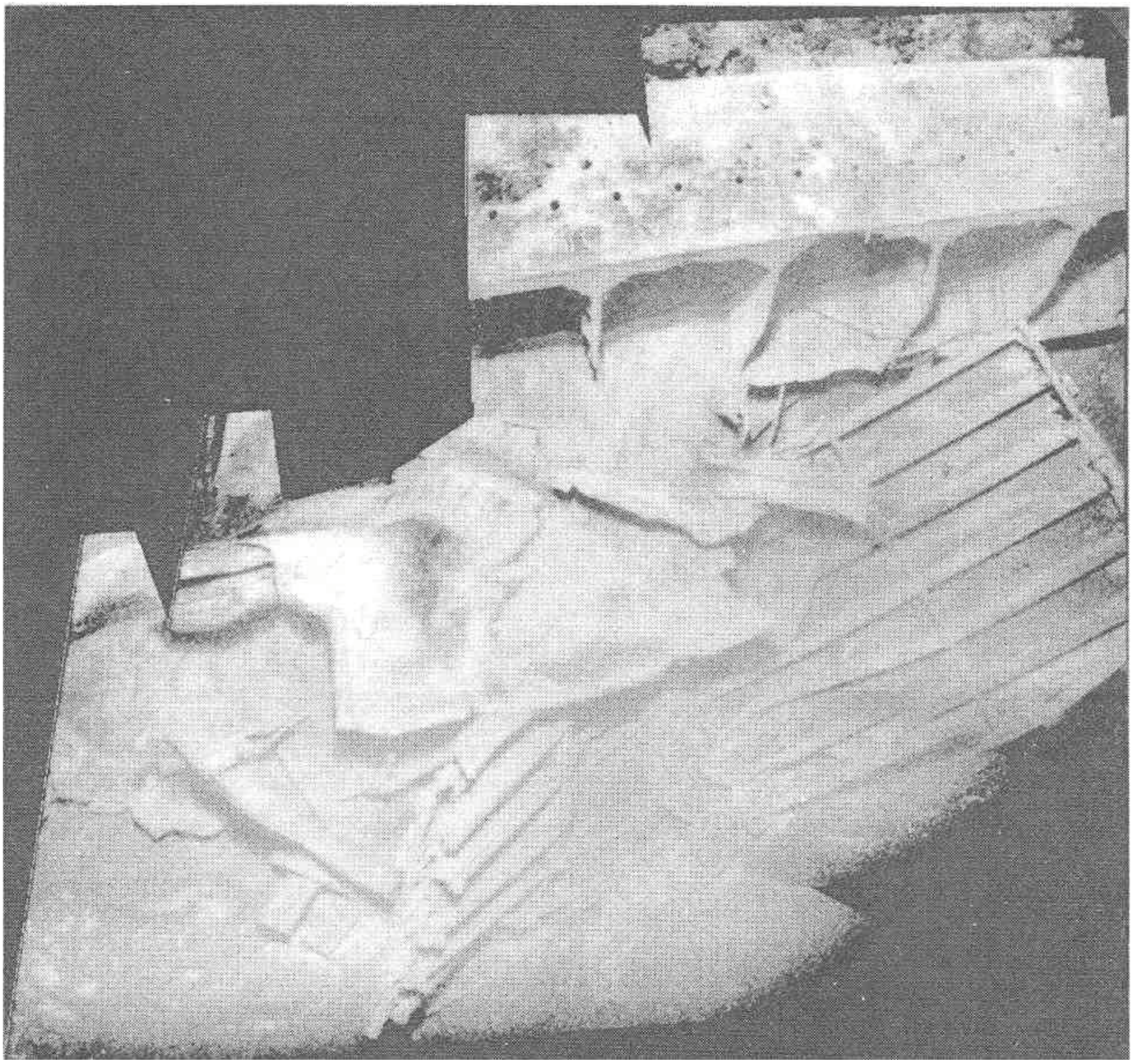


FIG. 17: Half of a Hatch Cover HD and Adjacent Part of Coaming

WRITTEN DISCUSSION

Mr Ernst Vossnack. I fully support the author's assessments that DERBYSHIRE was overwhelmed by a huge front wave, that the bow height was too small, that the hatchcover to No.1 hold collapsed and that No.1 hold flooded with the forebody.

I conclude that the freeboard regulations need to be reviewed by IMO giving increased bow height and that gross tonnage measurement should be banned (Fatal Influence of Gross Tonnage on Safety, Pollution and Sound Ship Design, E. Vossnack et al, September 1998)

Mr Paul Lambert and Captain David Ramwell. In describing modifications where hatchside girders ended at bulkhead 65 "with partial penetration welds forming a cruciform connection" Professor Faulkner notes: this "had been previous practice in the VLCCs which the firm had built".

Is there an implication intended: that such modification had to some extent been proved through established use? Whilst we am not aware of specific trouble in VLCCs (built at the yard) around the region where such modifications took place, given the second of the "twin aims" of the forthcoming Formal Inquiry, it is incumbent on the appropriate maritime authorities to investigate the efficacy of such practice.

In fact, insofar as it gives indication of the standards of quality at Swan Hunters at the time "*Derbyshire*" was built, the general history of these ships should also be investigated. (Our own investigation showed the distinct probability of some of these vessels not being built to high enough standard and brought me to the conclusion that, given the serious nature of the deficiencies that came to light, the lack of enthusiasm to investigate was engendered by a fear of certain parties being held culpable).

For example, "*Texaco Great Britain*" built in 1971, was scrapped but 10.5 years later, "hastened on her way to Taiwan by structural problems, and in particular extensive weld failures" (*FAIRPLAY SHIPPING WEEKLY*, 13th Aug.'81), "*Esso Northumbria*" cracked whilst loading in Bahrein. She also needed repairs because, in construction, longitudinals were too long - so chamfers were simply burned back to give correct length. This meant there was insufficient 'metal to metal' fusing. In the South Yard, Lisnave, she had 1030 such welds rectified: a massive job. And when she had left she was due to be followed by two sister ships requiring similar treatment. Mr. Williams and Dr, Torchio themselves mention the serious problems in "*Kurdistan*" in their assessment. "*World Unicorn*", "*Hindustan*" and "*Strait of Canso*" also suffered cracking.

We are not qualified to expound on B-60 Load Line Regulations; suffice it to say that if "this requirement could be met by "Derbyshire" it would seem to me "this requirement" itself needs fresh appraisal.

Whilst I know Professor Faulkner has always considered it unlikely that stress at frame 65 alone would have caused catastrophic failure, he has, in the past, considered the configuration in the area to have been a likely contributory factor to cause of loss.

"Hence, of the five possible failure scenarios which my students examined for '*Derbyshire'* in 1987, the progression of local structural cracking in way of bulkhead 65 leading to a catastrophic breaking away of the stern section was by far the most possible. It was interesting to note this was essentially the conclusion of the Department of Transport's first draft report..."

The above is an extract from a letter written in January 1993 by Professor Faulkner and sent to a Derbyshire Family Association researcher. The Professor kindly gave his permission for his letter to be used at time of writing.

In the same letter: "With regard to quality of construction, I believe John Jubb (Welding Consultant) got nearer the truth than anyone by drawing attention to the history of cracks, misalignment, substandard welds and doubler plates in the class of ships, and to the possibility of brittle fracture in material with no proven fracture toughness. "

We cannot agree (1.2 "The Loss and Events up to 1986) with: "As there was no available evidence, nor any established evidence of structural or other weaknesses in the six ships, the Government decided not to hold a formal investigation into the casualty. " The evidence was available; it simply was not looked for. The very first of the six ships, for example, had problems of a serious nature when on new ship trials - problems associated with bad workmanship.

Whilst accepting that the "introduction of the abnormal wave …opened (his) eyes to the extreme vulnerability of the hatch covers" (Letter from Prof. Faulkner to DFA Researcher June 1996) can we assume that Professor Faulkner still considers it possible that the ship failed at frame 65, but he now thinks it more likely that the No.1 hatched failed?

Regarding the "cracking in the vicinity of frame 65 in several ships in the class" (1.2), it was not (at the time) the DFA that was gathering information; it was Salvage Association surveyor, Peter Ridyard (who lost his son in "*Derbyshire*"). And it was not "several" but all the ships in the class that had experienced cracking in the vicinity of frame 65. Without this initial investigation by Peter Ridyard the campaign launched by the DFA after the Court delivered its conclusion in 1989, would never have got off the ground. Seafarers owe a great debt to Peter Ridyard; his work led to enhancement of their safety.

The 1986 report by the DOT displaced a draft version published in July 1985. It has still not been explained why two radically different conclusions can grow from such a similar technical base - one finding the greater probability being catastrophic hull failure, the other saying the ship was probably overwhelmed by the weather. To say, as did the DOT Minister at the time, "It was changed in the light of comments received and further information which became available", without telling what those comments were and who made them, and what was that further -information that led to the change, is to say nothing. The DFA has established, by elimination, that it was Lloyd's Register that made the input on reading the 'draft' report that caused the conclusion to be changed. And since LR held vested interest in the outcome of any investigation this fact should be recorded.

It was Nicholas Ridley MP who refused to put the DOT draft report in the House of Commons library, and who gave the "reasons" for the rewrite of the 1985-'86 reports. Ridley was a Director of an engineering subsidiary of the Newcastle shipbuilding consortium, Swan Hunter and Wigham Richardson.

"*Kowloon Bridge*" was cited in the March 1986 DOT report as evidence of the sound construction of the ships in general, the report refers to her as "*English Bridge*"). The DOT inspection of her in Bantry Bay, where the Captain had sought shelter and advice about craoking damage, was incomplete. The Minister told the House his inspectors had found no damage to suggest that the sort of damage that the some say caused the loss of "*Derbyshire*". Yet the inspectors had painted round cracks on deck in the area of frame 65 and two large girders were plainly visible welded across the bulkhead 65 on deck in photographs taken at the time. These girders were to compensate for that weakness known to exist and arising from the method of build in the area - a weakness shared with "Derbyshire".

As the Professor says, "*Kowloon Bridge*" was suspended on Stag Rocks by No.3 hold. But not only was the after part floating in good water, as it slowly filled, stresses were actually being relieved as they countered water pressures externally. The weather was fine at the time - in fact divers worked throughout the period. Despite all this she cracked across at frame 65, snapping the two girders as she did so.

A piece was cut from "Kowloon Bridge" frame 65 area and taken to the Formal Investigation, but, it was merely identified. However, the grade A metal it contained was tested at the Defence Research Agency in Dunfermline. The results were published. An abstract from the paper read :-

"Chemical analysis, tensile, Charpy, DW, NDT and fracture mechanics toughness data are presented for grade 'A' steel plate from the 'Kowloon Bridge', a sister ship of the 'Derbyshire'. The plate is shown to have poor toughness under dynamic loading conditions. "

"The results leave open the possibility that brittle fracture could have contributed to a structural failure in the 'Derbyshire' even at the high ambient temperature of 30° C which is reported to have existed at the time of the ship's disappearance".

"The results are felt to support the proposition that present classification society rules need tightening to reduce the risk of brittle fracture in ships. "

The DFA as a pro-active campaigning Association did not come into being until after the Formal Investigation(FI); so the DFA did not pressure the Government to hold the FI. Indeed, in retrospect, the FI was so unsatisfactory because the families lacked any influence; trusting in authority, they left it to others.

The FI failed to take into account properly the cracking of all the six ships at frame 65; it simply ignored the evidence available from "Kowloon Bridge", despite the wreck being the cause of the FI into the loss of "Derbyshire" being held. It failed to call witnesses the families thought as vital to their case.

The DFA was <u>not</u> outraged by the "lack of firm conclusion regarding frame 65"; the families were outraged by the dictatorial stance of the Wreck Commissioner and their lack of any input into the proceedings.

It could not have been that "The charisma of Professor Bishop in particular had a profound effect on the DFA" because the DFA hardly knew Professor Bishop. unfortunately - because they could not express it personally - the DFA's respect for Prof, Bishop only grew after he had died when all that he had done for seafarers' safety became known. It grew the more when set against the seeming indifference of others who could have done so much to resolve the 'bulker problem' but did not.

With regard to the Brunel team's hypothesis, the DFA did not (and does not) believe "absolutely that this apparent combination of poor construction and 'horns of high stress' at the frame 65 connections was the final proof beyond any "doubt that this was the cause of the loss."

We must be allowed to put the record straight: The DFA believes that the attention given to the probability of failure at frame 65 has not been commensurate with the strength of the evidence pointing to that probability. This is not to be construed as the DFA's certainty that is where the ship failed. If new evidence emerges with greater weight than that supporting failure at frame 65 then the DFA will, of course give due diligence to its consideration of such evidence. The DFA's "outrage" has been reserved only for those who have placed obstacles in the way of getting at the truth about the loss of the "Derbyshire" - <u>whatever that truth turns out to be.</u> .

Given the history of hatch cover failure in the six ships concerned, set against the history of hatch cover collapse in that same fleet, it is difficult to see how the Assessors managed to hoist "Hatch cover collapse" eight points ahead of "Deck cracking Frame 65" in their "Risk Matrix".

It is also difficult to argue with Professor Faulkner's figures when one has little or no technical background. But if common sense is allowed to intrude, very often these complicated "Risk Numeral Components" are rendered superfluous anyway; how many of the six ships suffered cracking at frame 65. How many of the six ships suffered hatch collapse in bad weather?

None of this is to argue that hatch covers do not need re-assessing for their ability to withstand 'freak waves'. In general there is a need for such a study, together with the associated problem of too small a freeboard especially in long hulled heavy ships - and the problem of the flush deck. But it is difficult to see how "*Derbyshire*" can suggest itself as the natural starting point for such a study.

Professor Faulkner pays tribute to the DPA: "Without their dogged perseverance and patience...the shipwreck would have remained an unexplored mystery". But, whatever the differences of opinion with Professor Faulkner, the DFA certainly recognises <u>his</u> tenacity and doggedness

Douglas Brown (Member): I have carefully read both the DETR report prpared by the UK/EC assessors and Professor Faulkner's present paper

It seems to me that there are two common conclusions, where both papers are in total agreement

a) that the frame 65 failure (referred to as C1) is ruled out as a cause of the Derbyshire's loss.

b) that the forward hatch covers were too weak to withstand abnormal storm waves.

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I would be interested in the author's view of the differences in approach by which each report reaches the above conclusions.

It would also be interesting, following his work on the Derbyshire, to have the author's views on the professional skills, analytical or otherwise, which he considers necessary in important investigative work of this type.

Dr K R Drake CEng MICE. Professor Faulkner is to be congratulated on producing such an excellent and wide-ranging paper. In particular, he has highlighted the hazards of steep-fronted nonlinear waves. With regard to the profiles of these waves, I have been working on the development of a simple design methodology which is based upon an underlying gaussian random process (Drake, 1997 - referred to by Faulkner). I have recently applied the methodology to the estimation of exceedence probabilities for relative motions between a slender vessel and the adjacent wave surface in head seas. I have concentrated on the relative motion at a longitudinal position which is representative of the no. 1 hatch cover location. The analyses have included the following:

- vessel heave and pitch dynamic responses;
- second order nonlinear corrections to the wave surface profiles;
- vessel lengths of 250m and 300m;
- Pierson-Moskowitz and mean JONSWAP wave spectra (i.e. γ = 1 and 3.3);
- the full range of survival seastates prescribed by Buckley (refer to Figure 4).

I am in the process of writing up this work for publication, however, I would like to take this opportunity to express overall support. My findings broadly concur with those of Professor Faulkner, however, there are some notable differences in the detail. Regarding the occurrence of relative motions in excess of the critical 13m (refer to Figure 12), I have found the following:

- for linear waves, the probability of exceeding 13m relative motion is small;
- for nonlinear waves, the probability of exceeding 13m relative motion is significant (although it is lower than the values given by Faulkner);
- the computed second order wave profiles do not warrant any reduction - compared to pyramidal waves - when trying to establish an average height of water over the hatch covers.

William H. Buckley. This paper is rational, thorough, quantitative and definitive as to conclusions and recommendations. It is quintessential Douglas Faulkner. As a proponent of a first principles design concept (FPM) I am gratified that his analysis of the *DERBYSHIRE* wreck is so illustrative of a first principles approach to casualty analysis. My comments will be concerned mainly with the paper's Prime Recommendations and Other Recommendations and with furnishing certain additional information which supports them. First a brief outline of FPM which is further described in subsection 1.3 of the Faulkner and Buckley (1997) reference. This methodology involves design envelopes of worldwide wave conditions, the identification of potentially critical time domain wave characteristics within a Survivability envelope, methods for quantifying critical design loads (static, dynamic, cumulative), associated strength analyses and finally criteria as to acceptable or unacceptable structural behavior. In the analysis of the *DERBYSHIRE* casualty only seaway conditions associated with Point (a) of Fig. 4 of the paper need be considered.

