Neutral Citation Number: [2012] EWHC 1245 (QB)

Case No: 2011 FOLIO 58

IN THE HIGH COURT OF JUSTICE
QUEEN'S BENCH DIVISION
COMMERCIAL COURT

Royal Courts of Justice
Strand, London, WC2A 2LL

Date: 11/05/2012

Before
Mr Justice Popplewell

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Between :

(1) EUROPEAN GROUP LIMITED
(2) ALLIANZ GLOBAL CORPORATE AND SPECIALITY AG (UK BRANCH) (formerly known as Allianz Cornhill Insurance PLC)
~~(3) CHARTIS INSURANCE UK LIMITED (formerly known as AIG (UK) Limited and AIG Europe (UK) Limited~~
(4) HDI-GERLING INDUSTRIAL INSURANCE (UK BRANCH)
(5) MITSUI SUMITOMO INSURANCE UNDERWRITING AT LLOYD'S LIMITED
(6) AF BEAZLEY (SYNDICATE 2623/623) AT LLOYD'S

Claimant

- and -

CHARTIS INSURANCE UK LTD (formerly known as AIG (UK) Limited and AIG Europe (UK) Limited)

Defendant

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Rachel Ansell and **Simon Goldstone** (instructed by **DAC Beachcroft LLP**) for the **Claimants**

Guy Blackwood and **Gemma Morgan** (instructed by **Waltons & Morse LLP**) for the **Defendant**

Hearing dates: 22 -23 March 2012, 26 March-30 March 2012, 2 April 2012 and 4 April 2012

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Approved Judgment

I direct that pursuant to CPR PD 39A para 6.1 no official shorthand note shall be taken of this Judgment and that copies of this version as handed down may be treated as authentic.

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Introduction

1. This case concerns which of two insurers is liable for losses caused by fatigue stress cracking to tubes in economiser blocks which were installed in a new waste recycling plant at Colnbrook, near Slough. Both insurers provided property insurance to the participants involved in the development of the recycling plant.

2. The Claimants, together with the Defendant, were the subscribing insurers to an Erection All Risks, Public Liability and Delay in Start Up Insurance Policy (the "EAR Policy") in relation to the project. I shall refer to such insurers collectively as the "EAR Insurers". The Defendant alone was the subscribing insurer to a Primary Marine Project Cargo/Delay in Start-Up Insurance Policy (the "Marine Policy").

3. Lakeside Energy from Waste Limited ("Lakeside") was the developer of the recycling plant ("the Lakeside Facility"). Lakeside engaged two entities as the engineering, procurement and construction contractors for the development of the Lakeside Facility, namely Itochu Corporation ("Iotchu") and Takuma Corporation ("Takuma"), collectively referred to as "the EPC Contractor". The manufacture of the economiser blocks was subcontracted by the EPC Contractor to Vulcan SA ("Vulcan") in Bucharest, Romania. Fourteen of the sixteen blocks were transported by road from Bucharest to Constanta, by ship from Constanta to Southampton and by road from Southampton to the Lakeside Facility. The other two blocks made the entire journey overland by road.

4. The EAR Policy named Lakeside and the EPC Contractor as assureds. It provided cover against physical loss, destruction or damage to all items designated for incorporation in the project, together with all consumables. Cover for equipment and materials procured from outside the United Kingdom commenced upon the attachment of lifting gear to such equipment and materials once at the Lakeside Facility.

5. The Marine Policy named Lakeside and all Lakeside's engineering and procurement contractors (which included the EPC Contractor) as assureds. It provided cover during transit against all risks of loss or damage to plant, equipment, materials and machinery in connection with the project. So far as the economisers were concerned, cover commenced from the time the goods left the factory in Bucharest and terminated on delivery to the Lakeside Facility.

6. Both Policies provided cover in the event that damage to the property covered by the Policy caused a delay in the start up of the project.

7. Both Policies also contained a clause ("the 50/50 clause") which provided that in the event it was not possible to ascertain whether the cause of damage to the insured's property occurred before or after the arrival of the property at the Lakeside Facility, the EAR Insurers and the Marine Insurers would each contribute 50% of the Insured's properly adjusted claim.

8. The Marine Policy incorporated the Institute Cargo Clauses (A) which by clause 4.4 exclude liability for "loss damage or expense caused by inherent vice or nature of the subject-matter insured".

9. Fatigue crack damage to the tubes was discovered in February 2008, when gas tight tests prior to commissioning led to the detection of leaks. By that time the economisers had been on site at the Lakeside Facility for between four and six months. Upon investigation, the fatigue cracking discovered was in way of the weld joints of cranked tubes to the upper headers in at least nine of the sixteen blocks. In some cases it had penetrated through the full thickness of the tube. It soon became clear, and is common ground in this action, that the fatigue cracking was caused by resonant vibration which must have occurred between the time the blocks left the factory in Bucharest and the discovery of the cracks on site.

10. There was a claim on both Policies for the costs of repairing the tubes and associated costs. There arose a dispute between the EAR Insurers and the Marine Insurers as to whether damage had been suffered prior to arrival at the Lakeside Facility. The Defendant contended, and maintains, that the necessary vibration could not have occurred during road and sea transport and that it is likely to have resulted from turbulent wind causing the tubes of the economisers to vibrate whilst the economisers were exposed to the elements at the Lakeside Facility. The Claimants contended, and maintain, that the vibration which caused the fatigue failure occurred during transport (predominantly or exclusively the road transport, rather than the sea transport) by reason of missing packing between the rows of tubes in the economisers.

11. The EAR Insurers settled the claim for £4,600,000 without prejudice to their position that the damage had occurred during transit and was covered under the Marine Policy alone. The Claimants now claim from the Defendant their respective proportions of the amounts paid in settlement of the claim, totalling £3,680,000 (i.e. £4,600,000 less the Defendant's 20% share in its capacity as an EAR Insurer).

The Damaged Economisers

12. The Lakeside Facility generates energy from waste. The process involves separating the recyclable elements from the waste and then incinerating the remaining waste to generate heat which drives steam turbines to generate electricity.

13. The heat which drives the steam turbines is generated by two boilers. Each boiler contains an economiser. Each economiser is made up of eight economiser blocks (numbered 1 to 8) which are arranged in two lines of four. The boiler water passes through the tubes in the economiser and hot exhaust air from the waste incinerator is blown around the tubes through the array so that heat is transferred to the water flowing through the tubes.

14. Each of the eight economiser blocks making up each economiser is about 7m high and consists of 24 rows of 40 vertical steel tubes. The rows of vertical tubes are welded to tubular steel headers at the top and bottom of each row. The vertical tubes have an outer diameter of 42.4mm and a 4mm wall thickness. 12 of the rows of tubes comprise short tubes which are 6.2m long and 12 of the rows comprise slightly longer tubes which are 6.4m long. The rows of longer and shorter tubes in each economiser block alternate so that the adjacent rows within the economiser blocks contain tubes of different lengths.

15. In service, the blocks of tubes are hung from the upper headers. The upper headers are connected together by welded plates. The lower tubular headers are not connected together and are only restrained by drainage connections. The blocks therefore comprise hanging rows of tubes, each row connected at the top and at the bottom to a tubular header running the length of the row, with the bottom headers alternately hanging a little above or below the level of the parallel one in the adjacent row.

16. In each economiser block, the 12 outer tubes in each row of 40 are cranked tubes. The crank is a kink in the tube about a third of the way down their length which enables the upper end to be connected to the header which is two rows across from the lower header to which the bottom of the cranked tube is attached. In each cranked tube the part above the crank is parallel with the part below the crank but aligned two header's distance apart. This is in order to interconnect the banks of tubes and to allow water to flow through the economiser.

17. The arrangement and numbering of the economiser blocks was as follows:

Boiler 2	Block 2	Block 4	Block 6	Block8
	Block 1	Block 3	Block 5	Block 7

Boiler 1	Block 2	Block 4	Block 6	Block8
	Block 1	Block 3	Block 5	Block 7

18. For each block in the above plan, the headers run vertically on the plan, connecting together 40 tubes in that dimension to create rows; there are 24 such rows which stretch across the plan. The cranked tubes are the outermost 12 of the 40 in each row, that is to say in the above plan they stretch across the entire width of the blocks and are 12 deep at the lower part for Blocks 1, 3, 5 & 7 and the upper part for Blocks 2, 4, 6 & 8. The cranked tubes were therefore those on the outside of the economisers and lining the corridor between the two boilers.