The following is abstracted from subsection 5.22 of Buckley (1983) and concerns the *MV CHU FUJINO* wave damage incident (Based on Telex report of interview with ship's master of *MV CHU FUJINO* prepared by Adm. Merlin H. Staring, U.S.N. Ret.):

The M/V CHU FUJINO was a 127,000 ton bulk carrier 261m long, 41m wide with a summer draft of 17.6m. It had a flush deck and a bridge located aft. On December 28, 1979, while fully loaded the ship was proceeding in a severe winter storm at a speed made good of about 1 knot. Winds had increased to 75 knots producing observed wave heights of 30 to 60 ft. At about 15:40 hours the ship was struck by a single wave estimated to be about 100 ft in height. The wave approached from about 70 deg. Off the starboard bow and thus at an angle approaching 45 deg. to the prevailing seaway. Damage consisted of the following: Stbd bridge windows smashed in. Stbd lifeboat washed away and davits flattened to deck. Two stbd aft liferafts and foredeck liferaft washed away. Wing/ballast tank vents damaged, baffle plates washed away, floats fell out, and wing tanks flooded. Bolted engineroom access plating on deck fwd of bridge buckled and torn loose, allowing water entry to engineroom. Foc'sle storeroom' fwd pumproom, No. I and No. 2 double bottoms port and stbd, forepeak tank all completely flooded. Some water in No. 3 topside tank stbd.
Because of the engineroom flooding the ship was totally without power and steerageway for over an hour. Flooding forward was such that the ship became 25 ft bow down by 0130 hours on the 29th, with the fo'c'sle awash. At this point the Captain radioed for U.S. Coast Guard assistance; however, the seas abated sufficiently by morning that the flooded spaces forward could be pumped out and the ship proceeded safely to Honolulu for repairs.

The height of the damaging wave is estimated as follows. The reported swell heights of 30 to 60 feet are interpreted to mean that 60 ft was roughly the height of the highest waves at the time. The significant wave height would then be estimated to be about 60/1.65 or 36 ft which is close to the lower value cited. For this significant wave height, a truly episodic wave is estimated to be 2.5 x 36 = 90 ft high which is slightly less than the reported value of 100 ft.

The *CHU FUJINO* was an OBO bulk carrier built in 1971. While somewhat smaller than *DERBYSHIRE* it is nevertheless similar in size and configuration. There are however several important differences from a damage point of view. Flooding forward was of a progressive rather than catastrophic nature following encounter with the abnormal wave. The watertight integrity of an access cover immediately forward of the deckhouse was destroyed as opposed to a cargo hatch forward.

This brief summary of the seaway conditions and wave impact damages associated with the *CHU FUJINO* helps to reinforce the prime (PR) and other (OR) recommendations of the paper in the following respects:

- The Survival Wave design approach of OR no.3 is applicable to the extreme seaway conditions which were encountered.
- The engineroom access cover having been buckled and torn loose demonstrates that the increased load criteria of PR no. I should apply to all weather deck hatches as recommended.
- The recommendation of OR no. 11 (design and protection of weather deck ventilators) is indeed valid for this ship.
- The recommendation of OR no. 10 (remote pumping of forepeak spaces) is also relevant to this casualty. The master was in fact injured by a boarding wave while pumps were being installed forward. If the seaway had not abated in the morning even this hazardous operation would not have been possible.

Figure 19: Abnormal Wave Encountered by *M V SELKIRK SETTER* During Winter Storm

- The recommendations of OR no. 1 (design for "near miss" scenarios C7, C13, C14) should be expanded to consider wave impact loadings in beam seas. During the I hour interval while the engine was disabled due to flooding and the ship lay beam-to, attack by a severely breaking wave might have lead to more serious problems.

Given that wave impact loadings are the primary cause of ship heavy weather damages as determined from IU.S. Coast Guard and U.S. Navy heavy weather damage information, see Sections 3.0 and 7.0 of Buckley, W.H. (1983), the fact that Prof Faulkner finds the 1966 ICLL design load criteria for hatches to be deficient (and those for coamings to be essentially non existent) is not, altogether surprising. Figure (19), here also illustrates why we really should not be surprised. The vessel shown is the Canadian bulk carrier *M.V. SELKIRK SETTLER* which was proceeding in overtaking seas (at about 90 deg. from the intended course) in a survivability level storm corresponding approximately to Point (a) of Figure 4 of the paper. The photo was taken by Captain George Ianiev who was then second mate of the ship. The wave shown was the largest observed during this multi-day day storm and caused the master to wonder whether the ship would survive. (see Buckley (1994)** What does the photo tell us?

- This wave which severely impacted the ship certainly did not produce a static loading as assumed in current hatch cover design criteria.

- Although the ship proceeded down seas as closely as the helmsman could manage for fear of broaching, the impact was largely from the side which suggests a more general need for design for beam-on loadings than might otherwise be considered.

- The height of this relatively short crested wave away ffom the hull is less than at the hull. Clearly the presence of the hull caused the wave to rise up and then drop so that the impact on the deck and hatches was more like that due to a plunging breaker than a spilling breaker. Had a ship been proceeding into the same seaway with a deck house well forward, a life threatening impact might well have occurred. Of additional significance is the fact that the height of the impact on the ship structure (say at bridge windows) is likely to exceed that of the undisturbed wave.

Basically this photo suggests that the structural loadings to which a ship may be exposed upon encountering an abnormal wave may not correspond to the simplified wave geometry and loading assumptions now in vogue. It also suggests that major advances in our knowledge of abnormal waves and their effect on ship structure is mandatory if first principles design is to become an option.

While in the long-term first principles design is to be desired, Prof. Faulkner is realistic in recommending new strength criteria for hatch cover design based on a substantially increased static head of loading together with a factor of 1.5 applied to define the loads which are to be withstood without collapse. In PR no.4 he further recommends that these requirements be employed in replacement hatches (or presumably suitably strengthened existing hatches) without delay. This discusser strongly endorses these recommendations because of the need for immediate improvement in the ultimate strength of hatches on large OBO ships. In OR no. 13 Prof. Faulkner further suggests that

"Guiding principles and practices for forensic analyses of shipwreck should now be established…". This paper is surely a landmark step in that direction.

**BUCKLEY, W.H., Stability Criteria: Development of a First Principle Methodology, *Proceedings of Fifth International Conference on Stability of Ships and Ocean Vessels, Vol.3, November 7-11, 1994*.

BY R.V.TURNER, BSc (Fellow & Honorary Vice President): The Author is to be congratulated on his clear exposition of the most likely causes of the loss of the ship, and on the measures needed to prevent further losses including the vital and immediate replacement of the existing inadequate covers on hatches in similar ships. Some of us who have had experience of loading VLCC's, even if only during Contractors' Sea Trials, were unsure during the May 1998 RINA/DETR Colloquium, of the validity of the official Assessors' conclusion regarding the importance of flooding of the forward spaces (C7 on page 17 of the DETR Summary Report). It is good to see from the Author's Appendix 2 that conclusion C7 is invalidated by the results of the more analytical assessment Appendix 1 to the paper is most welcome with its emphasis on the importance of breaking waves in the design of exposed ships' structures and fittings. The significance in ship design of the high pressures induced by breaking waves was referred to (based on the writer's personal experience) in the discussion of the "Bow Impact Loads on Ro-Ro Vessels" paper read at the 1997 RINA Spring Meetings, and although the points then made related to ferry design and not to bulk carriers, the physical phenomena are of course equally applicable.

The Author's assessment of the relevance of the 'gifle' pressures is not entirely clear from the text of Appendix 1, for although he presents the results of calculations of such pressures in the Table in the first column on page 36, he later infers that such pressures can be ignored by naval architects, as they arise from 'plunging breakers'. Clarification of this point would seem advisable, although it should be noted that the very high pressures measured on sea-walls resulting from plunging breakers have generally been associated (for well over 50 years) with wave impact on surfaces sufficiently smooth and continuous to permit extensive air-entrapment, leading to explosive pressure peaks.

Even if the effect of 'plunging breakers' can be safely ignored in ship design, our designers ought to be trained to give great attention to the effects of breaking waves generally. This requires understanding of individual wave dynamics on the lines of the descriptions and calculations contained in the booklets handed out so generously by the colleagues of Mr W. van Geuns of the Netherlands, during the RINA/IESIS Joint International Conference at Glasgow, in October 1997. For far too long our Profession and the mathematicians have dealt mainly with the sea as a statistical phenomenon, ignoring the fact that in the extreme it is the physics of a few really large waves that will cause disaster, as was almost certainly the case in the sinking of the DERBYSHIRE.

However it is noteworthy that the Author has not referred to the dynamic pressures which can arise from the impact on near horizontal surfaces of large masses of water descending from high above the bows of large bluff-bowed ships. It appears from reports in the literature that large masses of water are so thrown upwards, as one would expect from these ships behaving more like half-tide rocks than normally free-floating structures.

In this connection the destruction of the DERBYSHIRE's starboard windlass by the action of the sea is significant as was pointed out by Captain J. Richardson (1998). It is most unlikely that impact with some part of the wreckage after sinking commenced could have caused the damage because according to the DETR Summary Report (page 11), the anchor cable is still intact. Unless the windlass was of unusual construction, the water velocity needed to break the main gypsy wheel away from its bearings must have been extremely high, higher perhaps than would be expected to result from a 'spilling breaker'. There is some anecdotal evidence of structural damage caused by descending masses of water and it would be valuable to learn if the Author has other data on this phenomenon because of its significance to the design of hatch covers forward on large bulk carriers.

In view of the overwhelming body of evidence in this paper and in many of the other references listed, it would important to the Profession if the Author could say whether he has been informed that the urgent action he refers to in his Recommendations on the need to replace the most vulnerable now published, and that there is thus little or no significance to be given to the Assessors' assertion (para 32 of that Report), that the initiating event was flooding through the stores hatch, which they had claimed was not properly secured hatch covers on existing ships, is in fact being undertaken by shipowners in advance of changes in IMO and National legislation.

Roland Grard. Like many, I have been following the various developments concerning the sinking of mv *Derbyshire* for some years, and I was impatient to read the UKIEC Assessors Report which was published in March.

As a former Polyvalent officer, I have sailed as both Deck and Engineer Officer and, whilst I may not have enough knowledge to appreciate some of the more technical explanations in the Report, I feel I do have enough competency and experience to be able to make observations and judgements in some important areas. And, in these areas of my understanding I have identified several stark contradictions that then leave me unable to place confidence on the Report, especially with regard to the Assessors main conclusion.

The presentation and substance of the Report may impress the general public, especially when computer animation is made to translate the "technical evidences" understandable sequences of events. However the main conclusion is speculative, and yet the Assessor/not only seems to deliver it as representing the actual loss event, they avoid properly considering other loss scenarios by then arguing that, since their conclusion is right, the others could not have occurred. My letter requesting clarification from the Assessors met with no response

In contrast I find Professor Faulkners Paper takes an overview that properly considers the views and experience of others, both theoretical and practical, making it clear that the ship did not sink due to crew negligence and as a consequence of a fore access lid had being left open, as maintained by the assessors. It is more probably the case that the cause of the loss was the result of inadequate design rules for this type of vessel (large combination carriers) giving insufficient forward protection as well as insufficient strength of hachcovers.

Probably due to a desire to focus on main points, Professor Faulkner has not reviewed all the aspects mentioned in the UK/EC Assessors Report. However, I would appreciate the Professors opinion on the penetration, or apparent penetration, of the bow in the seabed because this was associated with the theory of forward flooding and consequent collapse of no. 1 hatchcover.

I would also welcome clarification on the possibility that the hull broke at or near the surface (i.e. before the onset of implosion mechanisms) during the rapid final sinking sequence (Ship Bending During Plunging as referred by Professor Faulkner). Considering the prevailing awful weather conditions and the considerable stress resulting from the flooding and the loss of buoyancy of the fore end, it seems unlikely that the hull girder remained intact until the implosion mechanisms started. The UK/EC Assessors rejected the possibility of the separation of the hull into two parts on the grounds that none of the cargo spaces between frame 65 and 339 were found intact. I fail to see how the fact that all cargo spaces suffered implosion/explosion damage precludes rupture of the hull having taking place in the final stage of sinking, especially if shock waves constantly traverse the structure in the process. It is to be noted that the UK/EC Assessors report describes these implosions / explosions actions as equivalent to two tons of TNT in each hold. Such mechanism could have also seriously damaged adjacent areas of the hull fracture which, withiout such mechanism, may have been expected to be found with relatively little damage during examination of the wreckage on the seabed.

I regret Professor Faulkner had to resign as UK appointed Assessor in order to put his own view. I strongly believe that the Professor's input, had he co-authored the Report would have made a better balance.

My hope is that the Professor's courage and tenacity is rewarded by media coverage for his Paper at least on a par with that coverage enjoyed by the publication of the Assessors Report. Such media coverage would, additionally, achieve the balance lost by the Professor's exclusion from writing the Assessors Report by demonstrating more feasible probability, whilst, at the same time, highlighting some of the elementary errors in the Assessors Report.

Dr Andrew G. Spyrou (member). The "Analytical Assessment of the Sinking of the MV DERBYSHIRE", by Professor D. Faulkner, has been done with impressive thoroughness. His observations, and deductions using reasoned facts, and constructive criticism of others makes the assessment a learning experience for the international maritime industry. The author has explained beyond reasonable doubt the most likely terminal cause for the sinking of the "DERBYSHIRE". His comments, however, invite historical reflection on maritime tragedies, particularly in the last 30 years.

Admittedly, the arguments in support of the scenario causing the loss can only be circumstantial. The observations leading to a logical explanation of what caused the ship to go down under appalling sea conditions leaves little doubt as to what caused the rapid sinking, with very little time to transmit the distress signal. The captain of the ship was an experienced seafarer who had already indicated they were proceeding under difficult conditions.

Purely from the point of view of safety-at-sea, it is worth considering here what occurred on two previous occasions where maritime. tragedies caused serious concern.

In Prof Faulkner's Presidential Address in October 1995, before the institution of Engineers and Shipbuilders in Scotland (IESIS), titled "Ship Safety and Public Concern", (paper No. 1547), mention was made of the loss of several hundred lives that occurred between the years 1833 and 1836. This led, in 1836 to a "Select Committee to inquire into the causes of ship wrecks". The Committee's report concluded that among the various causes, "the most frequent and the most generally admitted was defective construction of ships".

More recently. 1969 marked the beginning of the sad safety record of the first generation of hulk carriers. On January 5th 1969, the Japanese registered 54,700 dwt bulk carrier "BOLIVAR MARU", built in 1966, was lost in heavy seas, in North Western Pacific, east of Japan, with all hands on board. On February 10th 1970, the 56,400 dwt, Japanese registered bulk carrier "CALIFORNIA MARU" built in 1965, was lost in the same area with all on board. Both ships were carrying iron ore from the western United States to Japan.

These two disasters prompted the Japanese authorities to conduct, in 1971, a detailed investigation to establish the cause of these two losses. The 4-panel investigation was completed and the panel's report was published in 1976, (in Japanese).

It is of importance to note that the six "BRIDGE" class OBO carriers, one of which was the "*DERBYSHIRE*", were being designed and were being built in the UK during the years 1966 to 1976. Apparently the sinking of the Japanese bulk carriers and the report that followed, did not arouse the interest or curiosity either of the builders or the Classification Society who looked after the "BRIDGE" class ships to find out what caused the losses of the Japanese vessels despite the network of intelligence available to a Classification Society.

Assuming it was pride that persuaded those involved to mind their own business, we should then look at the bulk carrier safety-at-sea record in general which was regularly reported, above all, in London. Between "*BOLIVAR MARU*", "*CALIFORNIA MARU*", and the "*DERBYSHIRE*", dozens more bulk carriers and hundreds of seafarers were lost, mainly in the same area, but very little was reported about these losses. In fact, between 1990 and 1994, a total of 36 bulk carriers and more than 700 seafarers were lost.

It was in February 1997 - thirty years after the "*BOLIVAR MARU*" went down - that a London published maritime magazine expressed "BULK CARRIER SHOCK", briefly mentioning bulk carrier losses and quoted IMO, declaring 1996 "the year of the bulk carrier" Then IACS introduced what became known in the industry as "retrospective standards" prompting accusations that the Classification Societies were classing unseaworthy ships.

It is worth noting, the two Japanese bulkers, as well as the "*DERBYSHIRE*", were relatively new ships, designed, built, and registered in countries with a long maritime tradition. It is also important that the "*DERBYSHIRE*" was of double-hull configuration.

Where IMO's Technical Committee and the Classification Societies failed, was to acknowledge long after they should have, that the maritime industry had a serious problem. When they finally acknowledge that the problem did exist, they hastily introduced retroactive rules and regulations without proper evaluation of the new requirements.

In October 1997, the European Transport Safety Council, called for a European Maritime Safety plan driven by statistical targets for cutting the number of deaths and injuries at sea. The report of the independent industry group's first review on Shipping Safety, mentions the average maritime death and injury figures is 25 times more than air travel. Among other recommendations, the report includes a review of aspects of bulk carrier designs.

In fact, during IMO's 20th Assembly Session in November 1997 in London, which met concurrently with the SOLAS Bulk Carrier Conference, it was found that the interpretation of the definition of "Bulk Carrier", as given In Chapter IX of SOLAS 1994, as amended in 1994, required clarification. As for the Classification Societies, they have now recognised their failure to act sooner, and that bulk carrier studies have not been as progressive, leaving bulk carrier designs at a less mature level - this, after 25 years of continued casualties.

The November 1997 London Diplomatic Conference on the safety of bulk carriers was to adopt amendments to the International Convention for SOLAS. No one who follows such conferences anticipated that the new SOLAS Regulations would be an effective remedy for the large number of ships lost during the past several years due to catastrophic structural failures, especially ships carrying heavy-density cargoes. There is no mechanism at IMO capable of addressing the sad safety-at-sea situation by means of developing guidelines and standards for the international shipping industry. Also missing, is a monitoring arrangement to confirm that new guidelines, amendments and requirements really work.

At the conclusion of the Conference, it was widely felt that, not withstanding the political impact of the ship losses, the technical working group established to finalise the draft SOLAS amendments had no competence, skill or time to address the many outstanding safety issues because those involved are out of touch with service reality. This amply demonstrates the urgent need to review and reinforce the technical aspect of IMO's composition to deal with the. pressing and important technical issues confronting the Organisation.

Michael Grey, writing in Lloyd's List on "Designing vessels able to cope with the perfect storm" commented recently:

"Somebody along the long design road, was persuaded that a bulk carrier without a raised forecastle was acceptable", and that "the possibility of heavy green water stoving in, a forward hatch cover was remote…"

The implication of Grey's comments involves ethics which is a poorly defined subjective standard, influenced by an individual's upbringing, education and experience. Our learned institutions do provide some general guidance on professional conduct. The ship design engineer, however, who is frequently an agent of a profit-making organisation, faces the dilemma of economic, and consequently uncompetitive impact of improving the safety of a design beyond that required by existing rules and regulations.

What should the. shipping industry do to avoid future tragedies, especially since the competition between Class Societies has been intensifying ?

It makes no sense, to call for "Panels of Experts" to be formed after tragedies such as those of the bulk carriers and RO/RO Passenger Ferries, to try to solve safety problems. Acting after the event is not the way to promote maritime safety.

IMO, given the authority, is in a unique position to expose inadequacies. The loss statistics in ships, in public and seafarers lives call for a tough and intelligent effort that is honestly applied, even if it means that IMO legislation exceeds the requirements of a Classification Society. While the international shipping community supports the IMO and IACS, there is demand for fresh thinking and a new approach to old problems.

To avoid detrimental over-regulation that penalises well-run commercial ships, and to achieve a comprehensive and sustained effort to develop safer ships, it is proposed that an International Ship Evaluation Panel, (ISEP), be established to coordinate the efforts, and to harmonise the work for the design of the hull structure for commercial ships that will retain afloat and upright even under extreme sea conditions.

ISEP would be composed of a select number of highly qualified naval architects/hull designers/marine engineers of international stature and intellectual integrity, whose responsibility will be to assist IMO's Technical Cooperation Committee that deals with maritime safety and protection of the marine environment. The Panel's mission will be to:-

- Develop a rational hull design approach which is practical, and affordable, and to consider a broad range of safety requirements while keeping pace with technological developments, new materials and design concepts to improve safety-at-sea.
- Promote safety standards based upon sound technical judgement, through theoretical analysis, experimental testing and inservice experience.
- Provide an international forum for technical cooperation among researchers and practicing ship design engineers with the aim to improve safety-at-sea.

To achieve the above, it will be essential to recognize that ISEP and the regulators are part of the same team, endeavoring to integrate design criteria which will make ships safer, an.1 here is where attitudes require change.

The Panel's freedom from political and commercial pressure, would enable its members to investigate, analyse, and make decisions on safety problems facing the shipping industry, without fear or favour.

Establishing ISEP, will allow IMO to devote more time and resources to a sector of the shipping industry, the "human element", which is generally reckoned to account for about 80 per cent of accidents at sea.

In the closing paragraph, Professor Faulkner mentions the common theme among masters who have little faith in the safety aspect of weather routing. I recall many complaints I received from our bulk carrier captains, regarding the difficulties they faced because of the accuracy of weather routing.

Today, there is a relatively new activity, even though its beginnings can be traced to classical times of ARISTOTLE (323 BC). This new activity, known as OPERATIONAL OCEANOGRAPHY, has been successfully developed in the USA and Japan.

Accurate predictions of the sea-state, that is the ensemble of all characteristics both physical and dynamic on multiple space/time scales was extremely difficult to predict because of the gap between ocean observations and their analysis.

Operational Oceanography has improved tremendously forecasting skills, and this is important and timely since the International Panel on Climate Changes, (IPCC) has warned on the impact of global warming, projecting disasters for areas vital to international shipping.

With regard to monitoring accidents at sea, serious efforts are underway in the United States of America to develop a concept and create a mechanism, where information on sea casualties will be shared while providing full protection against liability to these involved.

If successful, the concept will be of great assistance in establishing data bases that will allow people to examine across the board, at the industry's reports provided under the concept. This is something that is often impossible to obtain today, and will have a very important impact on maritime safety, once such a mechanism is accepted internationally it will enable those interested, to examine why accidents at sea occur.

The feasibility and methodology are still under discussion, and may be some years from now before the idea could become operational

Professor K.J.Rawson: (Fellow & Honorary Vice-President) Council has been courageous to encourage at this time a highly significant contribution to the debate on the loss of the Derbyshire, even though, in the best traditions of the

Institution, it puts conventional wisdom to the sword. The matter is becoming esoteric, in that few people will have studied in depth the wealth of references upon which the Author so lucidly draws. What, however, is abundantly clear is that, despite the earnest endeavours of researchers, Assessors and Inquiries, we cannot be sure of the initiation of her loss. Professor Faulkner has, perhaps, best succeeded in investing a sea of speculation with logical argument and for this we should be grateful to him.

Once again, we see old crude rules almost certainly at fault, this time, those related to hatch cover loading and strength. Why has it taken thirty years and bulk carriers lost at the rate of six a year to discover this? Is there no feedback from sea publicly available by which responsible owners can insist on standards higher then those demanded by statute or Class? Surely, when there is doubt, one errs on the side of safety and altruism; the alternative is cynically to allocate one's moral responsibility to the insurance market.

While the use of FSA is to be warmly applauded, there is danger in forcing numeracy upon it where it is based upon whim or speculation and then investing the resulting pecking order with authority. Dismissal of unlikely events is the strength of FSA so that several possible scenarios should be embraced, 'just in case'. ALARP is, of course, far too vague to be of much help in selecting priorities and making choices. The shipping industry does not have a good record of considering 'just in case', as their defences of the losses of Ro-Ro ferries demonstrated. The recommendations by the Author are eminently sensible and fall mainly into the category of 'just in case'. They deserve the fullest consideration and implementation for new and existing designs.

We owe Professor Faulkner a debt of gratitude for an excellent paper and for his tireless independent pursuit of so important a matter.

Eur Ing D K Brown, M Eng, C Eng RCNC. (Fellow). The essence of this impressive paper comes in section 6.2 where it seems that the probability of the hatch covers being burst by one large wave is very much higher than that of slow flooding forward. In either case there seems an urgent need to protect and strengthen hatch covers. Breakwaters and stronger covers can be fitted at moderate cost to existing ships. New ships should have a forecastle whilst the value of a knuckle in keeping heavy seas off the covers should be examined.

I would quibble slightly with Lemma 3. In the sinking of the *Titanic,* the main hull girder failed on the surface but the two main pieces were held together for some time by the remains of the double bottom. However, nothing like that seems to have happened *in Derbyshire.*

Mr C V Betts, CB, FREng, RCNC (Fellow): it is good that the discussion of this formidable paper by one of our most eminent Fellows is to be made available to the reopened Formal Inquiry. It is, on the face of it, unfortunate that major disagreement should have developed between Professor Faulkner and the UK and EC Assessors. However, the truth can often be served more faithfully by intense debate than by ready agreement and it is to the RINA's credit that it has published what must be to some a controversial paper.

Having contributed to the discussion of the author's very similar SNAME paper mentioned in Appendix 2, and seen the author's response, I will only repeat here that I find Professor Faulkner's argument and every one of his conclusions most compelling.

In particular, and whatever the relative merits of the different views on why the DERBYSHIRE sank, the case for an immediate and substantial increase in the required strength of all hatch covers is overwhelming. I understand that some of the Classification Societies, and IACS, are persuaded of this and some action is in hand. Far too often in the past, the international shipping industry - and we naval architects - have been inexcusably slow in adopting changes that have been obviously necessary, despite the almost certain risk of further loss of life and property. It is imperative that this does not happen again, especially in the case of bulk carriers where the loss rate remains clearly unacceptable. I am sure that this paper will eventually be seen as having made a major contribution to remedying the situation.

Captain Jack Richardson, Master Mariner, (FNI). I consider Professor D.Faulkner's Assessment an extremely professional and in great-depth investigation into the disaster. The fact filled findings are both thorough and also impartial which also commends it. His comments and findings are fully backed up by logic, experience and the necessary mathematical formulae (most I will admit are beyond my comprehension!).

As an indication of the deep dedication of Professor Faulkner concerning this investigation, I make it one hundred and eleven varied references he has quoted in support of his deliberations and conclusions, truly remarkable! This contrasts starkly with the results produced by the DETR from the Assessors Torchio/Williams input.

As an experienced Master Mariner and Casualty Investigator (Retired some years) I have no argument whatsoever against this supreme, investigative and most informative effort of Professor Faulkner, he has performed a gargantuan task with his Independent Assessment, a stupendous effort for which he ia heartily congratulated, it was so excellently produced.

If only his safety advice and suggestions for the improvement of the Bulker and other Merchant Ship Standards are adopted and acted upon soonest then I am sure Merchant Seamen throughout the World will owe him a debt of extreme gratitude for vessels in which they can expect to survive almost anything Nature cares to throw at them whilst at sea. Most unfortunately some will still have to sail in the existing vessels with B - 60 freeboards until they outlive their commercial life (which could be quite short). The B -60 Freeboards should be abolished immediately to give those older vessels the additional freeboard they require to survive. It is only commercial greed that keeps them in existence.

Mr William duBarry Thacomas (Fellow). The events that have transpired since the reasons for the loss of Derbyshire were first mooted have led us to the point where we must weigh with great care the probable causes for each seemingly unfathomable accident. Thus is shown the importance of clearly considered forensic analysis to preclude situations similar to that of a jury found in error shortly after a hanging.

Professor Faulkner's deceptively simple matrix of risk numeral components in Table 2 offers an opportunity to assess the probability of various causes for a loss or other accident provided that the opinions of an assembly of experts are included in the matrix. The larger the sample, the better. The evidence shown on Table 2 is convincing, and especially so when it is viewed along with the riveting, if somewhat melancholy, images brought up from the ocean floor in the Derbyshire instance.

I am firmly of the opinion that, in the design of hatch covers, we have not stayed ahead of the curve; that is, we have not anticipated with clarity the inevitable "what if? " situation.

Although we have moved light-years from the tarpaulin-and-wedges days, there is ample reason to question whether some of the unexplained bulk carrier losses might have been the result of so-called state-of-the-art forward hatch covers encountering walls of water which simply overwhelmed them. Recall that Professor Faulkner's unforeseen scenario, C14 on Table 2, postulates that "the sea often springs surprises. "

When we experience, or even read about, 90 foot seas greeting *Queen Elizabeth 2* on the North Atlantic, or the environmental conditions described in Sebastian Junger's, *The perfect Storm*, or the loss of *World Glory* on 13 June 1968 (under conditions to which the crew of the tanker *Richard C. Sauer*, then only a few miles away and herself a less severe casualty, might attest), or any of a myriad of other close encounters with nature in extremis, then we must wonder whether we are not underestimating wave dynamics and the response of ships thereto. This last point is most critical, and although I believe that we continue to learn, we are not doing so quickly enough.

Finally, an element not mentioned heretofore (and not necessary germane to the Derbyshire case) is that of the physical condition of hatch covers. As we are constantly reminded today, there are well-maintained ships and there are others. Many of these others might be found amongst the unexplained losses?

Professor Faulkner's current efforts have certainly contributed not only to the understanding of the events that led to the loss of Derbyshire, but also much more importantly, to a realisation' that we have much difficult work cut out for us.

Christopher Grigson, MA, Ph.D., (Fellow). In (Lloyds Register 98), Table A4 of bulker casualties between Jan. 1990 and Feb. 1998, there are some very disturbing entries.

Type of bulkship	*Cargo*	*Displacement m^3, appprox*	*Fate*
Ore	*iron ore*	*130,000*	*Disappeared*
O/O	*iron ore*	*180,000*	*Disappeared*
O/O	*iron ore*	*200,000*	*Disappeared*
Bulk	*iron ore*	*190,000*	*Disappeared*
Ore	*iron ore*	*170,000*	*Disappeared*
O/O	*iron ore*	*220,000*	*Disappeared*
Bulk	*iron ore*	*180,000*	*Disappeared*
Bulk	*iron ore*	*90,000*	*Disappeared*

Eight very large laden ships, all in class, were never heard of again. All had hold volume 70% empty, and all had weak hatches.

Let us now put down in the simplest way, the physical facts which bear on this, as they are known and understood today.

(i) In storms, significant wave heights (1/3 highest waves) have been measured, with values as high as 14m.

(ii) In stationary conditions lasting 12 hours with H_s = 14 m, one wave with a height of 24 m will certainly be encountered. It will be asymmetrical, crest 60-65% of the height, or 15.4 - 16.6 m above calm sea level.

(iii) In B-60s laden, hatch covers are about 9 m above calm sea level.

(iv) The strength of hatch covers is fixed by regulations. At a specified stress, covers must support a depth of water of about 1.7 m. Covers satisfying the rule collapse if the depth reaches about 4 m.

(v) Thus in the course of 12 hours of such conditions, one wave will sweep the decks burying the hatch covers to a depth of water of 6 m. Collapse will occur, and the empty spaces above the cargo will be filled.

(vi) If the ship is in iron ore, about 3/4, of the volume of the holds is empty. Buoyancy will be lost. Probably very quickly.

This is not merely one among a number of conceivable scenarios which might happen. It is what the mechanics of the case analysed using the known statistical distributions of waves, state will happen with certainty. The case was worked out by Professor Faulkner in 1995 (Faulkner and Williams 1996). The depth of seawater above hatch covers of 6 m is actually an underestimate because no allowance is made for pitching.

Combining (i) to (vi), one may say that the *regulation* governing hatch cover strength is sure to lead to foundering of B-60s in dense ore if they are caught in extreme weather. To complete our understanding, the methods of operational research ought to be applied to determine the chance per annum that a B-60 in iron ore will meet a severe storm. A very simplified analysis indicates that the chance is of the order of one per annum, as the table above suggests.

To discover that a regulation is the root cause of these disasters is most unwelcome to officials, even though, in this case, no blame should be attached. For when the calamitous rule for hatch cover strength was decided in 1966, knowledge of wave statistics was new, and no one could conceive that the vastly bigger bulkships coming into service could meet waves as large as are now known to occur. Nevertheless, officials will resist tooth and nail the idea that a regulation is a principal cause of foundering.

After Faulkner had shown that hatch covers were critically weak, two further expeditions surveyed the wreck. It is interesting to learn that a great effort was made to verify the prediction that the hull sank because it broke in two at frame 65. In 1991 Bishop et al. had claimed from linear vibration theory that all large bulkships are dangerously weak in storms in regions near the bow and near the stem. Tremendous computations were applied to the structure of the *DERBYSHIRE* and these must have carried great authority. No weight was attached to the fact that the structure analysed and purporting to be that of the *DERBYSHIRE* was in fact a fiction, a single-skinned hull far less strong than the double-hull of the actual design (Corlett, Discussion of Bishop et al., 1991). The proof that the *DERBYSHIRE* did not break in two at the surface at frame 65 was the most important result of the underwater search.

Faulkner produced further analyses and applied them to other casualties as the present paper and its bibliography shows. By late 1997 officials at the DETR had become aware that their Assessor with the highest international standing, was pointing out that weak hatch covers must explain the disappearance of many large bulkers. This conclusion was unwelcome. The Professor was pressed to resign as Assessor. Shortly afterwards a debate on the DERBYSHIRE was held in the House of Lords. Lord Dixon read the following extract from a letter written by Miss Glenda Jackson, a junior minister at the DETR.

'Professor Faulkner's active promotion of the theory that the DERBYSHIRE sank when her hatch covers gave way is a matter of dismay for the Department. Officials have written to

Professor Faulkner to convey the Department's displeasure in his untimely interventions. In response he has offered his resignation as Assessor.' (Hansard, 3 Dec l997, p.1458)

It is never difficult to obtain experts to support an official view, though it is rare to find that the expert in question, Mr R A Williams, had previously, as joint author, supported the very analysis which was anathema. Inevitably, the recommendations of the summary report (Williams and Torchio 1998) cloud the principal question, the weakness of hatch covers approved for low-freeboard ships. The summary report includes a description,§40, 'The circumstances of the loss in more descriptive detail', a *postulated* sequence of events. This piece of fiction obscures the issues. Examination of the shattered remains on the seabed 2 ½ miles below the surface seldom allows the sequence of events at the surface to be determined. The bow must have been sufficiently damaged to fill with water before it reached implosion depth. But there is no way to prove that (§31) ' the bow mostly flooded prior to sinking'. It is false and silly to say (§§31 and 32) that flooding of the bow spaces (7000m^3) was the cause of the loss: the reserve of buoyancy of the DERBYSHIRE was 70,000m^3. With the bow flooded she should have remained afloat and sailed on when the sea calmed. She sank because her hatch covers were far too weak to stay watertight and so her holds filled.