19. The effect is that for a given upper header, it will be connected by 28 straight tubes to the lower header directly below it and by 12 cranked tubes to the lower header two rows across (which at the point where blocks are adjacent will be to the first two rows of the adjacent block).

20. The original design for the connection of the cranked tubes to the upper tubular headers provided for all of the cranked tubes to be welded using a butt weld to a stub coming out of the upper tubular header. This was referred to as a "set-through connection". The design was changed by Takuma's designers so that it provided for a set-through connection for tube 1 only, with all of the other cranked tubes welded directly to tubular upper headers, using a T-butt weld and an additional reinforcing fillet weld. This was referred to as a "set-on connection".

21. During the manufacturing process, the economiser blocks were assembled in successive layers. First the straight tubes were welded to their headers with the tubes aligned vertically. This allowed the connections to the upper headers to be carried out with the tubes inverted and the welder completing the circle of the weld from above the header. This was referred to as a "downhand weld". A different welding process applied to the cranked tubes, which had to be welded to a header two rows away. Each row of tubes was placed successively in a frame with the tubes in a horizontal position. The welder would then carry out a first layer of the upper half circle of the weld, from above in the downhand position, at the connection between the cranked tubes and the upper tubular header. The header and tubes would then be jacked up so that the lower half circle of the weld could be completed from below with the welder lying on his back. This was referred to as an "overhead weld". When carrying out an overhead weld it is more difficult to achieve an ideal weld profile, because of the position in which it is carried out. The upper tubular header and tubes were then lowered back into position and the weld completed by applying a final layer to the upper half circle of the weld in the downhand position.

22. Each of the economiser blocks was transported separately in the horizontal position in its own shipping frame. There was packing between the tubes. There is an issue, to which I shall return below, as to what, if any, of the intended packing was missing. The intended packing comprised the following:

    (1) Strips of rubber inserted between the tubes at three locations in the bank, namely near the upper headers, in the middle and near the lower headers.

    (2) Each "strip" of packing was not, so it appears, a single strip either as to depth or length. It appears from the photographic evidence that between each bank of tubes the packing at each of the three locations comprised about five strips laid on top of one another to provide the full thickness of the packing. These were not, so it appears, single strips extending the full width of the block, but were, at least in some cases, two (or possibly more) lengths of packing in a longitudinally adjacent line to each other. Accordingly when talking of "missing packing" between two particular rows of tubes, the packing might be partly missing along its length, or partly missing in its thickness, or wholly missing in either respect. Such is the case at each of the three places between rows of tubes where packing was intended to be placed.

    (3) Clamping bars were intended to be placed at each end of each of the three packing locations to secure the tubes to the frame and keep the packing between the tubes in compression.

23. The packing was inserted between the rows of tubes during the manufacturing process. It was placed between the rows prior to the welding of the crank tubes to the upper headers.

24. Each of the economiser blocks was inspected, subjected to a hydraulic pressure test, and certified by SGS prior to being transported from Vulcan's factory. It is common ground that the fatigue cracking did not take place prior to that point in time.

25. 14 of the 16 economiser blocks were transported by road from the Vulcan factory near Bucharest to the port at Constanta, which is a journey of about 215 kms. Each was on its own trailer and was a wide load. The sea leg of the journey took place on two vessels

which carried the blocks to Southampton, from where they were transported by road to the Lakeside Facility, again on their own individual trailers. Boiler 1 Blocks 1 to 6 were shipped on 10 July 2007 on board the M/V "OXL BLUE SEA". Boiler 2 Blocks 1 to 8 were shipped on 15 August 2007 on board the M/V "ROELOF". Boiler 1 Blocks 7 and 8 were transported by road all the way from the Vulcan factory to the Lakeside Facility in July and August 2007 respectively. On arrival, the blocks remained within their frames for a period awaiting installation. The blocks for Boiler 1 were installed on 25-26 September 2007, one month before those for Boiler 2 which were installed on 23-30 October 2007. At the time of the installation of the economisers, other parts of the Facility were still in the course of construction and the external skin of the building housing the Boilers was not complete. It was completed in late February 2008 after the cracks in the crank tubes had been discovered earlier that month.

26. On 9 February 2008 an air leak test was carried out on Boiler 1 which identified 26 leaks in the area of the overhead welds at the upper header of the cranked tubes in Blocks 1, 3 and 5.

27. Shortly after the air leak test, Takuma notified a claim under both the EAR Policy and the Marine Policy in respect of the damaged economisers, and claimed an indemnity for the costs of repairing the cranked tubes and delay in start up losses. The EAR Insurers appointed Mr. Francis Barber, a loss adjuster and a director of Cunningham Lindsey, to investigate the claim. Mr Barber instructed Dr Rob Allen, a senior engineer at Intertek Capcis ("Capcis"), to carry out a forensic investigation into the cause of the damage. The Marine Insurers appointed Mr Ian Levitt, a marine surveyor employed by Robert Lyon & Co who in turn instructed Mr Stephen Rowbotham, a metallurgist and senior materials consultant, who was at that time employed by Bureau Veritas.

28. Takuma appointed Force Technology ('Force'), to investigate the leaks. Takuma produced a Provisional Report on 25 February 2008 and a Summary Report dated 13 March 2008. On 13 March 2008 there was site meeting between Lakeside, Takuma, Dr Allen, Mr Barber, Mr Levitt and Mr Rowbotham. At that meeting, Lakeside and Takuma briefed the loss adjusters in accordance with the Takuma's Summary Report and put forward proposals for repairing the damage to economiser blocks. It was agreed that repair would be quicker than obtaining replacement blocks from the manufacturers in Romania, which would have had a 12 to 18 month lead time. The time which would have been taken up in any remedial or replacement works was as important to insurers as to the participants in the project because of the Delay in Start Up cover provided by both the EAR and the Marine Policies. It was agreed that testing (the scope of which was to be agreed between Dr Allen and Mr Rowbotham) would be carried out on behalf of all the parties by E.ON.

29. After further investigation the decision was taken to completely remove and replace the entire sections where all the crank tubes were welded to the upper headers in all 16 blocks. The reasonableness of this course was not in issue before me. The remedial work was completed on 24 November 2008.

30. Lakeside and Takuma were not prepared to allow an indefinite period to determine which Policy responded. They threatened to bring proceedings. The EAR Insurers and the Defendant entered into a written agreement dated 13 July 2009 (the "Insurers' Agreement") pursuant to which it was agreed that the EAR Insurers would finalise the settlement of the claim on express terms that the EAR Insurers and the Marine Insurers

reserved their rights in full in respect of the dispute between them as to the application or otherwise of the two Policies. Pursuant to the Insurers' Agreement on 17 August 2009 the EAR Insurers entered into a Discharge Agreement with Lakeside and Takuma pursuant to which the claim was settled at a compromise value of £4,600,000. The EAR Insurers took an assignment of the assureds' rights under the Marine Policy.

The extent and location of damaged tubes

31. Both parties sought to draw conclusions in favour of their case from the extent and pattern of damage to the tubes as between, and within, the 16 blocks. However there are limitations in the data on which these arguments were based. Where there had been a leak in the gas test, and where specific physical inspection had been carried out, it was possible to identify individual tubes which had suffered fatigue cracking. The investigations into the extent of cracking had, however, mainly been undertaken by Magnetic Particle Inspection. MPI testing can not distinguish between cracks in the tubes and mere crack like imperfections in the weld material which do not indicate fatigue stress fracturing and may be of no structural significance. The MPI testing identified a large number of tubes in which there were "crack like indications". It was agreed between the experts that it was likely that a proportion of the "crack like indications" found by MPI testing were fatigue stress cracks in the tubes. It was a matter of uncertainty as to what this proportion might be. In July 2010 Capcis examined 20 tubes which had been identified by MPI testing as having crack like indications (from the inner blocks) and found 7 of them to have actual cracks. It was impossible, however, to extrapolate from this small sample a general conclusion as to how many of the crack like indications were indeed cracks rather than innocuous weld imperfections; still less could one draw any conclusions as to the location of such tubes which were truly cracked tubes. Moreover there is a significant proportion of tubes which were not subjected to any testing. It is impossible to say whether any of these tubes were cracked, and if so, which.