Faulkner's analysis, which has caused such dismay to the DETR, is no more to be doubted than the accuracy (in macroscopic terrestrial situations) of Newtonian Mechanics. On which of course it is founded. Fortunately the Classification Societies have understood Professor Faulkner's reasoning, and bulkships contracted after I July 1998 must have greatly strengthened hatch covers (Lloyds Register 98, pp 22, 28). However, what is to be done to ensure the safety of older B-60s? It is said that new rules will require replacement of existing hatch covers by much stronger designs. Please would Professor Faulkner comment on this? Are the new rules adequate?

Congratulations, Professor Faulkner, on your analytical assessment of the loss of *DERBYSHIRE*! Congratulations on your application of the scientific method in the broadest way to solve the mystery of the B-60 losses, and to save many lives that would otherwise be lost at sea! And congratulations for refusing to be silenced.

Tom Allen (For DETR). I write on behalf of the Department of the Environment, Transport and the Regions to seek clarifications concerning certain key calculations included in this paper. I am confining our comments to points which could confuse consideration of the United Kingdom’s Bulk Carrier Safety initiative by- the international Maritime Organisation.

As you are aware thc United Kingdom is currently taking forward various initiatives at the international Maritime Organisation on bulk carrier safety. These initiatives are primarily based on the DERBYSHIRE Assessors' Report and the conclusions reached by the assessors. To progress the safety points the UK Maritime and Coastguard Agency are undertaking a research project on fore end design for bulk carriers and a Formal Safety Assessment project on bulk carrier safety in general.

As part of the IMO initiatives it was also important to take into account of this paper which Professor Faulkner considers to be complimentary to the UK and EC Assessors' formal report and that appears to cast some doubt on the Assessors' conclusions. We believe we have identified various key points which require further consideration, as they cast considerable doubt on the conclusions reached, plus some minor statements which require comment.

The first key point relates to the methodology for calculating the maximum wave height in any sample of waves during a storm and then the use of that information. Equation (7) shows the probable maximum wave height occurring in a sample set of waves for a Rayleigh type distribution wave spectrum. The Longuet-Higgins equation (8) as we know it is a means of calculating the variance of this single maximum wave height based on a number of separate similar samples. In Professor Faulkner's case all this applies to 1 wave in 3200 waves, a probability of 0.03%. The way the author has then extended the Longuet-Higgins equation to predict probabilities of large wave heights occurring within the spectrum itself and the resulting numbers of wetness events generated for the Derbyshire (432 for 800 waves) we believe is in error. All other equations/tables developed using these probabilities throughout his paper must therefore reflect this error and cast considerable doubt on the conclusions reached.

The second point is the authors definition of wetness events. No account seems to have been taken within the analysis of relative bow motion. He has assumed a level trim condition for the vessel at each wave encounter whereas in the Typhoon conditions bow motion must have been significant. He has also not taken into account any alterations in wave impact direction and the varying interaction with the bow form as it rose and fell. We believe this important point would be significant to the conclusions especially when taken together with the first point above.

Another key point arises in table 4, here the author appears to use four ventilator pipes of 500mm diameter for the fore peak ballast tank. However from the ship's drawings and the DERBYSHIRE Assessors' Report it is clearly shown that there were only three air pipes of 300 mm diameter. The 500 mm ventilators actually serve the bosun's store. The area used is therefore out by a factor of about 3.75. This obviously also puts the time to fill out by the same factor resulting in just over 21 minutes, instead of less than 6 minutes as quoted, to fill the ballast tank while the bow was sinking. His comparison with sinking time then shows a major variance This error is particularly featured in formulae 26, 27 and 28.

These three basic points, which are referenced throughout the paper, must cast considerable doubt on many of the conclusions reached. It would therefore be appreciated if Professor Faulkner could address these points.

There are other points that have caused confusion, e.g. the penultimate sentence of the section on bow flooding during sinking (Section 2.7.2), would appear to suggest that the fuel tank air pipes would actually ventilate these tanks. Air pipes are necessary to ensure a pressure balance and in no way would they be sufficient to guarantee venting the tank. Forced air ventilation of tanks' both fuel and ballast, is a standard practice for safety on all such ships. Likewise on Section 1.1 Bulkhead 339 was not the only single skin construction as bulkhead 65 was also single skin at it's centre.

Professor J.B Caldwell.(Fellow): With recent remarkable advances in underwater exploration and surveying techniques, there is likely to be a steady increase in attempts to deduce ship failure causes and event-chains from visual examination of sea-bed wreckage. This is expensive and time-consuming. Therefore the results of such efforts, their veracity, and their value in future ship safety assurance, must justify such investment of time and expense.

The *DERBYSHIRE* experience seems likely to leave room for some doubt as to whether the effort has been worthwhile. Different interpretations of the sea-bed survey data have led to differing opinions as to the event-chain preceding loss of the ship. It is possible that "political" (in the sense of non-technical) factors have obtruded. The result is that the elusive "truth" as to what happened, and hence the validity of any subsequent recommendations, may forever remain in doubt. (Perhaps this merely confirms the obvious: that in the absence of irrefutable proof, there can be no such thing as the *absolute* truth about a prior event; - only a set of truths or opinions, each influenced by subjective factors).

Of course, this is not to say that *such post mortem* researches should not continue to be considered. But what seems to be needed now are some experienced-based ground rules, by which accident investigators can decide firstly, whether to go to the expense and effort of sea-bed search and evaluation; and if so, what should be the guiding principles by which such forensic studies are to be conducted?

The Author's central role and independent experience in the DERBYSHIRE investigation make him uniquely well placed to formulate such rules and principles. He would be performing a most valuable service for posterity if he could now spare time and thought to derive from hindsight a concise set of "do's and dont's" for future use. These would greatly assist decisions regarding:

- whether to conduct a sea-bed search and evaluation;
- procedures for gathering data from wrecks;
- evaluation and interpretation of such data;
- reconciliation of differing technical opinions in a final report;
- how to deal with non-technical influences in such investigations.

If this request is tantamount to inviting a sequel from Professor Faulkner, the quality and value of the present paper ensures that it would be well worth waiting for!

B. M. Bell, (Member). Professor Faulkner has presented a paper on an important subject and one which will be of intense interest to naval architects and others who are concerned with merchant vessels. He has done much useful work in connection with the loss of the "Derbyshire" and together with others, been instrumental in drawing attention to the inadequate strength of hatch covers generally provided on large dry cargo vessels.

However, with regard to the suggested plastic collapse approach to the design of hatch covers, it is questioned whether such a method is appropriate in the case of closing appliances, which are required to remain watertight when subjected to the full design loading. Once permanent distortion has taken place, watertight integrity is almost certain to be lost and in severe storm conditions, could result in rapid ingress of water to the hold space sufficient to imperil the ship. So to base strength determination on the yield stress, using an appropriate load factor, would be a more logical approach. Thus the designer is able to readily control the point at which initial yielding will begin .

Clearly the strength requirements set out in the 1966 International Load Line Convention are inadequate for the hatch covers at the forward end of large ships, a matter that has already been addressed by the major classification societies in respect of bulk carriers. The revised IACS. requirements for bulk carriers would result in the hatch covers of a vessel with "Derbyshire's" characteristics, if built today, having hatch covers 1.6 times stronger than is required by the 1966 Load Line Convention. The Rule uses a maximum allowed stress of 0.8 yield and includes a valuable limitation to compression buckling of the upper flange, i.e. the plating.

It is not a simple matter to compare the proposals that the author makes for stronger hatch covers with the 1966 ICLL requirements, nor the classification society Rules, because of the different methods used. Hydrostatic head is not the only variable.

Using the resultant section modulus as the parameter for comparison, it is seen that the author's proposed increased design head of 9.0 metres for No. 1 Hatch results in hatch cover strength being increased to 2.5 times that for 1966 ICLL; less than might be expected for such a large increase in design head.

In the light of the magnitude of possible sea loadings revealed in the paper and the conclusions reached by other researchers, the question as to the necessary degree of strength improvement for hatch covers deserves farther discussion within this profession. The situation as regards other ship types should also be reviewed, in particular, large container ships, some of which no longer have hatch covers on all hatchways.

Besides the earlier work done by the author with Corlett and Romelling, it is noted that a paper prepared by Byrne and Evans for the RINA. International Conference on the Design and Operation of Bulk Carriers, 1998, suggests an increase in the strength of the forward hatch covers by a factor of 3. If this is related to the design approach laid down in the 1966 Load Line Convention, the hydrostatic design head would be 5.25 metres.

The author recommends that existing balk carriers should have their hatch covers replaced to meet a new standard of strength. Whilst this is quite feasible for ships with side rolling or pontoon hatch covers, in the case of ships fitted with single-pull hatch covers, the position is much more difficult and would quite likely require a reduction in the length of the hatchways concerned.

A further question arising from this paper which needs to be considered by the rule makers is whether the stated 1% probability of hatch cover collapse during 12 hours exposure to the dangerous semi-circle of a severe typhoon is over onerous a condition to meet. It is argued by some that the *"Derbyshire*" should not have been at the position she was in on the 9th September 1980 but we shall never have a clear answer to why she was at that particular location. However, if ships are to be designed so as to survive the worst of abnormal storm conditions, a fresh approach may be needed beyond the design of hatch covers. On the other hand it may not be necessary to set our sights so high if, statistically, the losses as a result of such conditions are very low.

Regarding the author's proposed improved design of hatch covers, the extra strengthening that would be required at the inboard edge of the side-rolling hatch cover panels appears to have been overlooked. The girder necessary to support the loads from the ends of the proposed transverse webs would have to be very substantial, approximately 5.8 times the strength of the longitudinal webs required by the 1966 ICLL in the case of *"Derbyshire*". Such a girder would certainly reverse the weight saving claimed for the "improved" design.

This contributor would caution against too great a reliance on calculated values of the forces, rates of flooding, motions etc. that a ship may experience in the conditions released by nature's fury in the turmoil of the worst sector of a Pacific Ocean typhoon. Whilst the author will probably agree with this proviso, he does not give any emphasis to such limitation. The Risk Assessment method of appraisal is subject to the same remarks, though in the absence of a better process, it has to be accepted as a useful tool for ranking the various risks.

The author emphasises the need for "advanced analytical thinking" on the part of the Assessors. Few will disagree with this statement and it seems apparent that such qualities were available to the Assessors in good measure by the author himself. Other qualities are also desirable in those undertaking the examination of a wreck of a ship such as the - "Derbyshire" It is essential, as with any team, that a proper balance of skills and experience of those comprising the group is achieved. It is to be hoped that those who selected the Assessors were sufficiently aware of this consideration when they reviewed candidates for the positions. It should go without saying that the appointed Assessors should be independent of the sponsors' interests.

In undertaking the task of examining the vast collection of complex data obtained from the wreck of the "Derbyshire", there will inevitably be scope for some disagreement among the Assessors, however, the disparaging context of the author's remarks on the views and findings of his former colleagues seems misplaced in the proceedings of this Institution.

Mr W. T. Cairns, (Fellow). The design of this class of O.B.O's was commenced in 1968 and construction of the first (Furness Bridge) started in 1969. The prime concern of all Seabridge owners was the danger of explosion "sparked" either by oil tank washing or sloshing in a partly ballasted dirty oil tank. A Bibby vessel suffered serious damage and loss of life about this time, Hilmar Reksten who controlled Thornhope (not members of Seabridge) had an oil tanker explosion. The strength of hatch covers was never questioned (many ships at that time general cargo - were sailing with wooden covers). We did discuss the fitting of a forward breakwater but it was felt that the large hatch coamings would serve the same purpose. but given the waves that swept Derbyshire it is arguable whether a breakwater would have made an appreciable difference; in fact a breakwater could have the effect of increasing the water head on the forward hatches.

I have the utmost respect for seafarers; they rely on naval architects to design and build safe vessels; Similarly we trust the ships will be safely operated.

In 1965 my company insisted I take a North Atlantic voyage on a newly delivered 400ft cargo ship. The deputy chairman said "You designed it. Go and see how it works". We sailed from Liverpool partly loaded because of a strike, the GM was over 6ft; soon after rounding Ireland we met a force 9 storm, and I estimated one wave some 50ft to 60ft high For over two days we operated at very low revs sufficient to keep the ship heading into the sea. We rode the sea like a "bucking bronco" ~ there was no damage thanks to a very experienced professional master. We later heard from our eastward bound sister ship that they had gone to the aid of a sinking Norwegian vessel regrettably they were unable to save any of the crew .

My next experience (in 1966) was on an 85ft tug, on leaving the harbour the vessel was hit by a wall of water which broke over the wheel house top. The tug was swamped and the hull was under water for what seemed an eternity. Eventually I was able to go below - The saloon house and the three cabins below were flooded with 3-4 ins of water and a generator in the engine room was put out of action. The cause was an open sidelight in the saloon and an open E.R. skylight. An "experienced" skipper, knowing a storm was forecast, had put to sea with an open vessel.

In 1974 "Furness Bridge" entered Port Darwin to register her place in the loading queue. The master planned to depart immediately thereafter for he had been tracking the approaching typhoon and he reckoned the storm would hit Darwin during the night. The harbourmaster tried to persuade him to stay in port. He ignored this advice, went to sea and put as much distance as he could between him and the path of the storm. He returned later to find devastation. There is no doubt that his professionalism saved his ship. He also informed me that in all his time on two of the OBO's (Furness B and Sir John Hunter) he never experienced flooding of the peak spaces - he knew from many years in ore carriers and oil tankers that it was pointless and damaging to drive a full block vessel through a heavy sea. I also asked him about the apparent practice of removing an oil tank manhole cover before arrival. His answer - "I would say no possibility". Did this suggestion emanate from the identification group?

Another ex-master friend and colleague when chief officer of a "VICTORY" ship experienced horrendous seas in a North Atlantic hurricane off Newfoundland in the early 50's. One wave swept across the boat deck lifting a lifeboat out of its chocks despite extra lashings, the seas had also damaged ladders, derrick crutches and rails. At night the vessel was hove-to in the "eye" (no wind but very confused sea). He said the danger arose when the "eye" passed at which time the wind direction changed 180 degrees, if the crew are unprepared the vessel could be pooped and overwhelmed. In his words they survived because they had an experienced master. When they cleared storm the vessel was listing and reacting very sluggishly. They sounded tanks and discovered the port forward deep tank partially flooded due to a missing sounding pipe cap. Such experiences are never forgotten and as a master he had two "brushes" with Pacific typhoons. However he was able to avoid the worst of the weather.

In December 1987, just before I retired, I attended the Derbyshire formal enquiry and was present when Brian Corlett (Burness Corlett & Partners) gave evidence on the results of their investigations into the loss. This included hindcasting the likely weather the vessel experienced. Brian Corlett suggested that although the ship reported 30ft waves the reality was probably 30 metres. Tank tests were carried out in Denmark and the film shown to the court was, in my view, frightening and I do not exaggerate - solid water rolling high over the hatch covers and smashing into the bridge front. (If the vessel had been down by the head the forces would have been greater)

Brian Corlett concluded that the hatch covers failed - he also discounted a structural failure at BHD 65. I commend Brian Corlett and his associates for their work which in my view was carried out in a highly logical and professional manner. I left the court convinced. It is deplorable that it is only in the last few years that hatch covers are being strengthened and now we have the author's paper effectively confirming the 1986/7 work.

May I now be a little contentious and remind the author that in the early 90's he supported the view that Derbyshire was lost because of a massive structural failure at bhd 65 and that this was more probable than hatch cover failure (discussion 1990/1 Bishop et al). This, even though there was no evidence that the Bibby ships had carried out repairs at bhd65 similar to three of the sisters. It is regrettable that many others were also convinced and this resulted in a great amount of time and effort in chasing a red herring. May I also suggest that if the poorly executed repairs on the Thornhope ships had not been discovered this location would probably not be given consideration in a pre-completion F.S.A. Suffice to say that three ships of the class had a life between 19 plus and 23 plus years and one (Tyne Bridge) was scrapped after 15 years (probably because of poor trading prospects). These facts must mean something!

I do not have any arguments with the major portion of this well written paper, I also agree that hatch covers must be strengthened, but I also consider that mariners should be reminded of the actions to be taken if they find themselves near the dangerous sector of a typhoon/hurricane. However I do agree with Williams and Torchio that the loss was probably caused by a combination of events.

Firstly, the stores hatch-was it secured? My own view is that it was probably not fully cleated and thus the peak space could have taken in water possibly over a period in excess of 24 hours when the vessel was likely to have been in heavy weather on the 8th of September. Unfortunately ships have been known to sail with hatches (or doors) open or not fully cleated and at times hatches are opened to improve ventilation. The damage to the store hatch coaming could have been caused by the falling mast. I consider it is stretching credulity to imply that a large windlass concentrated its mass only on the soft plating. We do not know when this damage occurred but it may well have happened after the lid was lost and when the vessel was in its final throes. If this hatch was not properly secured then this becomes a contributing factor.