32. All the cracks and the vast majority of the crack like indications were at the weld joint between the crank tube and the upper header. I was provided with a helpful chart, agreed by the experts, showing where the cracks and crack like indications were for all 16 economisers. The distribution can be summarised as follows:

    (1) In Boiler 1 there were 38 tubes with cracks and a further 178 with crack like indications. Several hundred tubes were not tested. The cracked tubes were predominantly in the outer blocks 1 and 3 (11 and 21 cracked tubes respectively), but cracks were also found in tubes in Blocks 2, 4 and 5. No cracks were found in Blocks 7 and 8, which were those which had been transported entirely by road, but both contained a number of tubes with crack like indications and each contained a number of tubes which were not tested. In Blocks 1 and 3 the cracks were predominantly towards the more outward facing tubes, but there were crack like indications running the full length of rows through to the innermost row of crank tubes (row 12). In Blocks 1 and 3, where the majority of the cracks and crack like indications were found, over 200 of the crank tubes were untested.

    (2) In Boiler 2 there were 6 tubes with cracks and 117 with crack like indications. Again, several hundred tubes were not tested. The 6 cracked tubes were

distributed in ones and twos between blocks 2 and 4 (outer) and 3 and 5 (inner). All 8 blocks had some crack like indications.

The Issues

33. The main issue in the case was as to the cause of the fatigue cracking. The Claimants contended that it had occurred during transport, entitling them to recover 100% of their claim; or alternatively that it was not possible to determine how or when it had been caused, in which case they were entitled to recover 50% of their claim. The Defendant contended that the damage had been caused by wind excitation after the blocks had arrived at the site and been installed.

34. The Defendant had an alternative case relying on the inherent vice exception in the Marine Policy. It argued that if, contrary to its primary case, resonant vibration during transit was a cause, nevertheless there was an additional proximate cause comprising one or more of (a) non zero mean stress imparted into the economisers by reason of the jacking up procedure necessary to complete the welds between the cranked tubes and the headers in the overhead position, (b) the quality of the welds themselves and (c) the set on design of the cranked tubes to the headers doing away with the stub pipe which was retained only for the outermost tubes. These, it was said, amounted to inherent vice and excused the Defendant from liability under the principle that where a loss has two proximate causes, one of which is within the policy and the other expressly excluded, the exclusion takes effect to exempt the insurer from liability.

35. There was additionally a quantum point taken by the Defendant that of the settlement sum of £4.6 million, only £4.3 million was established as a proved loss.

36. There were further issues as to the Claimants' title to sue and the measure of loss recoverable, and as to the application of deductibles. The Claimants advanced their claim on three alternative bases:

    (1) under the terms of the Insurer's Agreement, which the Claimants contended involved an express promise on the part of Marine Insurers to pay the amount of the claim if and to the extent that it was established that it fell within the coverage of the Marine Policy;

    (2) as assignees of the assured's rights under the Marine Policy;

    (3) as a claim in equity for compensation or reimbursement.

37. At the outset of the hearing the Defendant conceded that the Claimants had title to sue on the second basis advanced, namely as assignees under the Marine Policy. However it appeared that it might remain relevant whether the Claimants could establish a broader basis of title to sue because of a quantum argument advanced by the Defendant by reference to the measure of indemnity under the Marine Policy. The Marine Policy provided for the measure of indemnity to be cost, insurance and freight plus 10%. There was a Special Replacement Clause which provided cover for the cost of repairing or replacing the damaged property "subject to Underwriters' prior agreement". Mr Blackwood submitted that no such prior agreement had been given by the Defendant to the repair and of the cranked tubes. During the hearing I allowed amendments to be made to enable the Claimants to advance a case that:

(1) the Defendant had given agreement to the repair and replacement, or was estopped from denying it had done so, by reason agreement or non objection by its loss adjuster, Mr Levitt; and/or

(2) the cost of repair and replacement was recoverable as a sue and labour cost in avoiding the much greater loss which would have arisen under the Delay in Start Up section of the Marine Policy had the decision been taken to secure newly manufactured economiser block by Vulcan in Romania.

38. In the light of these amendments and the evidence of Mr Levitt which essentially confirmed that he had given his agreement to the repair plan as the most sensible economic course to avoid a greater claim for delay in start up, Mr Blackwood did not pursue his objection to the repair and replacement costs as the measure of claim recoverable by EAR. The sue and labour basis of claim also removed an issue which had previously been identified in relation to the application of deductibles.

39. At the conclusion of the hearing therefore, there remained three issues:

(1) Was the cause of the damage resonant vibration during transit or by wind excitation after arrival on site?

(2) If resonant vibration during transit, was an additional cause inherent vice so as to exclude the Defendant's liability?

(3) Did the assured suffer insured losses of at least £4.6 million, and if not, how much?

The witnesses

40. The witnesses of fact from whom I heard evidence were Mr Barber for the Claimants and Mr Levitt for the Defendant. There was little in their evidence which was the subject matter of serious dispute and they both gave evidence in a straightforward and helpful way. I also read statements from Mr Goodman for the Claimants and Mr Rowbotham for the Defendants, which were uncontroversial.

41. Evidence from expert witnesses covered three areas. On issues of metallurgy and welding I heard from Dr Allen and Professor Burdekin for the Claimants and Mr Hughes for the Defendant. On road and sea transportation issues I heard evidence from Mr Anderson for the Claimants and Dr Hunt for the Defendant. On wind excitation the evidence was given by Mr Mackenzie for the Claimants and Dr Hunt for the Defendant.

42. Dr Allen has worked in the field of metallurgical failure investigation since gaining his PhD in metallurgy in 1989. Since 2006 he has been employed as a Senior Engineer with Capcis specialising in metallurgical failure investigations across a wide range of industries. I found Dr Allen's evidence to be well reasoned and helpful.

43. Professor Burdekin was appointed as a Professor of Civil and Structural Engineering at University of Manchester Institute of Science and Technology in 1977 following a period of 7 years in industrial research and 8 years with professional consulting engineers responsible for inspection of major welded structures. He retired from UMIST in 2002 and is now an Emeritus Professor of the University of Manchester and remains active in

consultancy and technical work. His particular area of expertise is in welded design and the application of fracture mechanics to fracture and fatigue failure and implications for safety, reliability and non destructive testing. He has published widely in this area. He played a major part in the preparation of the original British Standards Guidance Document on Acceptable Standards of Flaws in Welds (PD6493) and remained on the British Standards Committee responsible for converting this document to the current standard BS7910 until his retirement in 2002. He was an impressive witness in a field in which he had extensive experience and expertise and his evidence inspired confidence.

44. Mr Hughes is a Chartered Engineer and Member of the Institute of Materials whose career as a metallurgist, most recently with his own consultancy firm, has been predominantly concerned with marine structural and engineering failures. Although prepared to venture opinions on fracture mechanics, flaw assessment, weld assessment and the application of BS 7910, and to criticise Professor Burdekin's views in these areas, he confirmed in cross examination that they were outside his expertise and that he had not read some relevant parts of BS 7910 upon which Professor Burdekin had relied in his written reports. His evidence at times lacked consistency. For example at paragraph 5.6.9 of his first report he considered that it would be appropriate to consider the welds as Class G or worse for the purposes of BS7608, but subsequently changed his mind in paragraph 2.1.18 of his second report. There was no cogent explanation for this change of view. In his evidence he remained critical of Professor Burdekin for treating the welds as Class G. He said that he on the basis that he was not able to agree with that treatment because he had "not seen evidence to say it is correct to do that". He was at times inclined to align himself too closely with the Defendant's case and not prepared to concede ground where the science suggested that he was mistaken or where he lacked the necessary expertise.

45. Mr Anderson is a Chartered Engineer and Member of the Institution of Mechanical Engineers with experience as a stress engineer at British Aerospace and for over 20 years as a consultant involved with design, stress analysis and investigation into fatigue and fracture failure of industrial structures including their transportation by road and sea. He gave evidence which impressed me as fair minded and clear, albeit subject to the limitations, which he acknowledged, in the modelling and data upon which he based his views.