Notwithstanding this, all we can say is that fore-end flooding would increase the forward draught somewhere between 1 and 2.5 metres. However as some heavy duty cross joint cleats on 5 cargo hatches were not engaged, it is quite possible that water gained access to the forward hold(s). Hold vents were also missing. Thus it would seem that the ship could have been down by the head to such an extent that steering was adversely affected. The vessel would then have been slow to react when in a hove-to situation to the changing direction of the weather. The aft end damage may support this supposition.

In my view the probable head trim was a contributing factor.

During the May 1998 colloquium I said that an additional one or two metres of freeboard would not be material in extreme typhoon conditions; (incidentally I believe a "B" class vessel would also not have survived). I got the response that further model experiments would test my opinion. I hope that such tests (with the addition of a trimmed model) have been carried out.

I find it difficult to agree with the author when he says that the <u>direct</u> cause of the loss was inadequate hatch covers. Surely the prime cause was that the vessel appears to have been directed into the most dangerous sector of a typhoon and we cannot be certain of survival even with stronger covers.

Blame should not be laid on weather routeing per se. It should be used to divert the vessel clear of bad weather and even though additional distance may result overall time would be saved. In the case of Derbyshire (and possibly other ships?) it appears that this was not the ease! Notwithstanding this I find it difficult to accept that a ship master would give priority to economics over matters of safety.

So what of the future? Bulk carriers, when carrying heavy density cargoes must operate at a much reduced draught such that they will not founder if a hold is flooded. Logic suggests that heavy density cargoes should only be carried in specially designed vessels ~ they are called ORE carriers! After all Ro-Ro ferries are being designed to counter the danger of a door failure. It follows we should design the hold spaces to take account of possible hatch failures.

During the 1998 RINA bulk carrier conference a delegate stated that ships should be able to survive all weather conditions. In the current politically correct climate this might sound reasonable, however as the worst possible condition cannot be defined I question whether this is practical. Any attempt at a precise definition will surely be confounded by the forces of nature and confirm that- Solomon's Proverb 30 -18l19 concerning the way of a ship on the high seas is Still as appropriate today as it was a few thousand years ago.

This was an avoidable disaster and hopefully the forthcoming enquiry will produce the final pronouncement.

Dr J C Chapman, FREng. Professor Faulkner has performed a great service in his unremitting efforts to establish possible causes for the loss of *Derbyshire*. But the significance of his work will be more far-reaching. I wonder whether he might have underestimated the possible effects of cargo liquefaction. Whether or not the water content on loading was above the specified limit, the unpelletised cargo must have been wet, and expert opinion is that liquefaction might have occurred. Recognising the double hull, is it possible nevertheless that listing following Iiquefaction could have made a disastrous event more likely?

Takashi Jono (consultant) and Minoro Fujita (Fellow). The *Derbyshire* case gave the lie to the conventional way of thinking toward the safety of ships; such a gigantic ship as m.v *Derbyshire* is unsinkable even in extreme seas. The author pointed out an abnormally steep and high wave exists in certain sea conditions and analytically showed such a single abnormal wave can be a cause of her loss. We are the author's opinion and thank him for his valuable report and pay our respects to his indomitable effort.

The authors of the UK/EC Assessors' report, concluded that the decrease of the forward draft due to the flooding fore peak spaces through openings such as access hatch and ventilators incurred the total loss of the ship. We don't agree with this conclusion because there is no verification that the collapse of cargo hatch covers could not occur if fore peak spaces had been intact and forward freeboard maintained. Decrease of forward draft might quicken the foundering of the ship, but can not be a sole reason of this tragedy. Their conclusion seems to be speculative.

In this regards, besides the author's recommendations, we would like to make two additional recommendations:

1. Seaworthiness model test

It is impossible to reproduce an exact ship and wave motion at the moment of the loss because no one knows the real sea state then. Nevertheless, comprehensive seaworthiness model tests will surely give us much knowledge to help understand what happened to m.v. *Derbyshire*.

Through our careers as naval architects we had advice from one of our clients, who is a naval architect also, that a vessel with small bow flare is apt to plunge the bow deep into sea with small shock felt by crew members. The crew generally praise such a ship as being a good wave piercer. But they find damage on deck plates and deck fittings due to green sea afterwards. In contrast, in a ship having sufficient bow flare, deck damages are comparatively rare, because the bow breaks water and protects the deck from green water panting.

Above all, heavy impact to bow flare and shuddering all over the hull makes a trained seaman cautious in operating a ship. As a conclusion, he asked us to make the bow flare reasonably large with necessary reinforcement to side shell construction in way of the bow.

Noting the block coefficient (Cb) of m.v. *Derbyshire* is as large as 0.84, we remembered above story. A ship large Cb tends to have a vertical or cylindrical bow form with small flare. Also cargo loading distribution at the time of the accident is rather particular; cargo was loaded in 7 out of 9 holds reducing its weight in way of a midships and concentrating weight fore and aft of cargo spaces. It means the ship had a large longitudinal moment of inertia compared to that of an ordinary alternate hold loading pattern. This would have a significant effect on ship motion.

Therefore we strongly recommend comprehensive seaworthiness model tests consisting of at least 3 models, let's say Cb=0.84, 0.82, 0.80 with varying longitudinal radius of gyration. Each model should have the same deck side line and bulbous bow. All the effect of change in Cb should appear in the degree of flare.

We believe such tests will provide thorough information to help understand the phenomenon and can be basic data on which a necessary safety regulation should be developed.

2. Operational avoidance criteria in heavy sea.
It is our understanding that ILLC requirements for the minimum freeboard of a ship is empirical and it does not guarantee ship safety wherever a ship may navigate. It expects a ship master will take appropriate measures to avert a heavy sea adjusting ship speed or changing its course if he judges the sea condition is risky to the ship.

So are the rules of classification society adequate? It is said the strength requirement of a classification society is based on wave conditions in the North Atlantic as being representative for the most severe conditions world-wide. But it does not mean that a ship complying with a rule requirement and properly maintained can safely navigate everywhere without worrying about sea conditions as far as the strength of the ship is concerned. Appropriate avoidance acting is a prerequisite. For example, a ship is generally designed for the heaviest sea conditions it is likely to meet in 20 years, while an offshore structure which is stationed in one place and can not avoid waves is generally designed to that of 50-100 years. Design conditions for an offshore structure are much more severe than for a ship with nearly the same life span.

But what is the standard criteria to judge whether a ship is risky or not, especially in the matter of structural failure. The *Titanic* case gave rise to discussions on "Life saving" and "Subdivision". We got a new regulation then. Similarly, the *Derbyshire* case gave rise to a structural problem directly related to total loss of a ship. Frame 65 problem may not be a direct cause of the loss but cargo hatch cover strength against green water impact is most probably a key.

One approach may be to substantially increase the requirements for the strength of hatch cover structure. But at the same time, we believe the establishment of a clear operational criteria to avoid such structural failure in heavy sea is essential.

AUTHOR'S REPLY

General Remarks

This final printing of the paper includes some minor editorial improvements to the text. I write using the first person because this is more direct and is generally now accepted.

To have discussions from 20 people from five European Countries, USA and Japan is gratifying enough, but to find only one that objects in any significant way to my conclusions and recommendations is more than any author could wish for. Thank you all for your overly flattering discussions which have added considerable value to the paper. The discussors may be pleased to know that the paper, their discussion and my reply were made available to the final Re-opened Formal Investigation (RFI).

Because their recommendations are potentially so important for future ship safety, I have highlighted for special consideration the discussions of Buckley, Turner and Spyrou. They have each given rise to what I call a need for a *paradigm change in thinking* by naval architects and others concerned with ship safety, with three recommendations under the following headings:

- First Principles Survival Design (Buckley)
- Breaking Wave Impact Design (Turner)
- International Ship Evaluation Panel (Spyrou) and related matters

I believe each is very much needed. The first two are mentioned in the paper, but the third one comes only from the discussion. I commend these three discussions and responses to all those concerned with ship safety.

In considering how to respond to the discussions there were some common themes and questions. But I have preferred to deal with these generally where they first appear as a major issue. I have therefore used cross references to my reply to that particular discussor. My replies are therefore discussor by discussor, generally in the order in which I received them.

Several of the discussors, like myself, have called a spade a spade and have tilted at the UK/EC Assessors' report and at officialdom. To some this may seem offside, but I believe this is an issue on which innermost feelings should not be suppressed. The human aspects of engineering are often more difficult than the technical problems. Moreover, it is surely the essence of good professional debate on such an important issue and is regrettably becoming rarer in these busier "don't rock the boat days". It must be evident that this particular boat does need to be rocked if the most probable truth is to emerge. Too many UK formal investigations have been overly influenced and distorted by vested interests, the government's included. The new 1995 Merchant Shopping Regulations reduces but does not eliminate it.

I thank the SNAME for making this paper possible and the RINA for this updated version and for agreeing that its debate is made available to the final RFI.

Revised Bow Flooding Calculations (C7)

Since the original printing of the paper the Attorney General's office provided the author with various data which he was prevented from obtaining or checking following the Phase 2 survey. Most notably these are, the corrected number and sizes of mushroom vents and air pipes. The stores' hatch has been omitted in Table 4 as it was properly secured. The Engineers' store has been omitted because it is located in the aft of the ship. Its longitudinal co-ordinate is incorrect in the Capacity Plan. The fore peak flooding data and calculations in Tables 4, 5 and 6 have now been revised. The opportunity has been taken to approximately include the significant effects of the build up of air pressure in the fore peak tanks and stores spaces which increasingly slows down flow rates into these tanks. Broadly, the effects of these changes show:

- the flooding of the FP spaces through all the damaged openings (scenario C7) would have been slower than those initially calculated
- the UK/EC Assessors' bow flooding scenario, which also requires flooding of the deep fuel oil tank through these same orifices and an assumed open hatch cover before the ship starts to sink would, according to these revised calculations, take many days at the same peak intensity of the storm before the draft reduction at the bow reached 2.5 m (2.1 m at no. 1 hatch cover)
- the absence of any significant implosion-explosion damage to the bow can now be more fully and properly explained by including the effects of the long split in collision bulkhead 339 during the sinking process (as suggested in 2.7).

These factors have required Table 5 and the associated text to be revised. These relate to flooding through broken weather deck air pipes and MVs into the bow ballast tank and the whole of the Bosun's Stores Flat whose spaces are not watertight. Further downward flooding through open manholes to the deep fuel oil tank and ballast tank, as assumed by the UK/EC Assessors was not considered to be realistic.

This reinforces the previous conclusion that with H_s = 14 m any calculated flooding of the bow spaces was minor and incidental to the loss. Refining these calculations is not therefore justified. (See also Dr. K.R. Drake's supportive contribution and my reply).

Since these opening remarks were completed, the Formal Investigation has reopened and completed. A Postscript has therefore been added to this reply to complete the story. This includes some experimental data.

Ernst Vossnack's timeless pursuit to revise the freeboard regulations and to ban gross tonnage is, I believe, slowly penetrating the maritime conscience. He speaks from a wealth of design and ship operating experience and is in my opinion the modern Mr. Plimsoll. His full support for my assessment is therefore particularly welcome.

Paul Lambert and **Captain David Ramwell**. Paul is Chairman of the DFB and David an advisor who is dedicated to their interests. He wrote a book on the subject with Tim Madge (1992). Their merged and very welcome contributions spend much time on workmanship issues related to frame 65 (loss scenario C1). They also raise previous issues regarding past history, Government reports, decisions, and the 1987-89 Formal Investigation (FI), many of which are not appropriate in the context of this paper. I therefore only respond to the specific questions or relevant comments that are made, as follows:

(1) The cruciform connections in VLCCs were approved connections as they were for the last five Bridge class ships. My paper does not imply that this design was proven by service experience. Although the design is sometimes unavoidable, it is unsatisfactory in principle because the longitudinal stresses have to be transmitted through-the thickness of a mild steel plate whose mechanical properties in this direction are notably weaker than those in the plane of the plate. It is also an unsatisfactory design in practice because of the difficulty of assuring a good alignment of the members each side of this plate, which then gives rise to fatigue cracking, as experienced in all the Bridge class and many other ships. There are several ways to improve the design, as have already been suggested by Meek, Williams and Faulkner.

It must be understood that fatigue cracking is an inevitable event in the life of aircraft and ships. It is only life threatening in ships if brittle fracture conditions exist (as in the *KURDISTAN* and *FLARE*), or if these cracks are allowed to grow extensively in primary structure without repair. It is also a fortunate fact of life that bad workmanship seldom sinks ships. However, it must be minimised.

(2) My 1987 statement that I considered massive cracking at frame 65 (scenario C1) to be the most likely cause of the loss is correct. I deal in notional probabilities and at that time I had no reason to expect hatch covers to be so substantially weaker than the very strong deck structure which they were penetrating. It was my work 8 years later with Lord Donaldson which exposed this weakness and gave rise to the hatch cover collapse scenario (C4) having a higher risk numeral than for C1. This was nevertheless still retained as the second most likely of the 13 loss scenarios then considered. It is inevitable in the scientific assessment of safety that previous priorities should change in the light of new knowledge, data or evidence.

(3) I am asked if I still consider it possible that the ship failed at frame 65. It is abundantly clear in the first section of 6.1 of the Conclusion that this is not possible. Section 5.3 rules out the structural loss scenarios C1, C2 and C3 (based absolutely on *Lemmas 1* and *3* in 5.2 and circumstantially on other evidence).

(4) In saying that the previous FI was biased towards the frame 65 loss scenario I was merely reiterating a statement passed on by one of the Wreck Commissioners' Assessors that "the subject had occupied about 40% of the proceedings". I also believe that the 1997 underwater survey spent unnecessary time on scenario C1 to the detriment of other scenarios like C4, C13 and C14.

(5) It seems the families were outraged by the dictatorial stance of the FI Wreck Commissioner rather than by the lack of a fair conclusion regarding frame 65. This apparently restricted any input from the DFA into the proceedings. It is also implied that the FI was not conducted fairly and properly. From my own experience, and from recent discussions with others who have been involved in formal court room proceedings, I know this to be true and therefore I have considerable sympathies with these remarks.

Court room proceedings are often handicapped in two ways. First, by the limited scope of the questions which are decided by the Wreck Commissioner and by lawyers representing the interested parties. Also, the *adversarial* nature of Court proceedings is not helpful (Thomas, 1977). An investigation would be more likely to arrive at the most probable cause of the loss in an *inquisitorial* climate (Reid, 1997) not held in a law court. Such a procedure would also be quicker and much cheaper and it does appear to be adopted in other countries. Vested interests can then be pursued legally afterwards as necessary. However, it does seem that the provisions of the recent Merchant Shipping Regulations of 1995 should now exclude the apportioning of blame and reduce the influence vested interests, (Faulkner and Reid, 2001).

(6) It is naive to imply that, because the six Bridge class ships suffered cracking at frame 65 but none of the other five suffered hatch cover collapse in bad weather, the former should have a higher risk rating than the latter. As section 1.5 of the paper makes clear, the risk matrix is based on many factors and not just on the very limited experience of the six Bridge class ships. For example, we know with reasonable certainty that a significant number of bulk carriers (about one a year) had hatch cover or coaming collapse (probably in two cases in recent years) as the main route for water ingress (Faulkner, Corlett, Romeling, 1996). The most recent confirmation appears to be the loss of the *LEROS STRENGTH* (Faulkner, 1997b, and Aftenposten, 1997). Moreover, experimental data from the 1989 FI and recent theory have highlighted the vulnerability of hatch covers. In stark contrast, no ship is known to me which has been lost without trace because the stern broke off.

(7) In reiterating my dedication of the paper to the DFA I naturally share the expressed hope that my independent more analytical assessment will help to prevent the debate ending with the conclusion of the UK/EC Assessors' Report.

Douglas Brown (Member) seeks my views on the different approaches used by the UK/EC Assessors and by myself to arrive at two conclusions which nevertheless agree regarding the frame 65 and the hatch cover scenarios.

First, I must stress that Williams and Torchio attempted to arrive at their conclusions almost entirely from the physical wreckage evidence on the seabed. I believe that to be quite impossible for various reasons, as given in the paper, but mainly because of the destruction arising from implosion-explosion actions. My approach was therefore based mainly on logic and analytical work, supported where possible by circumstantial and direct evidence in some cases, and by external factors like, previous experience, other casualty data, etc. Astonishingly, the other Assessors felt no need for this.

Frame 65 (C1) exclusion is mainly based on logic from *Lemma 1*. This was readily agreed by Williams but not initially by Torchio. It became evident he was probably appointed to "safeguard" this scenario. Nevertheless, he signed the official report with Williams so it must be assumed that we all agreed for essentially the same reason, logic. I also offered other reasons in 5.3.

There is no convincing evidence or reasoning offered by Williams and Torchio for deciding that hatch covers are weak (C4). Of the many assertions in their report, those given for the hatch covers are the most unconvincing, dubious and contradictory, as shown by my analysis of this scenario in section 5.3. I believe they contrived to come up with support for this scenario because of the arguments and evidence in the Annex to Lord Donaldson's Assessment, and because they did have the good sense to realise a burst hatch cover would be terminal for the ship. But, there is no seabed evidence to show the hatch covers are too weak. Moreover, it is quite impossible to say by how much they are too weak without any analysis!