46. Mr MacKenzie is a chartered structural engineer and a Fellow of the Institution of Structural Engineers. He has long experience as an independent consultant with the design and assessment of building structures such as bridges, tall masts, and steel lattice towers, with particular emphasis on the aerodynamic effects of wind and the dynamic response of structures to wind. His opinions were clearly expressed and appeared generally to be well supported by the material upon which he relied.

47. Dr Hunt is a Member of the Institution of Mechanical Engineers and Senior Lecturer in Mechanics at the University of Cambridge where he teaches courses in Dynamics and Vibration. His doctoral thesis in 1988 was on the subject of traffic induced ground vibration and the essential content was published in two papers in the Journal of Sound and Vibration. He has acted as a consultant on a wide variety of engineering topics. His curriculum vitae describes the best part of his research career as having been occupied with vibration from railways. He had a teacher's gift for explaining science in terms which were readily comprehensible to the layman. However his evidence suffered from a number of disadvantages. His theory of how the fatigue cracking occurred through wind

excitation after arrival on site has changed. In his first report Dr. Hunt suggested that the fatigue cracking occurred because the wind caused a phenomenon known as "fluid elastic instability". This theory was dismissed by Mr. MacKenzie, and Dr Hunt now accepts that fluid elastic instability was not the cause of the fatigue cracking. Instead Dr Hunt's theory advanced in his subsequent report and at trial was that the fatigue cracking was caused by vibration induced by gust buffeting wind turbulence. He is not a specialist wind engineer and in this field he had much less experience than Mr MacKenzie. His reports contained several mathematical errors of sufficient significance to affect the conclusions they were said to support. He was inclined to be combative and dogmatic even in areas outside his particular expertise, such as welding. I have no doubt of his sincerity in believing the cause of the cracking to be wind excitation and not transport vibration, but I gained the impression that his certainty of this conclusion sometimes impeded his ability to make a balanced assessment of the available data and supporting science.

Fatigue cracking

48. Fatigue failure occurs when a crack grows incrementally under the action of fluctuating loads and stresses. The fracture surface of a fatigue crack is smooth but may have tiny 'striation' markings. These are invisible to the naked eye and are produced on the fracture surface as the fatigue crack propagates through the material. They occur at the successive positions of the crack front and so their spacing indicates the rate of crack growth with each cycle of loading. Striation markings therefore make it possible to estimate the number of cycles of loading required for a crack to grow. This in turn can be used to estimate the magnitude of the stress ranges which must have occurred to cause the crack.

49. Vibration in a structure is described by its natural frequencies and mode shapes. A structure can have several 'modes' of vibration, each characterised by a natural frequency. The natural frequency of a structure is that at which it will naturally vibrate. Structures can have multiple natural frequencies, the values of which depend on the structure's mass and stiffness distributions and the way in which the structure is supported. If the input forcing vibration is the same or similar frequency as the natural frequency of the structure (or a multiple thereof) the phenomenon of resonance occurs in which the magnitude of the vibrations builds up rapidly.

50. In this case we are concerned with two modes of vibration in the cranked tubes. The "swinging mode" is the expression used to describe the vibration mode in which the tubes sway from side to side or up and down (depending on the orientation of the cranked tubes) when fixed in a single position, at the upper tubular header. The swinging mode for the tubes in the blocks occurs at a natural frequency of about 1Hz. The "washing line mode" is the expression used to describe the vibration mode in which the tubes are supported at each end. In this mode the tubes move from the straight position to form an arch. The washing line mode for the tubes in the blocks occurs at a natural frequency of about 5Hz.

Cycles to failure and stress range

51. There are two elements which are of importance in informing the analysis of whether vibration during transport or by wind was causative of the damage in this case. The first is the determination of how many cycles of resonant vibration occurred to cause the fracturing observed in the tubes. The second is the determination of how much stress is required to cause fatigue failure over at that number of cycles.

52. As to the first, the experts agreed the following:

    (1) The fatigue striations which were observed in the Scanning Electron Microscope investigations carried out at Cambridge University Engineering Department and at Capcis were at a spacing of between 0.1 and 0.3 microns.

    (2) For a crack propagation length of the thickness of the tube, 4.2mm, these spacings "indicate a total number of cycles to grow a fatigue crack through the wall of the tube of the order of between 15,000 and 50,000."

53. Some of the cracks were observed to be not merely through the thickness of the tubes but extending some way round the circumference of the tube. The evidence of Professor Burdekin and Mr Anderson was that once the tube wall is breached, the fatigue cracking round the circumference occurs much more quickly. Professor Burdekin estimated a maximum of about 70,000 to 80,000 cycles for the tubes with such cracking. Dr Hunt estimated that extended cracks may have been subjected to as many as 200,000 cycles. I prefer the evidence of Professor Burdekin who had the expertise in this area which Dr Hunt lacked. Professor Burdekin was not asked to estimate a minimum number of cycles for such cracking. I infer from his evidence on the maximum number, and the agreed lower end of the estimate for tube thickness cracking, that it would be of the order of 20,000 cycles.

54. As to the stress range necessary to cause failure over that number of cycles, Professor Burdekin used two methods to calculate the stress range which would have been required to cause the cracks in way of the overhead welds.

    (1) First, he estimated the crack growth behaviour for an initial crack at the toe of the weld for different stress ranges using the TWI software 'Crackwise' which implements the methods set out in BS 7910. This calculation shows that assuming there was a an initial crack or imperfection in the weld of 0.2mm, a stress range in the region of 300 MegaPascals (MPa) would be expected to cause a crack through the thickness of the tubes in 30,500 cycles and a stress range in the region of 200 MPa would do so in 98,500 cycles. If there were an initial crack or weld defect of 0.3mm then a stress range in the region of 300 MPa would be expected to cause a crack through the thickness of the tubes in 26,800 cycles.

    (2) Secondly, he derived guidance from British Standard, BS 7608: 1993 Code of Practice for Fatigue Design and Assessment of Steel Structures. This is, as its name suggests, a design standard and contains a summary of design and "Mean Line S-N Curves" for different types of welded joints. These S-N Curves are graphs which plot stress range versus cyclic life and are based on experimental tests. They show how many cycles a particular type of weld will, on average, withstand before it cracks by reference to the stress range to which it is exposed.

He estimated that a stress range of 305MPa would have been required to cause failure over 20,000 cycles; 225MPa would have been required to cause failure over 50,000 cycles; and 180MPa would have been required to cause failure over 100,000 cycles. The relevant curve for a weld in a tube to tube connection is identified as Class F. Professor Burdekin, however, based his estimate on the assumption that the welds were equivalent to class G welds because of the imperfections in the overhead welds. He explained that this involved an assumption that the imperfections at the toe of the weld were 0.5mm deep, which was in fact worse than the than the typical imperfections which were found. It follows that the S-N Curve for class G welds may underestimate the stress range necessary to cause the failure in the crank tubes. On the other hand it is a standard deviation curve, so it would predict a statistical probability of a 50% failure rate at those stresses, with a probability of some failure rate at lower stresses.

55. Mr. Hughes and Dr. Hunt disagree with Professor Burdekin's calculations and suggest that a lower stress range would have caused the cracks. They did not, however, identify what that lesser stress range was.

56. I prefer the evidence of Professor Burdekin. This is an area in which he has particular expertise. He sat on the British Standards Committee which drew up BS7910 by reference to which he justified a number of his conclusions. By contrast in the course of cross examination Mr. Hughes confirmed that fracture mechanics, flaw assessment, weld assessment and the application of BS 7910 were outside his expertise; and Dr. Hunt confirmed that he was not a specialist in welding or fatigue analysis.

57. Professor Burdekin dealt convincingly with the criticisms made by Mr Hughes and Dr Hunt of his use of BS 7608. Mr Hughes' criticism was that the overhead welds should not be "downgraded" to Class G because the classes in BS 7608 are based on weld geometry and BS 7608 makes no provision for changing class. He had in fact treated the welds as Class G in his first report. His subsequent criticism misses the point. Professor Burdekin has the expertise to make the judgment as to how the S-N Curves can be applied where there are weld imperfections. The fact that BS 7608 does not address such a question does not undermine his conclusions.

58. Dr. Hunt's view was that BS 7608 could not validly be used for the purpose of assessing the fatigue performance of the welds in the cranked tubes because of the presence of other residual stresses (which he called non zero mean stresses). These were said to comprise two elements:

    (1) The requirement to lift up the cranked tubes in order to weld them to the tubular headers and then lower them resulted in a tensile stress in the overhead welds of 50 MPa (the "jacking up residual stress").

    (2) The cranked tubes were welded in the horizontal position and would have sagged during welding, as a result of gravity. When put in the vertical position the relief of gravity caused a hogging bending moment which would have resulted in a further tensile stress of 40 MPa (the "gravity induced residual stress")

59. Professor Burdekin explained that residual stresses are stresses locked into a structure as a result of the manufacturing process. They are caused by conditions that exceed the yield strength and cause plastic deformation in a local part of the structure whilst the surrounding material remains elastic. Welding inevitably causes residual stresses of yield strength magnitude due to the significant changes in temperature during the welding

process and differential expansion/contraction during heating and cooling of the weld. Residual stresses, once established, do not fluctuate and do not therefore increase the stress range. Residual stresses increase the minimum to maximum stress ratio which has to be taken into account when considering the fatigue performance of a weld. This is taken into account in BS 7608 because BS 7608 assumes that the residual stresses in welds are at yield strength magnitude. This means that other residual stresses which occur during fabrication or installation will make little or no difference to the fatigue behaviour of the welds. This is confirmed in Annex A of BS 7608.

60. Dr. Hunt also relied on clause 4.1 of BS 7608 as rendering that standard inapplicable because of the residual stresses. Clause 4.1 states that

> *'The procedure in this code for deriving fatigue stresses should only be considered as valid provided that the calculated maximum fibre stress of the net area of a member, remote from geometric stress concentrations and* ***excluding self-regulating stresses (such as residual or thermal stresses)*** *does not exceed 60% of yield stress under normal operating conditions" and "80% of yield stress under* extreme *loading conditions…." [Emphasis added]*

61. Dr. Hunt has not applied the principles set out in clause 4.1 correctly because he wrongly includes the jacking up residual stress in his calculation of the maximum fibre stress. I accept Professor Burdekin's evidence that the jacking up stress should not be included in the calculation of the maximum fibre stress because it is a "self-regulating" stress within the meaning of the British Standard.

62. My conclusion from the metallurgy evidence on fatigue cycles and stress ranges is that a stress range of something of the order of about 200 MPa would need to have been applied to the crank tubes to have caused the cracking which they suffered. This figure is however only an approximation because the calculations are hedged about by a number of uncertainties. In particular:

    (1) The number of cycles to failure was derived from an examination of the striations on only six of the tubes (two at Cambridge and four at Capcis) and was within a range differing by a factor of three.

    (2) The experts' agreement as to the number of cycles to be derived from this was expressed as "of the order of" the figures identified.

    (3) The Crackwise figures are sensitive to the size of the weld imperfections as to which the evidence is exiguous.

    (4) Using BS 7608 in the way described means extrapolating the Curves from the graph shown at Table 8 because the graph starts with 100,000 cycles. Whilst Professor Burdekin regarded this as legitimate at the number of cycles being considered (down to 15,000 cycles) it was agreed that at a low number of cycles the extrapolation became more uncertain.

    (5) Treating the welds as equivalent to Class G can only be an approximation.

(6) The Curves are standard deviation curves reflecting an average which may have been deviated from in any given case.

63. For the purposes of assessing the rival theories for the cause of the damage, I therefore consider that the range of cycles suffered by the tubes which failed was of the order of minimum 20,000 and maximum 80,000; and that the stress range was of the order of a minimum of about 200 MPa. Both these sets of figures are subject to a certain degree of imprecision.

Wind

64. Mr. MacKenzie's opinion, expressed in his reports and maintained in his evidence, was that resonant vibration by wind buffeting, sufficient to have caused the fatigue cracking, could be ruled out. This was essentially for two reasons. First, the amount and duration of wind loading to which the tubes were exposed on site could not have produced the stress ranges necessary to have caused the fatigue cracking over the number of cycles agreed by the metallurgists. Secondly, the pattern of damage to the blocks of tubes was inconsistent with the cracking resulting from wind induced vibration.

65. As to the first his conclusions were supported by the following:

(1) The wind speed data came from records of wind at Heathrow, which was close to the plant. What was recorded was, for each hour, a mean wind speed and direction and a maximum gust speed and direction. This gust speed and direction would only have occurred for some part of the time during the relevant hour (being a gust or gusts) and might therefore represent gusting for a matter of only minutes or seconds during that period.

(2) M MacKenzie calculated that resonance to fatigue failure would require gust buffeting with a wind speed of 20m/s for 60,937 hours, which is the equivalent of about 6.9 years; whereas the economisers were only exposed on site for a matter of months; and the greatest gust speed recorded was 22.5m/s with speeds of over 20 m/s rarely recorded. Moreover, wind speeds on the east side of the building where the majority of the cracks and crack like indications were observed (Boiler 1) were greater than 13 m/s for less than an hour. At 1.01 Hz this would cause at most only about 3,500 cycles, which was insufficient to account for the observed fatigue cracking. Accordingly even assuming full exposure to the wind of a force recorded at Heathrow, there were simply insufficiently strong winds by an enormous margin.

(3) The tubes were not, however fully exposed to the wind. Each block was protected by another block on two sides. The inner blocks were protected by the outer blocks. Within each block, the inner tubes were protected by the outer tubes in the block. For most of the period during which the economisers were installed on site, the blocks benefitted from considerable protection from the wind by reason of the partially completed roof, the tipping hall, the accommodation block and the fire wall. Since wind load is proportional to the square of wind speed, any reduction of wind speed by protection from, or deflection off, other structures

would very significantly reduce the ability of the wind to impose relevant wind loading on the economiser blocks.

(4) The headers are physically unable to move to the extent required to produce the observed damage. The banks have alternate lengths of tubes, each row attached to a particular header being slightly longer or shorter than its neighbour. Mr MacKenzie calculated that the longer tubes and shorter tubes will swing with slightly differing frequencies. They will not therefore swing in synchronisation; they will hit each other if they swing further than the distance between them. The gap between them is such that the maximum distance any tube can swing is 44.4mm. In order for the vibration to be of the order calculated by Dr Hunt as sufficient to give rise to fatigue failure, however, the tubes would have to swing +/- 175 mm. This is physically impossible because of the physical restraint of contact with the adjoining row of tubes. Moreover it would have made a considerable noise which is unlikely to have gone unnoticed; and the very fact of collision would have caused significant damping to occur, thereby causing vibration to stop almost as soon as it started.

66. As to the pattern of damage to the economiser blocks he regarded the damage as inconsistent with wind induced damage because:

(1) The wind speeds and directions were plotted in his second report, showing that the winds were predominantly from the west and south western side of the building and were of a significantly higher speed and longer duration from that direction.

(2) As wind loading and response is proportional to the wind velocity (whether or not the wind is laminar or turbulent), greater damage should have been observed in the economiser blocks to the west (Boiler 2) especially as the eastern blocks (Boiler 1) were shielded by the tipping hall, the office accommodation and the refuse bunker as well as the various platforms and other structures in the corridor between the two boilers. Further, there should have been no damage in the inner blocks where the wind speeds would have been especially low.

(3) The pattern of damage however was inconsistent because it showed the following:

(a) The damage occurred to tubes within the blocks rather than in a pattern in which the outer row most exposed to the wind suffered damage; whereas the effect of gust buffeting would be most significant in the outer row of tubes which would feel the brunt of the wind.

(b) Boiler 2, which was most exposed to west and south westerly winds, was found to have markedly fewer cracks and crack like indications than Boiler 1.

(c) The four outer economisers on Boiler 2 most exposed to the wind only had three cracked tubes between them.

(d) Most damage was detected to the blocks in Boiler 1 to the south east.

(e) Damage was found in the inner blocks of both Boilers;

67. I found Mr MacKenzie's evidence to be cogent and convincing. By contrast the criticisms of it by Dr Hunt, who had much less experience and expertise in this field, did not seem to me to go very far to undermine these conclusions or the foundations on which they were built. The main criticism was that Mr MacKenzie's theoretical approach, without producing any model and subjecting it to wind tunnel testing, was flawed and could not underpin any safe conclusions; whereas by contrast, it was said, Dr Hunt's approach of modelling was more secure, despite some of its limitations. Mr MacKenzie maintained that he regularly carried out gust buffeting analyses for structures in turbulent wind on a numerical basis, although that did not enable the specifics of every detail of a structure in every single beam and element and tube to be "modelled" which is why for large design projects such as bridge building a physical model is produced. He did not accept that his conclusions were undermined or invalidated by his not having carried out physical modelling. He subjected Dr Hunt's model and the conclusions purportedly drawn from it to sustained criticism which I address below.