My approach ignored the seabed evidence and was based on green water modelling, evidence from other ship casualties, evidence from DMI test data and from analyses presented at the previous FI. Further support now comes from Dr. Drake's discussion and my reply.

As regards professional skills and personal qualities, which are necessary for important investigations, I suggest the team should be made up of people who:

- have a high level of professional and intellectual abilities and so recognised by their professional Council
- are competent analysts who are also capable of logical and lateral thinking
- know the class of ship well and have full access to class experience, other casualty data and relevant analyses
- preferably have some previous direct experience in investigating ship casualties
- preferably understand the principles of Formal Safety Assessment and the Risk Matrix
- can distinguish between initiating and terminal events
- are adaptable and willing to suggest changes to the survey plan in the light of present findings
- are intellectually honest and independent, able to stand up to external pressures from sponsors and interested parties during the survey and when analysing the results for the final report; however, the team should be willing to seek advice as necessary from any qualified parties (which they may reject)
- have some familiarity with high-tech underwater equipment and its capability
- are physically able to take their full share of watch-keeping and other duties over an extended period in rough weather.

I also add a personal preference for the contractor to be willing to adapt to any changes or extension in the survey plan within reason. A high level research organisation like WHOI is ideal as they are freer from true commercial pressures than most.

Dr. Kevin R. Drake is a civil engineer with offshore experience. His contribution is particularly welcome as it is the only one that offers results, albeit preliminary, from an alternative analytical approach for estimating probabilities for the average water head over no. 1 hatch cover. This is important because my prime conclusion rests mainly, but not entirely, on an analytical approach to this.

Dr. Drake's hatch cover notional short term failure probabilities (based correctly on 13 m relative motion) are 0.45 to 0.57 arising from the passing of 1000 peaks. They compare very favourably with my own values of 0.81 to 0.90 in Table 6, bearing in mind the quite different assumptions made. His analysis is for a uniform rectangular ship responding to short term long crested waves having second order corrections to their extreme profiles. It incorporates the inherent uncertainty in the assumed wave spectra, but assumes no uncertainty about the expected value for any particular wave period. That is to say, Dr. Drake's probabilities are conditional only on the chosen H_s and T_p and do not take account of the longer term real sea uncertainties. These have to be judged but are substantial and would normally increase his notional probability of failure values quite significantly.

However, I must apologise for going a little beyond the data in Dr. Drake's contribution, as I have subsequently been privileged to see the draft of his valuable paper (Drake, 1999a). There one would also find encouraging support for my chosen wave asymmetry (as in Drake, 1997) and for my assumption based on the DMI experiments that the vessel is close to level trim when the extreme wave crest is abreast no. 1 hatch, that is, the ship has begun to recover from its downward pitch into the trough of the wave. It seems that my quasi-static approach to wave force balancing is also supported. This is all very encouraging.

Since writing his discussion I have received a further contribution from Dr. Drake (1999b). This suggests, and I quote, that whilst he agrees with my use of the extreme value probability distribution for predicting hatch cover collapse, he believes a Rayleigh or similar short term distribution would be more appropriate for the fore end flooding predictions. The number of waves which could cause flooding in each wave height interval which I considered (in section 2.5) would then be much lower.

In principle Dr. Drake is of course quite correct and I have been a little surprised no one spotted my questionable assumption earlier. However, I had qualitative reasons for using the *first passage* probability density function of eq(10) rather than the more correct *out crossing* of a threshold method, both of which are referred to in 2.5 (under "Philosophy").

First, any assumptions made for predicting either the first passage of a single wave to collapse no. 1 hatch cover (C4), or the many outcrossings of a threshold for predicting flooding through orifices (C7), would be subject to very large uncertainties for RTS storms such as ORCHID. Having taken expert advice on the occurrence of steep elevated extreme waves I made the judgement of 75%:25% for the mix of notional probabilities first mentioned in the notes under Table 3. Subsequently, in using this for the fore end flooding scenario of Tables 4 and 5, I felt this might be biasing the probabilities toward C4 hatch cover collapse to the detriment of the UK/EC Assessors' preference for scenario C7 fore end flooding. The DETR had already (unjustly) accused me of such bias. Using eq(10) would more than compensate for any such bias. Moreover, many readers would have little knowledge of the statistics of waves and extremes, and I did not wish to introduce another distribution based on different statistics which might have confused them. I did point out that whilst my assumptions could be criticised, the same probability distribution was being used for both.

With hindsight I wish I had not bent over backwards to appear to be fair to the UK/EC Assessors. I can confirm what Dr. Drake states, that the number of waves which can cause flooding would be very much reduced. For example, my own estimates over wave heights 20 m to 26 m using an appropriate Rayleigh distribution over a period of D = 3 hours during Typhoon ORCHID (H_s = 14 m) indicates the amount of flooding would be reduced in Tables 4 and 5 by a factor of about 0.04. Even allowing for sea state uncertainties this might become 0.1 which is very much less than I had anticipated. The values of flooding and changes of trim in Tables 4 and 5 would become negligible and would invalidate the UK/EC Assessors' conclusion that bow flooding caused the loss of the *DERBYSHIRE*. But, quality tests are required

William H. Buckley is the pioneer of the much needed First Principles Methodology for the design of ships to withstand critical storm conditions. I am therefore pleased that he finds my paper to be a good illustration of the method applied to casualty analysis and a landmark step to establishing guiding principles and practices for forensic analyses of shipwrecks.

Mr. Buckley cites the severe damage to the eight year old 127,000 ton OBO carrier *CHU FUJINO* on December 28, 1979 which was struck by a single abnormal wave estimated to be about 100 ft high approaching at about 70° off the starboard bow causing very severe damage and flooding over the length of the ship. His analysis of this casualty shows that my definition for abnormal waves gives 90 ft height, slightly less than the 100 ft estimated and that several of my recommendations are supported by the evidence he presents. In particular, Buckley wishes to expand my recommendation to consider wave impact loads in beam seas. This of course has implications for all hatch covers and coamings, and this is illustrated by the fact that the aftermost cargo hatch in front of the bridge of *CHU FUJINO* lost its watertight integrity. The starboard bridge windows were also smashed. Had the ship been laden in dense ore she might not have survived.

Mr. Buckley goes on to draw more supportive evidence for the survivability envelope (Fig. 4) from the Canadian bulker m.v. *SELKIRK SETTLER* which was severely impacted by the waves. In particular, he points out these loads were certainly not static as is assumed in current hatch cover design. Again, beam on loading is stressed, including the enhanced impact on many of the deck hatches. The presence of the hull caused the wave to rise up and then drop more like a *plunging breaker* (see his photo Fig. 1). Another of Buckley's important observations is that the height of water impact on ship structure, like at bridge windows, is likely to exceed that of the undisturbed wave.

Mr. Buckley also stresses the urgent need to strengthen the hatch covers in existing ships, which is a plea made by several discussors. I refer him and others to the last paragraph of my reply to R.V. Turner. His exceedingly valuable discussion not only enhances my paper, but leaves much for naval architects to consider. I commend his earlier work to them as I seriously believe a *paradigm change* in, or addition to, our design thinking is long overdue. As a profession there can now be little doubt that we have not given enough attention to our prime professional concern with ship safety.

R.V. Turner (Fellow) draws on his experience and observations to make some very pertinent remarks which enhance the paper.

In relation to bow flooding, several people have expressed doubt about the UK/EC Assessors' conclusion that the initiating event was flooding through the stores hatch. Their assertion that it was not properly secured has been challenged. But whether or not this was the case, Mr. Turner believes Appendix 2 invalidates the Assessors' prime conclusion. I point out that the revised calculations with smaller orifices, etc. are even more convincing in this respect, and my reply to Dr. Drake reinforces this.

Regarding water impact in Appendix 1 I apologise that my text below the Table is too brief and unclear. Pressures arising from plunging breakers certainly do in principle require a dynamic analysis, which is what I recommended in section 2.4. For local water impacts this can in principle be provided for by the experimentally derived pressure coefficient C_p in eq(14). But the pressures do vary rapidly in time and space, and so time-averaged measurements have been taken only over limited areas. As far as I can determine these may be taken as:

C_p = 9 over say a plate element area of about 1 m^2

C_p = 3 over say a stiffened panel area of about 2.5 m to 3 m squared, that is, 6 m^2 to 9 m^2 say.

I apologise for not including this at page 10. However, hatch cover areas are much larger (156 m^2 in *DERBYSHIRE*) and there is as yet no pressure data available other than from the 1/50 scale DMI model tests using scaled long-crested waves no more than 26 m high, and dynamic wave plunging effects were not particularly significant. Using C_p = 1 correlated surprisingly well with my eq(13). Furthermore, as a matter of judgement, this seemed to me to be a sensible value over such a large area. Therefore C_p = 1 is what is implicit in my section 2.4 for hatch cover collapse. For collapse of the 2.2 m high coamings, the higher values of 3 and 9 are however relevant because of their small height.

Moreover, what I was also trying to show from the table in Appendix 1 is that whereas the horizontal wave celerity is appreciably enhanced for the spilling breaker or near-breaking waves, the vertical component of velocity is entirely due to gravity. The impact pressure on a flat horizontal surface is then 0.5 C_p ρ u^2 which is C_p ρ gh, that is, a pressure head of $\underline{C_p h}$ where C_p is appropriate for the area size being considered.

Regarding the very high pressures measured on sea-walls from plunging breakers Mr. Turner is absolutely correct, and the influence of trapped air is very significant (see Bagnold, 1939 and Rapp, 1986). The 2,700 ton breakwater at Wick in Scotland was pushed back en masse into the harbour by steep elevated waves by an average pressure estimated to be 304 kN/m^2. However, most of these near shore incidents are generally made much worse by substantial shallow water steepening of the wave fronts and by the long-crested breaking waves that are usually generated. Shoaling water certainly does affect coastal shipping but less so for deep water shipping. Mr. Turner referred to van Geuns' excellent booklet (1994) and urges naval architects to understand the physics of a few really large waves rather than dealing with the sea purely as a statistical phenomenon. I strongly endorse this sentiment and hope educationalists will take note.

The penultimate two paragraphs from Mr. Turner continue the destructive theme of downward water impact at the fore end of bluff-bowed ships. The absence of any significant flare exacerbates these effects as Messrs Jono and Fujita also point out. It is my belief, which cannot be absolutely proven, that the same abnormal wave (freak, rogue, episodic whatever, the name is less important than its actions) which burst no. 1 hatch cover, also broke off the port gypsy and tore the starboard windlass, and bollards from their seatings. Before they went overboard (some evidence to starboard) one or other of these heavy items almost certainly struck the aft coaming of the stores hatch and sprung its lid off. There is good evidence for this, as I describe in the paper. At the time the paper was written I was unable to check whether the starboard windlass had ever been found, but I understand it has and is lying close to the bow. This would seem to support Mr. Turner's arguments and the above hypothesis. Another *paradigm shift* in thinking is required to deal effectively with water impact in design. Present standards are far from adequate - it underestimates by an order of magnitude according to a recent HSE report.

Finally, in answer also to Buckley, Grigson and others, I see no evidence so far that existing ships are having their most vulnerable hatch covers reinforced or replaced with stronger ones. Nor do I see signs of increasing the freeboard of B-60 ships as Captain Richardson would wish. Regarding new designs, I understand that IACS and IMO have set in motion steps to substantially increase the strength of forward hatch covers. Unfortunately at this stage the draft IACS S21 (1997):

- does not in my view adequately cater for survivability loading of the forward hatch covers
- appears to require no increase in design loading of the remaining hatch covers
- has a silly error of about 2:1 in the stress based safety factor which then effectively halves the design safety for all hatches!

It is to be hoped some external independent scrutiny can be arranged to put these matters right, otherwise the safety of no. 3 hatch cover may have worsened (see reply to Bell).

Roland Grard has, like many, followed the *DERBYSHIRE* saga avidly; and then, feeling disappointed with the UK/EC Assessors' report, regretted that I had to resign. The DETR really left me no choice.

It would not of course be proper for me to review all the weaknesses in the UK/EC Assessors' report, but I am happy to offer my opinion to Monsieur Grard on the apparent penetration of the bow in the seabed. As he says, this was associated with the Assessors' theory of forward flooding and consequent collapse of no. 1 hatch cover. First, I refer to my slightly revised remarks in section 2.7.2, (and supported by 2.7.1). This offers reasoning, supported by calculations and circumstantial evidence, which:

- explain why the bow was essentially unimploded
- indicate the bow would become completely flooded soon after the sinking actions started.

The Assessors were therefore clutching at a rather weak straw in support of their "theory" .

Regarding bow penetration, I did some calculations for the Attorney General for the Reopened-FI. My results showed:

- terminal sinking speed 8.9 m/s $\pm$ 20%
- penetration of the pelagic ooze at 15° declination was 9.6 m $\pm$ 35%.

The first value compares reasonably well with US Navy full scale measurements for a large Fleet Auxiliary of 7.7 m/s and is as expected somewhat lower than 14.5 m/s from model tests for the fast fine form nuclear cargo ship *OTTO HAHN.*

The estimate of seabed penetration is much less certain because stiffness was estimated from very few but variable core samples. The derived average deceleration of the bow was 4.1 m/s^2 (0.42 g) with a maximum about 50% higher. These decelerations are unlikely to have caused the damage claimed by the UK/EC Assessors. However, it may have caused the observed shear buckling in the side shell port and starboard below the stores flat if the stores spaces were flooded (which I accept is likely).

Whether or not the bow spaces were flooded makes no difference to these calculations, because one way or another these spaces would certainly fill as the ship sank.

Roland Grard seeks clarification on the possibility of the hull breaking during the rapid final sinking sequence. I am not sure I can add much more than is said in 2.7.3 as I regard the possibility as unprovable and speculative. If there was a major break of the hull I believe we can be reasonably certain that final separation of the two parts would have occurred at some appreciable depth as for the *TITANIC* and this would almost certainly have been aided by implosion-explosion actions from intact structures in the vicinity. Had a complete separation occurred close to the surface, the aft section would almost certainly have floated free to be eventually overwhelmed some distance from the bow wreckage (*Lemma 3*).

Having made the calculations I referred to in 2.7.3 I tend to agree with Monsieur Grard's belief that it was unlikely the hull girder remained totally intact during sinking. See also my reply to Mr. D.K. Brown. Also, with the scenario just described, this does not preclude implosion-explosion occurring. Monsieur Grard evidently has a sharp mind and is himself an experienced mariner. His contribution and earlier thoughts expressed to myself and Williams are helpful and very much appreciated.

Dr. Andrew G. Spyrou (Fellow) concentrates initially on historical reflection, which is a very salutary and at times helpful exercise to remind us of how badly mariners and the marine industry are served at present. Coming from such a widely informed and experienced naval architect ship owner, his remarks carry conviction and are welcome.

However, Dr. Spyrou is not content to accept the status quo. He refers to his International Ship Evaluation Panel (ISEP), which he first proposed for dealing with tanker safety. In its present broadened form I supported the idea of an independent Panel to review ship safety issues and wrote to the Shipping Minister. She told me it would not work because everyone has vested interests. Neither I, nor I believe Andrew Spyrou, accept such a negative answer.

I seriously commend the ISEP concept to everyone's attention coupled with a drive toward better and publicly available monitoring of accidents at sea which he described at the end of his interesting and valuable discussion. I would like to believe that the type of forensic investigation indulged in here, together with Bill Buckley's First Principles Methodology approach, would become the working tools of such a panel. This organisational-cum-managerial approach to actively monitoring and improving ship safety is a third *paradigm shift* in thinking closely linked to the previous two I identified. Their implementation would I feel sure pay for themselves several-fold by virtue of much improved ship safety. Design rule errors, such as those given in my reply to Mr. Turner would be much less likely to occur. Moreover, no longer would the Mr. Plimsoll's of this world need to struggle individually against the complacency and inertia of the establishments.

Dr. Spyrou refers to the improvement in the accuracy of weather routing, which I accept. But it is the emphasis on charter dates and times rather than on ship safety which is the main concern still.

I was particularly interested in two of Dr. Spyrou's last comments. First, is the relatively new activity known as Operational Oceanography in the USA and Japan to provide improved sea state forecasting skills. I can also report that arising from my *DERBYSHIRE* work I have become involved in a European project to improve orbiting satellite oceanographic data of extreme seas in the main trade routes and ship graveyards. The main objectives are to provide shape and statistical data on the sort of abnormal waves which cause the most severe damage and loss.

Dr. Spyrou ends by describing present efforts in the USA to monitor casualties at sea and to create a mechanism where information will be shared while providing full liability protection. This is an exciting and badly needed concept which, if successful, would greatly assist in establishing data bases for people to examine across the board. In particular, it would be of enormous benefit to the ISEP mentioned above. However, it is vital that IMO and IACS back these endeavours so that mariners at sea and owners take it seriously. Working with the Confederation of European Ship Masters, I know how difficult it otherwise is to get the right sort of *causative* information from sea.