68. As mentioned above, Dr. Hunt's first theory as to wind excitation was that it occurred by fluid elastic instability. He abandoned this theory. The change of view was as a result of building a model, which came to be known as the "936 Tube Model", at which he blew air at from a hairdryer and a vacuum cleaner. He drew his conclusions from the behaviour of this model and calculations from scaling it up. The essential steps were these:

(1) He used finite element analysis to model the banks of tubes, including cranked and straight tubes, welded at the upper header. In his initial report he calculated that the natural or resonant frequencies of the tubes swinging en bloc in rows is a narrowband resonant frequency of 1.6Hz in the swinging mode, which is the relevant mode for these purposes. The figure contained mathematical errors and should have been 1.01 Hz, as Mr MacKenzie pointed out and Dr Hunt accepted.

(2) He then considered the wind speeds needed to excite the tubes at those frequencies using results from his 936 Tube Model and scaling up the results using a reduced velocity equation. His results were that if a mean wind speed of 1m/s excited vibration on his model, it is to be expected "all other things being equal" that wind at a mean speed of 3.8 m/s will excite vibration in the economisers; if at 2 m/s in the model then at 7.6 m/s for the economisers; and if 3 m/s for the model, then at 13.3 m/s for the economisers. He had to revise these figures due to mathematical errors so as to become:

Model	Economisers
1 m/s	2.41 m/s
2 m/s	4.82 m/s
5.5 m/s	13.3 m/s

(3) Winds of 2 m/s were found by experiment to have excited vibration on the 936 Tube Model. Accordingly he concluded that the wind need only have been 4.82 m/s for the tubes in the economisers to have vibrated. If 20,000 cycles were needed to reach failure, that would have occurred in 5.5 hours. If 80,000 were needed that would need 22 hours. The wind data had gust speeds in excess of

those figures (both in force and duration) whilst the economisers were on site and exposed to such winds.

(4) In order to determine whether the vibration would have been of sufficient amplitude to cause the cracking, by scaling up amplitude of vibration in the 936 Tube Model, Dr Hunt calculated that the tubes in the economiser blocks could be expected to be displaced by +/-52mm in winds of 13.3m/s.

(5) This would not be sufficient to cause the fatigue cracking. However he then assumed that the economiser blocks had a much lower damping ratio than the model. He had used a damping ratio of 0.02 in his initial calculations which only gave an expected displacement of +/- 52 mm. He now took a damping ratio of 0.0024, being about 10 times less, despite having taken 0.2 as the relevant figure in his first report when promulgating the theory of damage by fluid elastic instability. Applying the new damping factor of 0.0024 he calculated that a displacement of ±433mm could be expected, albeit that it could not happen because the tubes would collide. Displacement of ±433mm with stresses of 364MPa is larger than would be needed to cause the failure.

(6) He performed two calculations against which to check these displacement results, based on the Blevins formulae and on BS 8100-1. He found a "very strong correlation" between the results of the calculations he carried out which predicted displacements of +/-84mm and +/- 86mm respectively which gave him confidence that the assumptions he had made completing those calculations were realistic. The lower figures predicted by Blevins and BS 8100-1 were explained by the fact that these calculations would produce an under- estimate of the actual amplitude.

69. Mr MacKenzie advanced the following convincing arguments for undermining this conclusion and the steps in the reasoning:

(1) The 936 Tube Model is not an accurate aerodynamic model of the economiser blocks and cannot therefore be used to predict the behaviour of the economiser blocks in wind. In particular:

(a) The 936 Tube Model is too small.

(b) The lower headers (which are the important part of the structure for Dr Hunt's analysis) have not been modelled correctly because (i) they are square and not curved; (ii) the rods protrude out of the lower headers; and (iii) the distance between the headers on the long and short tubes has not been correctly modelled. A further difference between the model and the economisers is that in the model the "tubes" were solid.

(c) the cranks were in the wrong place.

(2) The application of the wind to the 936 Tube Model, and Dr Hunt's calculations therefrom, took no account of the fact that each of the economiser blocks would have been shielded on at least two sides.

(3) One could in fact tell that the 936 Tube Model has been modelled in such a way that it does not respond to vibration in the way that it is known that the

economisers will respond to vibration. This is shown by the fact that when wind is blown on lower headers in the 936 Tube Model there is no response in the in-wind direction. The tubes only swing in a direction perpendicular to the wind. This is significant because if there is an in wind response as well as a cross wind response, the effect is to dissipate energy.

(4) The model has not been scaled properly. In particular, Mr. MacKenzie pointed out that Dr. Hunt had incorrectly calculated the Reynold's number by using the length of the tube rather than the diameter of the lower header. The Reynolds number is a measure of whether flow round an object has been dominated by inertial forces or by viscous forces. It is calculated by multiplying the velocity by the characteristic length scale and then by dividing the product by the kinematic velocity. The higher the Reynolds number, the more flow will be dominated by inertial forces and the lower the Reynolds number the more flow will be dominated by viscous forces. Mr Mackenzie's opinion was that the characteristic length scale was the diameter of the tube and not its length, which was what Dr Hunt had used, and which gave a much lower Reynolds number which brought scaling issues into play. Mr Mackenzie's evidence was that the Reynolds number was in the hundreds of thousands, a figure at which scaling effects start to become significant. Dr. Hunt disagreed that he had incorrectly calculated the Reynold's number, but I prefer Mr. MacKenzie's evidence on this issue, which is supported by the formula set out in BS 8100.

(5) The turbulence was not scale modelled. The use of a hair dryer and vacuum cleaner involved applying wind which did not replicate or model the turbulence which would have been present on site; and the scale of the turbulence in the wind applied to the model was not calibrated or quantified. Therefore, as Mr MacKenzie put it "*We have no idea what the scale of turbulence is, we have no idea if it is representative of the atmospheric turbulence. What we're effectively doing is we are scaling results from something we don't know what the load is*." Since the input to the model was not properly calibrated, the scaling up exercise was meaningless.

(6) Dr Hunt erroneously calculated the fatigue life of a straight tube rather than a cranked tube and based his thesis on this error.

(7) In cross examination Dr Hunt agreed that his model did not show that the economisers were excited by turbulent wind. His evidence included the following passages:

> *Q: What I'm exploring with you is whether your model supports your alternative suggestion that it was caused by turbulent wind.*
>
> *A…….I am certainly not saying that it is a correct and proper model because you have pointed out some of its weaknesses. Just because I see a field full of brown cows doesn't mean there aren't any white cows.*
>
> *Q No and just because your model, which doesn't represent the economisers properly sways or shows vibration in a hairdryer flow doesn't mean the economisers vibrated in wind.*
>
> *A. And it doesn't mean they didn't."*
>
> *……………………………*

> *A. …………[the model] is small and it gives some guidance and it doesn't rule out at all the possibility of turbulent wind excitation. The very fact that we see turbulent wind excitation [in the model] means that the possibility remains.*"

(8) The calculations which Dr. Hunt carried out to support the results of the 936 Tube Model provide no such support. The Blevins and BS 8100 calculations carried out by Dr. Hunt provided no support. Blevins was not an appropriate calculation to use, and his calculation was affected by mathematical errors which when corrected did not support the conclusion. Insofar as his BS 8100-1 calculation was concerned, Dr Hunt agreed with the criticisms made by Mr. MacKenzie of that calculation and described it as "pretty weak".

70. As to the pattern of damage, Dr Hunt said:

(1) The fact that the damage pattern did not apparently correlate with the winds experienced on site could be explained because:

(a) the amplitude of vibration caused by turbulent wind is not dependent upon the direction of the wind; and

(b) none of the tubes in the blocks were sheltered from wind because of the turbulent nature of the wind which would have been travelling around, over and under the blocks.

(2) The damage in the inner blocks could be explained because turbulent wind would have entered the corridor between the boilers and would have excited rows of tubes which meant that the tubes inside the inner blocks could be excited.

(3) The greater damage in the blocks in Boiler 1 could be explained by the fact that these blocks were subjected to a significant period of windy weather in September 2007 before the blocks in Boiler 2 were installed.

71. These points, too, were convincingly refuted by Mr MacKenzie. In particular:

(1) Turbulent wind load is proportional to the square of its velocity. Accordingly any deflection to change the direction of the wind and create turbulence in a deflected location within the structure has to take account of the fact that it will have significantly greater load at the location prior to deflection. The wind direction is therefore a good determinant of where the predominance of damage should appear.

(2) As Mr. MacKenzie explained during his cross examination the wind was going to be highly accelerated as it went over the top of the ramp of the tipping hall roof, and would mostly go over the top of the economisers. Any eddies which came off the roof were going to be spinning off. As he put it: "*To suppose that the air coming over the top is going to dive down, then dive around and conveniently dive round and hit these economisers, and shake them sideways, I think is completely beyond the realms of possibility*.'

(3) Most damage was observed in Blocks 1 and 3 in Boiler 1. If that damage had been caused by turbulent wind from the corridor it would have had to pass under Blocks 2 and 4 before it reached Blocks 1 and 3. Blocks 2 and 4 did not have the same level of damage.

72. I have not overlooked the argument advanced on behalf of the Defendant that Mr. MacKenzie's conclusions based on the pattern of damage are undermined by Professor Burdekin's evidence. Although it was the Defendant's case, put to Professor Burdekin in cross examination, that there was greater cracking in Boiler 1 than Boiler 2, Professor Burdekin did not accept that the actual damage (in the sense of through thickness cracking) could be determined to be greater in Boiler 1 than Boiler 2 because crack like indications might or might not be cracks. Professor Burdekin explained at that it was only the blocks of Boiler 1 which were pressure tested; and the MPI testing that was undertaken on Boiler 2 was "pretty sporadic". This evidence was put in cross examination to Mr MacKenzie, who maintained his opinions, by reference to the agreed chart, if one took into account the pattern of crack like indications as well as cracks.

73. Nor have I overlooked the Defendant's arguments that there are other reasons why Boiler 1 Blocks 1 and 3 might have been more heavily affected by the wind, including in particular that:

(1) The blocks of Boiler 1 were hung between 25 and 26 September 2007, exposing the blocks of Boiler 1 to a full month of wind additional to that to which Boiler 2 was exposed, and potentially to the very windy day of 24 September 2007. On that day there were recorded between 9 am and 6 pm gusts of over 24 m/s occurring during each hour, with a gust or gusts at one point of as much as 35 m/s. One can only say "potentially" because the construction records identify the economiser blocks in Boiler 1 as being hung on 25 and 26 September (after the severe wind) but the removal of the temporary structure around some part of the economiser as starting on 24 September.

(2) The blocks of Boiler 1 were fabricated first and so may well have been subject to the worst of the welding procedures in the difficult overhead welds which may explain the preponderance of damage in Blocks 1 and 3.

(3) The quality control records at Vulcan show that the blocks of Boiler 2 were endoscope tested and a number of repairs carried out on them which was not something from which the blocks of Boiler 1 benefitted.

74. I do not regard these points as going any significant way to undermine Mr MacKenzie's conclusions. Dr Hunt's wind theory still can not account for the fact or preponderance of the known damage in those blocks, nor the known pattern of damage taking into account the uncertainties of whether there were cracks in tubes which were untested or in which crack like indications were found by MPI testing.

Conclusion on damage by wind excitation on site

75. Ultimately the highest it was put on behalf of the Defendant was that the effect of Dr Hunt's evidence was that there was sufficient cause for concern that, without conducting

extensive physical modelling and wind-tunnel testing, it could not be stated with any certainty that vibration would not have occurred on site as a result of wind excitation. Therefore, it was argued, this is exactly what did occur in circumstances where transit can be discounted as the source of the resonant vibration.

76. I disagree. For the reasons identified above, I accept Mr MacKenzie's evidence that resonant vibration by wind buffeting, sufficient to have caused the fatigue cracking to the tubes which occurred, can effectively be ruled out. As a mechanism to explain the damage I do not regard it as a realistic possibility.

Causation: legal analysis of the correct approach

77. In this case the parties have agreed that the fatigue cracking was caused by resonant vibration. They have also agreed that resonant vibration was a fortuity which was an insured peril under each policy. The question, whose answer determines which insurer is liable, is: when did the resonant vibration causing the fatigue cracking occur? Two rival theories have been advanced, which the parties have agreed exhaust all possibilities. It might, therefore, be thought that at this stage of the analysis, having ruled out the possibility that the fatigue cracking was caused by resonant vibration by wind excitation on site, I should inevitably conclude that the only other possibility canvassed, vibration during transit, must have been what occurred. In the words of Sherlock Holmes in *The Sign of Four*, "*When you have eliminated the impossible, whatever remains, however improbable, is the truth*". The authorities show, however, that this would be an erroneous approach to the issue of causation.

78. In a marine policy of insurance, the insurer is liable for losses which are proximately caused by an insured peril. A proximate cause (of which there may be more than one) is that which is proximate in efficiency; the focus is on the real efficient cause: see e.g. ***Global Maritime Systems Inc v Syarkat Takaful Malaysia Berhad ("The Cendor Mopu")*** [2011] UKSC 5 [2011] 1 Lloyds Rep 560 at paragraphs 19 and 49.

79. Generally where an assured brings a claim on a marine policy, and the assured and insurer each put forward a rival explanation for the cause of a loss, there are three rather than two possible findings the Court may make. It may decide that one or other explanation is the probable cause on the balance of probabilities. Or it may be left in doubt, such that even if it rejects the insurer's explanation, it can not say that the assured's explanation is more probable than any alternative (uninsured) explanation. In those circumstances the assured's claim fails on the burden of proof, as it did in ***Rhesa Shipping Co SA v Edmunds (The Popi M)*** [1985] 1 WLR 948.

80. ***The Popi M*** was, as has often been observed, an unusual case on its facts. In ***Ide v ATB Sales Ltd*** [2008] EWCA Civ 424 Thomas LJ giving the judgment of the Court identified its ratio in the following terms:

> "[3]……..Lord Brandon of Oakbrook, giving the only substantive opinion, described the approach of the trial judge as erroneous by reference to the inappropriateness of applying what was described as the dictum of Mr Sherlock Holmes. First, a trial judge was not compelled to choose between two

theories, where the evidence was unsatisfactory; he could decide the case on the basis that the claimant had not proved his case. Secondly, it was not possible to proceed on the basis of eliminating the impossible and deciding that the remaining explanation, however improbable, must be the cause, unless all the relevant facts were known; that state of affairs did not exist, as the ship had sunk in deep water. Thirdly, the concept of proof on a balance of probabilities had to be applied with common sense. It required a judge, before he found a particular event occurred, to be satisfied on the evidence that it was more likely to have occurred than not. If the judge concluded that the occurrence of an event was extremely improbable, a finding by him that it nevertheless was more likely to have occurred than not did not accord with common sense."

81. Thomas LJ went on to say::

> " [4] The circumstances of the case were, as Bingham J pointed out in his judgment, novel and striking. Some of the features were particular to a proof of loss by perils of the sea under a policy of marine insurance: see the judgment of Colman J in *Glowrange v CGU Insurance* (2001) WL720222. *The Popi M* was a very unusual case and as these two appeals demonstrate, the difficulties identified in that case will not normally arise. In the vast majority of cases where the judge has put before him the issue of causation of a particular event, the parties will put before the judge two or more competing explanations as to how the event occurred, which though they may be uncommon are not improbable. In such cases, it is, as was accepted before us by the appellants a permissible and logical train of reasoning for a judge, having eliminated all of the causes of the loss but one, to ask himself whether, on the balance of probabilities, that one cause was the cause of the event. What is impermissible is for a judge to conclude in the case of a series of improbable causes that the least improbable or least unlikely is nonetheless the cause of the event; such cases are those where there may be very real uncertainty about the relevant factual background (as where a vessel was at the bottom of the sea) or the evidence might be highly unsatisfactory. In that type of case the process of elimination can result in arriving at the least improbable cause and not the probable cause."
>
> "[6] As a matter of common sense it will usually be safe for a judge to conclude, where there are two competing theories before him, neither of which is improbable, that having rejected one it is logical to accept the other as being the cause on the balance of probabilities. It was accepted in the course of argument on behalf of the appellant that as a matter of principle, if there were only three possible causes of an event,

> then it was permissible for a judge to approach the matter by analysing each of those causes. If he ranked those causes in terms of probability and concluded that one was more probable than the others, then, provided that those were the only three possible causes, he was entitled to conclude that the one he considered most probable, was the probable cause of the event, provided it was not improbable."