Professor K.J. Rawson (Fellow, Honorary Vice President) suggests that the RINA Council has been courageous to publish this paper and encourage debate at this time. I believe that, the Council lacked courage and were initially influenced by the DETR. Moreover, their actions delayed the desired debate which the Attorney General was anxious to have. It will not be published with its debate and my response until after the Reopened Formal Investigation. I understand a prime task of the Institution is to encourage a virile professional debate on all matters concerning ship safety as soon as proper information is available for debate. I suggest it should not be influenced by the wishes of any government department unless there is a genuine, and openly seen as defensible, reason for doing so. In this case there was none, and as my opening remarks indicate, the Attorney General's office had warmly welcomed the paper and its discussion. The Treasury Solicitor's office much earlier also welcomed SNAME's printing of this paper and its extensive debate and this was known at RINA. It will not be widely known that I resigned from Council over the issue.

Professor Rawson asks why it has taken 30 years and many ship losses to discover this. Andrew Spyrou and others imply the same question. History shows that even in the 1966 ILLC debate, wise voices opposed the B-60 reduced freeboard concession because of the consequential greater vulnerability of hatch covers. It was suggested as one of the five most likely causes of the loss of the *DERBYSHIRE* in the 1986 DoT report. It was very strongly put forward to the FI of 1987-89 as <u>the</u> cause of the loss. Perhaps a clue to the answer lies in the last sentence of my reply to Dr. Spyrou, and is itself a good reason for organisational changes in regard to ship safety. Ken Rawson himself has suffered from the lone (but wise) voice syndrome! His support therefore has a personal touch, for which I am grateful

Regarding feedback from sea experience, I refer Rawson to the end of Spyrou's discussion and my response. The Classification Societies often pride themselves on their extensive feedback data. But, this data is mostly *incidence* and seldom *causative.*

Professor Rawson was the former Chairman of the RINA Safety Committee. He rightly sees a danger in forcing numeracy upon FSA. To reduce this danger numerals, such as those used here, must be seen as being a notional guide which are best judged independently by several qualified people and then discussed and agreed. I believe this is how it is applied in some offshore assessments to identify the more critical elements of the design and operation. I understand IMO will take a similar route for shipping. As Rawson surmises, most of my secondary recommendations do come into the "just in case" or "near miss" categories.

Eur Ing D.K. Brown (Fellow) has one slight quibble with *Lemma 3*. First, *Lemma 3* does admit the possibility of low probability exceptions. Having said that, I was careful to qualify it by my words "if a hull has separated into two parts before sinking". In *DERBYSHIRE*'s case this was demonstrably untrue by *Lemma 2*. But, as will be seen in my reply to Monsieur Grard, I do accept the possibility that *DERBYSHIRE*, like the *TITANIC*, failed in tension in the upper deck as the stern came out of the water but did not separate into two pieces for some time.

C.V. Betts, CB (Fellow) is slightly misled in believing I had a major disagreement with Williams and Torchio. I am quite sure our differences could have been resolved had we been able to continue working together. Indeed, Mr. Torchio has written a letter of apology to me regarding this. There was a mutual unhappiness, shall we say, between the DETR and myself which Dr. Grigson touches on in his discussion.

But I am pleased that my resignation has enabled me to be the architect and catalyst for this present debate before the forthcoming Formal Investigation.

My reply to Mr. Betts' discussion of the SNAME version of the paper, along with all 10 such discussions, has already been made available to the FI. His last paragraph expresses views and hopes very similar to those of other discussors and requires no particular response from me, other than to thank him for his double support.

Captain Jack Richardson has himself been involved in ship casualty investigations. His knowledge of naval architecture and his experience with naval architects is unusual and in depth. His fulsome support is therefore particularly welcome, and I thank him for bringing his vast experience to our profession, especially in relation to the *DERBYSHIRE*.

Captain Richardson's recommendation to abolish the B-60 freeboard would immediately protect the most vulnerable existing bulkers. But there would probably be objections from the operators because of the associated loss in cargo deadweight. I believe there is no need to abolish the B-60 freeboard providing all hatch covers are replaced with ones at least 3 times stronger than at present. Providing additional fore end protection, as he and others have recommended, is also very sound advice in the light of the fore end carnage seen in the wreck.

Captain Richardson is quite correct to refute as preposterous the UK/EC Assessors' claim that the fore end of the ship turned upside down and the starboard windlass fell off!

William du Barry Thomas (Fellow) is a member of SNAME's Marine Forensics Panel, and his support is doubly welcomed. He reminds us that hatch covers are not always well maintained. His reference to Sebastian Junger's very readable *The Perfect Storm* and other ship disasters is also very appropriate. We indeed do have a lot to learn about the dynamics of ships (and storms) *in extremis*.

Dr. Christopher Grigson (Fellow) has worked with an eminent shipowner for many years and has made a lengthy contribution which is valuable. It has much common sense and advice, particularly for the operational side of our profession.

First, let me make one pedantic but important correction to his step-by-step catalogue of known facts. In (i) he says significant wave heights have been measured as high as 14 m. That is true, but higher values have also been measured from NOAA data buoys and are also recorded in Hogben et al's 1986 Global Wave Statistics. Such measurements define Buckley's (H_s , T_p) survivability envelope of Fig. 4, where a maximum is seen at H_s = 18 m. This value is in fact being used in the NE Atlantic margins for some offshore installations West of Shetland. Grigson's H_s = 14 m is appropriate for his argument because it is the value I used for typhoon ORCHID which sank the *DERBYSHIRE*. But, other typhoons have been much more intense, including ISA which was raging 425 m East of the *DERBYSHIRE* wreck during the last survey! For unrestricted operation of ships I have recently recommended a ship length (L) dependent significant wave height in the length range 75 m ≤ L ≤ 400 m:

$$H_s = 16-(4-L/100)^2 , 150 \text{ m} \le L \le 400 \text{ m} \quad (33a)$$

$$= 2.75 + 0.0467 L , 75 \text{ m} \le L \le 150 \text{ m} \quad (33b)$$

This is a slightly preferable relationship than eq.(6) recommended earlier for ship loading because it becomes tangential to H_s = 7.5 m for L ≤ 75 m and equal to H_s = 15 m for L = 300 m. However, these recommendations are provisional. For water impact, for example, it may well be argued that imposing a length dependency may be unrealistic.

Dr. Grigson's discussion, with which I entirely agree, is really a stand-alone contribution to the debate and only asks one or two related questions. But I will make some supportive and perhaps important amplifying comments:

Although there were wise voices in 1966 who pointed out the increased vulnerability of B-60 ships, these were opinions of a few which could not then be based firmly on analyses or experience. So, as Grigson says, no blame should be attached to the 1966 regulations. But, as Rawson says, why should it take more than 30 years to prove the inadequacy?

It is a sobering thought to wonder that had I not been appointed to assist Lord Donaldson, and written the Annex to his report (1995), would it now be generally agreed that the hatch covers are far too weak? I stress this can not be derived from the survey evidence.

This leads to the value of the survey. I share Grigson's view that the proof that the *DERBYSHIRE* did not break in two at the surface at frame 65 was the most important result from the underwater search. But, it can be argued (see brief remarks in 3.4) that the very economic 1996 phase 1 survey virtually established that fact beyond reasonable doubt. So, a second sobering thought is, was the much more expensive phase 2 survey worthwhile, beyond establishing the underwater technology? It most certainly has not determined the cause of the loss.

Grigson said of the Williams and Torchio recommendations they "cloud the principal question, the weakness of the hatch covers". It is ironic that one of the things I was taken to task over by the DETR was in upsetting Mr. Torchio by using the phrase during the survey "you are clouding the issue"! At the time I explained the meaning of the phrase to Mr. Torchio, but he was not placated. Months later he apologised to me when he realised the trouble he had caused me. But the damage was done and by then the DETR were determined to see me go.

As far as I am aware, nothing is being done to ensure the safety of existing B-60 ships. Captain Richardson suggests abolishing the B-60 freeboard, but this would probably be opposed (see my reply to him).

Regarding new rules for hatch covers, the situation is in hand, but the present proposals are far from adequate (see my reply to R.V. Turner).

Several discussors have suggested the profession owes me a major debt of gratitude for refusing to be silenced. But I could not have achieved this without the constant moral support and professional encouragement from Dr. Grigson in particular, and many others also. Thank you, Christopher, and those others who will recognise themselves.

Tom Allan (Fellow) is the UK's Permanent Representative to the IMO and will be the Chairman of their Marine Safety Committee in 2000. This late submission on behalf of the DETR, which really is clutching at straws, therefore seems astonishing until one remembers that DETR have vested interests in the outcome of the *DERBYSHIRE* investigation. Moreover, this discussion was also copied to the SNAME, which rather suggests this may be a belated attempt to discredit my paper in the USA as well as through the RINA.

In view of the many criticisms that have been made here and elsewhere of the DETR UK/EC Assessors' reports, I am amazed to learn that UK's submission to IMO's bulk carrier safety initiative is based mainly on their conclusions! These are mainly assertive speculations, confused, in places contradictory, qualitatively plausible but unconvincing – "distraction from the truth" as several have remarked. Moreover, only 10% of their many recommendations are based on hard evidence from the surveys – the remaining 90% are opinions. This does not seem to me to be the best basis for taking forward a UK submission. A better mechanism must surely be established in future.

The above facts, together with the tone, low key quality and negativeness of Mr. Allan's discussion, are in stark contrast with that of the 29 other discussors of the paper in SNAME and the RINA. My reply will therefore show little sympathy to him or to his anonymous DETR colleagues. Incidentally, I said my paper was complementary to the UK/EC Assessors' report, not complimentary!

Equation (8) does not calculate the variance of wave heights. The difference in units alone should indicate that. Mr. Allan suggests that my probability of experiencing any given wave height once in 12 hours is 1/3200 waves = 0.03%. This is nonsense, for example, because it implies that:

- the probability in a shorter period of 6 hours say would be 1/1600 = 0.06%, double!
- the probability does not depend on wave height!

I suggest the DETR does its homework more carefully next time. Ochi (1996) and Hogben (1997), who are two very eminent statistical oceanographers, fully support my use of eq(8) and Drake has derived comparable probabilities (1999).

Associated with these probabilities is my derivation from the Longuet-Higgins exceedance equation, of the pdf eqs(9) in section 2.2. I used this later for fore end flooding (in 2.5). From this Mr. Allan is concerned that I seem to allow for only 432 waves rather than the 800 expected in 3 hours. His supposition is correct but then he and his advisers completely miss the reason for it, which is the necessary truncation at waves above H_2 = 26 m which would cause no. 1 hatch cover to burst.

Therefore, far from agreeing with Mr. Allan's bold statement that "All other equations/tables developed using these probabilities throughout his paper must therefore reflect this error and cast considerable doubt on the conclusions reached", may I respectfully suggest he attempts to understand probability theory a little better before a man in his influential position uses it for such an important issue.

Mr. Allan would have been on safer ground if he had confined his remarks to challenging my use of eq(10) for bow flooding rather than a Rayleigh or some other short term distribution. It was a deliberate choice at the time to introduce an acceptable bias toward bow flooding (C7) for the reasons given in my paper. I have elaborated on this in my reply to Dr. Drake where I have used a Rayleigh distribution with H_s = 14 m and allowed for a seastate uncertainty v_s = 0.2 (Faulkner and Sadden, 1979). This shows that the extent of flooding through damaged openings over 3 hours is reduced to about one-tenth of those initially calculated. This would seem to rule out the UK/EC Assessors hypothesis that fore end flooding of tanks and stores led to the ship sinking.

DETR's second criticism is my neglect of relative bow motion. This is partly correct, but unhelpful. Have they got a better alternative? There were no tools readily available to me which could cater even for long-crested seas, let alone the highly confused short crested seas of typhoons. Linear strip theory is quite invalid on many counts.

I therefore did two things, as mentioned in the paper. I studied the DMI photographs of the response of a 1/50 scale model of the *DERBYSHIRE* to elevated long-crested waves 18 m to 26 m high. I also talked to two Master Mariners and one other responsible mariner who had been caught in typhoons in large ships. From these two sources I concluded that after the bow dips into the trough the oncoming steep crest lifts the bow a little and by the time it is abreast no. 1 hatch the ship is close to its static trim which I then assumed.

Dr. Drake's analysis (1999) with similar large and elevated steep crested waves, also supports my level trim assumption and, incidentally, it supports my assumption of quasi-static forces used for ship bending. I also intuitively believe that very short-crested seas are likely to arouse fewer large deviations from the initial static trim than do long-crested seas. I rest my case.

Mr. Allan and his colleagues' third set of comments relate to the diameter of foredeck air pipes and ventilators. I have not been careless and am happy to be corrected. After the final survey, the DETR refused me access to London to check such details and refused my subsequent request for specified photos and drawings. Later on, tabular and drawing details of the fore deck openings were sent to me at the Attorney General's request. These show three 12″ air pipes (305 mm) and various 20″ diameter (508 mm) ventilators mentioned by Mr. Allan, which I had previously taken as 500 mm from the Assessors' report. Bow flooding has therefore been recalculated in Tables 4 and 6, but the results are now of lesser interest for the reasons given in the Postscript which follows this discussion.

As Mr Allan says the new data on the diameter and number of the air pipes and ventilators will affect the time to fill the Ballast Tank. Table 5 has been recalculated to reflect this. The increase in time is not, however, as great as suggested by Mr Allan because as shown by Equation 28 (which is correct within the assumptions made) the filling time is inversely proportional to the orifice area to the power of 2/3. Thus the factor is 2.42 not his 3.75. Nevertheless the recalculated filling times are significantly longer than corresponding sinking times and the lack of implosion in way of the Ballast Tank needs explanation. As mentioned in the paper there are several credible factors that would reduce or eliminate the risk of implosion, the most probable of which is the influence of the long split in Bulkhead 339. The new data certainly does not make the UK/EC Assessors' suggestion that the manhole covers forward had been removed any more credible!

I am sorry Mr. Allan suggests there are other (unstated) points that have caused confusion. If they were important they would no doubt have been included. From the other 29 discussors I have only had one request for clarification. His point about the ventilation of the forward fuel tank is correct, and I accept it entirely. It is unimportant, as my main point was my disbelief that the manhole cover at the top of the FO tank would have been opened. On that issue I have spoken to two Master Mariners who agree with me. I suggest it would be wise for the UK/EC Assessors to change their tune in this respect. The comment regarding bulkhead 65 warrants no reply.

In summary, I do not believe the explicit and implicit criticisms raised by Mr. Allan and his anonymous colleagues are justified. Indeed, I regard them as a most regrettable lowering of the tone of this vital debate by the SNAME and RINA.

To end, I would caution everyone to treat any probability predictions for typhoon storms as being highly notional. I do, however, regard them as being good enough for guidance and comparison purposes only. We have much more to learn about Revolving Tropical Storms in general and extreme ones in particular. I hope the IMO and IACS will therefore support an international endeavour that will shortly be seeking funds or support in kind for this type of study (see the Postscript).

Professor J.B. Caldwell (Fellow and Past President) usually raises perceptive issues, as he does here in asking if the phase 2 final survey was worthwhile. I firmly believe the survey evidence does not by itself lead to the cause of the loss, or even to the most probable cause of the loss "beyond reasonable doubt". Moreover, as Professor Caldwell says, there are differing opinions as to the event chain preceding the loss. Was it therefore worth the two to three million pounds spent? Of course, this depends on what value you attach to several intangibles. I believe there were four positive achievements from the underwater survey:

- Referring to sections 5.4 and 6.1 of the paper, five loss scenarios can be eliminated absolutely. These include the three primary structural scenarios C1, C2 and C3 and it has been said that eliminating Frame 65 scenario (C1) alone made the survey worthwhile. My reaction to that is that we were very nearly able to eliminate C1 from the much cheaper reconnaissance phase 1 July 1996 survey (approx. £200,000) in only 10 hours (see section 3.4). With another 24 hours underwater we would surely have eliminated it. Two of the fore end vulnerability scenarios C5 hatch attachment failures and C6 fore deck corrosion were also eliminated.

- Five other scenarios can really be discounted, four by virtue of their lowest probability ratings ($P_i = 1$): C8 cargo shift/liquifaction, C9 propulsion loss, C10 rudder loss/steering failure and C11 explosion/fire in ER. C12 pooping from forward waves was also ruled out, as argued in section 5.3.

- Accepting these deductions, and referring to the *posteriori* final risk matrix Fig. 18, only four scenarios remain to consider:
  - C4 hatch cover collapse (my conclusion)
  - C7 fore peak flooding (the Assessors' conclusion)
  - C13 pooping actions
  - C14 hatch coaming failure

  I believe there may be some circumstantial evidence to support the possibility of C14 being a contributory factor to the loss. These deductions will hopefully narrow down those to consider at the forthcoming Reopened Formal Investigation (RFI).

- The survey demonstrated that the underwater technology now exists for such investigations in the future.

At the conclusion of Lord Donaldson's work I was alone in questioning whether a survey costing the estimated £2 million was justified. I believed then, and still do, that the implosion-explosion actions deduced from the ITF survey would rule out any possibility that the survey would provide positive evidence to prove or disprove the most probable loss scenario C4. Nevertheless, on balance I do believe that for this particular loss the elimination of the frame 65 scenario (C1), plus the other benefits, do justify the cost. I also believe those costs could be substantially reduced because of the lessons learned from the two final surveys.

As regards "do's and don't's" for future surveys, I have already suggested the professional skills and personal qualities required of the survey team in my reply to Douglas Brown. Professor Caldwell will also be pleased to know that I do have in mind a future paper with a suitably qualified co-author on do's and don'ts, etc. including the legal aspects. Present pressures are likely to push that into late 2001(see Faulkner and Reid, 2001).

Regarding irrefutable proof, I agree with Professor Caldwell's deduction that there can be no such thing as the *absolute* truth about a prior event like a ship loss. However, I do believe that *conditional* truths exist and should be sought. I have attempted to do just that in my paper, to arrive at the most probable loss scenario which would satisfy a court of law beyond reasonable doubt. In this I cannot over stress the value of *Lemma 5* which distinguishes between *initiating* and *terminal* events (section 5.5). This was not considered by Mr Justice Colman at the RFI.

B.M. Bell (Member) refers mainly to hatch cover strength matters. He also refers to "others" who have been instrumental in drawing attention to the inadequate strength of hatch covers. Who they were? I have acknowledged the qualitative concerns following ICLL'66.

After my work with Lord Donaldson I became aware of Burness Corlett & Partner's tests and contribution to the 1989 FI and their firmly stated belief that weak forward hatch covers was the prime cause of the loss of *DERBYSHIRE*. I also know that DNV had previous casualty experience which caused them to increase their forward hatch covers strengths somewhat, as did two other class societies after Lord Donaldson's Assessment.

Whilst the UK/EC Assessors recommended increases in hatch cover strength, there is nowhere in their report any calculations or evidence which justifies this. Indeed, their conclusions suggested it was only because of complete bow flooding that the forward hatch covers experienced loads in excess of their design values!

Regarding the 1988 IACS UR S21 revised requirements, Mr. Bell is incorrect in saying that a new *DERBYSHIRE* type bulk carrier would have hatch covers 1.6 times stronger than is required by the 1966 ICLL. My calculations agree approximately with 1.6 for no. 1 covers, but for nos. 2 and 3 the factors are about 1.2 and 0.8 respectively. That is, no. 3 covers are now actually weaker than by the 1966 ICLL. This arises, it would seem, because the allowable stress was increased from about 0.4 yield to 0.8 yield, without any reference to the loading. Hence the required increase in hatch cover safety (about three-fold increase) was not achieved. The final report by Mr. Justice Colman recognises the likelihood of this inadequacy and recommends UR S21 be re-examined and, in any case, replaced with improved formulations. He also made a strong recommendation for retro strengthening or replacement of hatch covers in existing bulkers (about 5000 ships).

I do not accept Mr. Bell's reasoning for preferring a working stress approach to the ultimate limit state approach which I advocate. He should read Professor Caldwell's contribution to the SNAME version of my paper (Transaction 1999-2000) which points to the far greater efficiency of the ULS or plastic collapse approach. Briefly, a ULS approach:

- is more efficient because yield initiation is not a good indicator of collapse in normally loaded orthogronally stiffened plated grillages
- does not imply any permanent set at the design load, because the choice of load factor can prevent this where this is a requirement
- does allow for elastic or inelastic buckling of the plating and/or stiffeners.

It is therefore the logical choice, and I wonder if Mr. Bell does not adequately understand ULS methods.

Regarding a recommended load factor to collapse beyond the present design load, I am aware of the Byrne and Evans 1998 paper and their suggested increase for the forward covers by a factor of 3. This is the upper end of my values in Lord Donaldson's Annex, but I have hardened on at least 3 since then, as my present paper suggests. The professional debate from the discussors has not challenged this, but I have recommended that this be finalised in the light of the final tests at MARIN (see the Postscript). Incidentally, my suggestions also extended to cargo ships of differing lengths between 150 m and 400 m. These cater for all hatch covers and not just the forward ones, because extreme elevated seas from the stern quarters or on the beam do occur.

I accept that extra strength is required at the inboard edges of the side-rolling hatch covers, and at the three other boundaries. Beyond the primary stiffening my paper made no attempt to examine or optimise the supporting structure. Byrne did this for the Re-opened FI, and confirmed my judgement that the extra weight and cost was not excessive, and certainly not proportional to the three-fold increase in strength. See also Faulkner, 2001.

Mr. Bell wonders if my suggested design criteria is "over onerous" to meet. But two paragraphs later he seems to imply that the conditions reached by "nature's fury" may be rather different from those calculated. May I suggest he reads the papers by Buckley and myself at RINA's October 1997 conference "*Design and Operation for Abnormal Conditions*". This describes Survival Design principles and attaches much importance to experimental data and improved service feedback where these can be obtained. A second version of the Conference is to be held 6/7 November 01. See also the last paragraph in the Postscript which refers to an ongoing EC MaxWave study to provide some of the improved data for Survival Design.

Regarding desirable team qualities when examining a wreck, I refer Mr. Bell to my reply to Douglas Brown. I list those professional and personal qualities that seem desirable to me in the light of the *DERBYSHIRE* experience. In some of these political interference is implied and was certainly present and eventually led to my "resignation" - which many found strange.

Mr. Bell is right in saying some disagreement among assessors is inevitable. Most disagreements will be secondary and indeed are healthy, providing an agreed consensus is reached, as was the case throughout the work Robin Williams and I did for Lord Donaldson. Unfortunately, the EC appointment was political. But that alone was certainly not the main reason why the DETR forced my resignation. All I will add here is that I had absolutely no wish to resign, as those who know me will understand.

I thank Mr. Bell for touching on so many important issues.

W.J. Cairns (Fellow) provides a most welcome contribution with some interesting experience and comments, most requiring no specific replies. Captain Edwards has recently written an excellent book (1998) which discusses several incidents of severe damage or ship loss that many naval architects would benefit from. It would make up for their sad lack of seatime experience and little discourse with seafarers.

I refer Mr. Cairns to my Postscript which refers to excellent SSRC tests that suggest breakwaters are only marginally beneficial. Moreover, they could, as Mr. Cairns suggests, actually increase green water loads on no. 1 hatch covers. I agree with Mr. Cairns that there is no service experience of extensive flooding of the fore peak spaces in the large bulkers. And yet Mr. Justice Colman concluded without evidence that extensive flooding was likely, as discussed in my Postscript.

I share Mr. Cairns' concern that the weakness of the forward hatch covers was not recognised much earlier. The results from the excellent DMI tests of 1986-87 which Burness Corlett & Partners sponsored, and their review of service experience, surely demonstrated this beyond reasonable doubt. By my estimates as many as 400 seafarers may have perished as a result of this delay. The short comings of the 1989 FI have been well ventilated.

Incidentally, I believe that Brian Corlett's suggestion that the ship's report of 30 ft waves was in reality probably 30 m is now widely accepted. Nevertheless, Mr. Justice Colman's findings were mostly influenced by tests of short duration (mostly 2 hour runs) having a significant wave height of only 10.85 m as suggested by Dr. Cardone, whose firm provided advice to LR and to the RFI. Again, I add additional comments on that in my Postscript.

Mr. Cairns is not being contentious when he points out my change from believing in 1990 that failure at bulkhead 65 was more probable than hatch cover failure to vice versa in 1995. I hope my reply (2) to Messrs. Lambert and Ramwell adequately explains the change in the light of new knowledge.

I agree with Mr. Cairns that it is likely (but not certain) that many losses are due to a combination of events. Mr. Justice Colman felt, as did the UK/EC Assessors, that some substantial degree of bow flooding was necessary before the forward hatch covers became vulnerable. But, with respect, that is pure assertion based on no wreckage evidence of substantial flooding, nor does previous ship experience support this as Mr. Cairns himself points out. I believe this is the one blemish in the Judge's final report. My Postscript also briefly discusses this.

The UK/EC Assessors' belief that the forward stores hatch was left unsecured can easily be rejected on structural behaviour evidence and reasoning, or by other reasoning, as agreed by Mr. Justice Colman and mentioned in my paper.

Mr. Cairns believes the prime cause of the loss was that the vessel appears to have been directed into the typhoon's most dangerous sector. This is rather novel and could suggest that the weather routing was at fault, but Mr. Justice Colman rejected this. This leaves the Captain, but whilst he has been criticised for not avoiding the typhoon, I believe this to be unfair speculation.

I offer no comments on Mr. Cairns' other speculations and thank him for his wide ranging, thoughtful and interesting contributions.

Dr. J.C. Chapman (Fellow) wonders if I might have underestimated the possible effects of cargo liquefaction, and that subsequent listing might make a disastrous event more likely.

Certainly, cargo shift can occur with some level of liquefaction and therefore cannot be ruled out. I studied some of the quite extensive UK research undertaken in UK for the Department of Transport (Krusjewski et al, Skinner and others). I also recognised that in 1980 for the last voyage of the *DERBYSHIRE,* trimming (levelling of fine ore concentrates was not practised. Nevertheless, in risk matrix terms, I assessed notional values to:

- the **probability** of serious cargo shift was low $P_i = 1$ on a scale of 1 to 5
- the **seriousness** of the **consequence** should it occur (about 6° list assumed based on information from the 1989 FI) as being moderately serious $S_c = 2$ or possibly 3.

The resulting risk numeral R_n of 2 or 3 suggested that cargo shift was a low risk in *DERBYSHIRE* compared with 10 other loss scenarios having $R_n \geq 6$ and 5 scenario with $R_n \geq 10$ which were given greater priority in our studies with Lord Donaldson. It was also felt that the Captain would have detected and reported any significant cargo shift - this was the main reason for my low value of P_i.

Subsequently, I came across evidence of 4 large bulk carriers over a period of about 15 years that had experienced and reported significant lists due to cargo shift. Three were towed to port and the fourth was abandoned and eventually sank. No lives were lost. In only one ship was the list estimated at about 10° (I recall the ship was rolling heavily).

In the light of this work I recommended to the DETR in 1997 that further research be undertaken to provide better understanding and better IMO guidelines. Even though efficient trimming is nowadays quite general, there is nevertheless still some risk of cargo shift. I am unaware of any further research.

Takashi Jono and **Monoru Fujita** are consultant naval architects from Osaka. Their strong support is very welcome, particularly as their country's naval architects have examined some of their own bulk carrier losses in some detail.

They go on to make 2 additional recommendations. The first is for seaworthiness model tests to study the effects of incorporating more bow flare, and of varied longitudinal inertia arising from different cargo distributions to see if the observed tendency to plunge the bow deep into the sea can be minimised. I agree with these proposals, especially as the Japanese have some very fine testing facilities and a sensible tradition still of attaching importance to physical tests. It is important to simulate steep elevated waves at a reasonable scale, some of which should, if possible, approach the survivability conditions defined in this paper.

Their second recommendation is to establish clear operational criteria to avoid such failures in heavy seas. I certainly agree with what I understand is their belief that present ship design rules are not adequate to enable a ship to have safe unrestricted operation worldwide. But I am less convinced that taking appropriate avoidance actions can be made to work as a prerequisite for safe operation. Weather routing, ostensibly introduced to achieve this, has been in operation for more than 40 years and it is simply not working in the best interests of ship safety, as many mariners will tell you.

I do not believe present ship rules are adequate. That being so, Mr. Buckley and more recently I have been pressing for a survivability approach to ship design. This was touched on in my paper, but I suggest Messrs Jono and Fujita obtain our 1997 RINA joint paper, and the earlier ones I have referenced by Buckley and his recent paper (2001). You have touched on a very big issue, and I believe a concerted international move toward survivability design is a vital prerequisite to achieving a significant improvement in ship safety.

Thank you both for your well considered and positive discussion. I hope you can find sponsors for your proposed tests which would be of interest and value to many.

POSTSCRIPT

This reply to the Discussion was held back until after The Honourable Mr. Justice Colman had finished his report on the Re-opened Formal Investigation (Colman, 2000). This would allow Messrs. Williams and Torchio, and perhaps others formally involved in the RFI, to contribute to the paper. No one did, so I end my reply with this Postscript on some relevant model tests and a comment on Mr. Justice Colman's main conclusion.

Strathclyde University's Ship Stability Research Centre completed some excellent 1/65 scale *DERBYSHIRE* model tests at their upgraded Denny Tank (IMO, 1999). Head seas at three seastates up to H_s = 12.8 m with forward speeds of 0, 4.8 and 8 knots showed:

- deck wetness and green sea deck loads are very sensitive to bow height and forward speed
- changes of bow shape and introducing breakwaters are only marginally beneficial
- introducing higher reserves of buoyancy has questionable benefits
- the existing Load Line standard for hatch cover strength is clearly inadequate for all tested conditions.

The tests did not support the EK/EC Assessors' hypothesis of slow bow flooding, nor did subsequent theory (Vassalos and Jasionowski, 1999), which also examined fast flooding during sinking.

For directionally spread seas the model was transferred to MARIN. Unfortunately, the vast majority of the tests were conducted at moderate seastates with H_s = 10.85 m ± 10% as predicted recently by Oceanweather hindcasts for typhoon ORCHID at *DERBYSHIRE's* likely locations before sinking (MARIN, 1999-2000). These tests merely confirmed that no. 1 hatch cover would not be at risk (42 kPa taken as failure pressure) and that some degree of bow flooding could occur if any of the foredeck ventilators and air pipes had been damaged.

Even though the corkscrew ship motion seas did lead to water being scooped up by the bulwarks, the likelihood of many such openings being damaged at this moderate seastate would seem to be low. The tests also showed that with the bow fully flooded (about 9000 tonnes) no. 1 hatch cover would fail. This is hardly surprising as the loss of freeboard at no. 1 hold would then be about 2.7 m!

I pointed out that Oceanweather's hindcast was merely an *average* over 23 km grid squares and that the upper and lower bands of ± 10% were quite misleading. These were based on comparisons of 290 hindcast averages, and whilst 10% may reflect the standard error for *average* predicted seastates over 23 km grid point squares, it could not possibly reflect the uncertainty in the *local real sea*. I suggested this is likely to be three or more times higher (Komen et al, 1994), that is H_s = 14 m or more. Recent analysis for the SHIEHALLION FPSO bow damage West of Shetland

(November 1998) has confirmed H_s at 14 m compared with the initial hindcast prediction of about 10 m.

Therefore, later tests during and after the Court hearings included higher seastates of 12.5 m H_s (and eventually up to 15 m and 16 m H_s, but these results were not available to the Court or to Mr. Justice Colman). For a ship speed of 2 knots, the maximum thought likely for the prevailing seas, the probabilities of no. 1 hatch covers failing for the intact ship as deduced from tests over 90 to 120 minutes were:

H_s (m)	p_f (%)
10.85	0
11.9	4
12.5	30

However, these tests had their limitations:

- test runs no more than 2 hours
- no tests with partial bow flooding only
- sparse data requiring generalised Pareto probability extrapolations
- no injection of abnormal waves (freak, rogue, episodic, etc.)

Moreover, as Mr. Justice Colman pointed out, no serious attempt was made to consider the accuracy with which the MARIN test data replicate true sea conditions allowing for intrinsic uncertainties. These and substantial sea state uncertainties would naturally increase the hatch cover failure probabilities. Longer exposure (than 2 hours) and the occurrence of abnormal waves would also of course increase these probabilities still further. It also seems possible to myself and others that many, if not most, of the broken ventilators and air pipes were damaged by such a wave which dislodged the starboard windlass (found near the bow) and collapsed no. 1 hatch covers at the time sinking started.

Lower seastates favour bow flooding through any MVs and air-pipes which may be broken. Most MARIN tests were at 10.85 m H_s with no normal possibility of collapsing no. 1 hatch covers. Only abnormal waves would then do that and this possibility was not explicitly considered.

This is very probably the reason why Mr. Justice Colman regarded some degree of bow flooding as being the necessary *initiating cause* of the loss. I would, of course, have been happier with the phrase *initiating event*, but in either case I would draw attention to my *Lemma 5* in section 5.3.

Bow flooding would not itself sink the ship. It may have exposed no. 1 hatch covers to a test which it fails. It is therefore this fundamental weakness which is the true cause of the loss, and hence the 1966 ICLL is the underlying cause. It is also gratifying to note that this is implicit in Mr. Justice Colman's strong recommendations for substantially stronger hatch covers in both new and existing bulk carriers.

I am pleased to be able to close this Postscript by referring to a 3 million ecu 3-year R&D study "MaxWave" which started in December 2000 and which owes much to my involvement in the *DERBYSHIRE* investigation. In September 1998 I was invited to give a Keynote address to meteorologists, oceanographers and ocean engineers (Faulkner, 1998c). The white paper from the follow on working groups recommended such a study, and myself with Johannes Guddal of DMI, Bergen, and others have been developing 10 work packages for the study. These cover enhanced satellite and other remote sensing devices to get more definitive data on steep elevated abnormal waves through to proposing new survival design and operational requirements of ships and offshore installations. The work is being undertaken in France, Germany, Norway, Portugal and UK, and is supported in principle by IMO, IACS, HSE, the International Chamber of Shipping and others. They have been kept informed and will study the recommendations for implementing these new requirements.

However, at the end of the day its actions that count, and we all know how reactive and ponderous IACS is.

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