82. In this case the 50/50 clauses provide that each insurer is to pay 50% if "*after proper investigation it is not possible to ascertain whether the cause of such Damage happened prior to termination of the marine venture or subsequently*" (as per the 50/50 clause in the EAR policy) or "*where it is not possible to establish if the loss or damage was caused before or after arrival of the goods at job site or store or place*" (as per the 50/50 clause in the marine policy). These clauses would be applicable if:
    (1) there was such uncertainty that it is not possible to reach any conclusion as to when the damage occurred; or
    (2) one theory is so improbable that even if the other theory is ruled out, it cannot as a matter of common sense be described as more likely than not to have occurred.

83. Having eliminated the possibility of wind excitation, I must therefore examine the alternative hypothesis of vibration during transit in order to determine whether I can conclude that it is more likely than not to have occurred. If I can not reach such a conclusion, either because the evidence is so unsatisfactory, or because such a conclusion is so improbable that, even though I have ruled out wind excitation, it cannot as a matter of common sense be described as more likely than not to have occurred, I can not treat it as a proximate cause of the damage.

Transport

84. Both Mr Anderson and Dr Hunt addressed the question whether the damage occurred during transport by scientific analysis of the necessary mechanism of vibration. They approached it, however, from different directions. Mr Anderson sought to establish what vibration to the tubes would have been required to cause the cracks, and then to consider whether this level of vibration input during transport was credible. Dr Hunt, on the other hand, built a finite element analysis model seeking to input all relevant data, making assumptions where data was lacking, so as to see whether the response of the model could account for the damage being suffered during transport.

85. Although there was very substantial disagreement between them over many aspects of their analyses, their conclusions were not in fact very different. Mr Anderson concluded that it was credible that damage had occurred by reason of resonant vibration during transport from the Vulcan factory to Constanta. Dr Hunt's conclusion, expressed in his first report, was that "Road transportation is very unlikely to have been responsible for the damage observed at the welds in the economiser blocks." He did not however conclude that it was impossible or that it could be ruled out. His view was that it was less likely than wind excitation on site as a cause of the damage.

86. On behalf of the Defendant it was argued that the evidence in fact established that damage during transport was impossible. The essential elements of the argument were these:

    (1) The modelling carried out by Dr Hunt went further than supporting his stated conclusion that road transportation was very unlikely to have been responsible and in fact rendered that theory impossible; by contrast the flaws in Mr Anderson's modelling destroyed the validity of his conclusion that it was a credible cause.

    (2) The theory requires packing to have been missing to an extent which is not in accordance with the available evidence or probabilities.

    (3) The theory would require longer journeys and rougher road conditions than occurred between the Vulcan factory and the port at Constanta.

    (4) The theory is inconsistent with the evidence of corrosion to the fracture surfaces.

87. I shall address each in turn.

Finite Element Analysis Modelling

88. Dr Hunt sought to use a finite element analysis model to simulate the situation in which the economiser blocks were being transported by using a "half-car" model. He sought to model the vehicle and its inertial forces; the banks of tubes in two dimensions to take account of cross-coupling effects and the inertial forces caused by the weight of an economiser block; the suspension of the trailer; the transport frame; the damping of the tubes; the vehicle suspension; the packing; the pitch of the vehicle; and the effect of wheel based filtering. In a number of respects he had to make assumptions on inadequate data: for example the trailer suspension frequency, or the mass and stiffness of the trailer or towing vehicle. He then sought to subject his model to an input of road roughness for "a very poor road". Dr Hunt's model showed many modes of vibration of the tubes responded to the road roughness input i.e. a broadband resonant response was observed. Dr Hunt calculated the amplitude of vibration which was obtained at around the 5Hz washing line mode which Mr Anderson claimed would have been excited. He then calculated the stresses at the weld joints which would have been felt at those amplitudes of vibration. He found that these were too low to have caused cyclic stress failure in the welds.

89. The Claimants and Mr Anderson had a number of criticisms of Dr Hunt's modelling, just as the Defendant had a number of criticisms of Mr Anderson's.

90. There is a particular difficulty with taking Dr Hunt's modelling as a starting point for an assessment of the likelihood of vibration during transport having caused the damage. This is that his finite element analysis model did not seek to model the case which Mr. Anderson says is the most likely cause of the vibration, i.e. that packing was missing from some of the rows. Dr Hunt has taken an all or nothing approach and modelled the blocks with no central packing throughout. This is significant because it materially affects the conclusions which can be drawn from the model in a number of respects. For example, Dr Hunt's evidence was that the acceleration input from the trailer bed will vary significantly according to the number of tubes which it is assumed are vibrating, due to the vibrating tubes exerting forces back on to the trailer bed and thus reducing the excitation from the trailer bed. Dr Hunt's analysis does not show how the tubes behave if

only a few, as opposed to all, of the tubes are vibrating. Accordingly, Dr Hunt's analysis does not show the stress range which would have been imposed on the tubes if some of the packing was ineffective or missing.

91. Moreover Dr Hunt has not disclosed or provided some of the calculations or analyses which he carried out, and upon which he relied to justify his conclusions from his modelling. By way of example, he did not provide his mesh sensitivity analysis; his time domain analysis, which he says showed the effects of cross-coupling; or his sensitivity analysis of his estimate of the towing vehicle and trailer. Given the errors which have been seen in some of the analyses which Dr. Hunt has disclosed, I am not prepared to assume that his undisclosed analyses, which Mr Anderson has not had the opportunity to check, support his conclusions.

92. I have found it more helpful, therefore, to approach this issue by addressing Mr Anderson's modelling and considering whether and to what extent the criticisms of it are well founded.

93. In summary, the essential elements in Mr Anderson's process of reasoning were as follows:

(1) Mr Anderson used a finite element analysis model to predict the frequency of the washing line mode of vibration in crank tubes which were missing packing at the upper header and central position. He derived a figure of 4.4Hz when modelling a single tube and figures of 4.9Hz and 5.0Hz (for the short and long tubes respectively) when modelling complete tube rows. Since the hypothesis was of some packing missing for parts or the whole of rows, the range of vibration frequency taken for his analysis was 4.4 to 5Hz.

(2) He then modelled input acceleration from the trailer bed to simulate vibration transmitted from the road. Assuming an acceleration input from the trailer bed (power spectral density or "PSD") of $0.03g^2$/Hz and critical damping of 2%, he determined that the root mean squared ("RMS") bending stress in the upper tube would be 111MPa; that the displacement of the tubes would be ±47mm; and that the dynamic displacement of the trailer bed required to cause these displacements and stresses was of the order of a few millimetres. He concluded that all of these were credible.

(3) To convert his 111 MPa of RMS to a peak stress range he used a factor of 2 to convert from stress amplitude to stress range and a factor of $\sqrt{2}$ to convert from RMS stress to peak stress. This gave him a credible peak stress range during transport of 314 MPa.

(4) This was more than sufficient peak stress range to account for the damage. He assumed that 66,667 cycles of random vibration were required to cause a crack in the welds. This was the 50,000 cycle figure taken from the metallurgists' agreement that the number of cycles was "of the order" of 15,000 to 50,000 but adjusted to take account of the agreement between Mr. Anderson and Dr. Hunt that the number of cycles to failure (based on peak stress) should be reduced by 25% on account of the randomness of the road vibration. He identified that on the basis of the S-N mean curve for class G welds in BS 7608, this would equate to a

stress range of 204MP. This is in line with my conclusions above that the minimum stress range necessary to cause the damage was of the order of 200MPa.

(5) Since the hypothesis of acceleration input had given him a stress range (314 MPa) which was greater than that likely to have caused the fatigue cracking (204MPa), he scaled back the necessary acceleration input to create a stress range of 204 MPa. This he calculated to be $0.013g^2$/Hz. This was close to the vibration input at 4.4Hz to 5Hz which Dr Hunt calculated would be expected according to ISO 8608 with a "very poor" road and a trailer suspension of 2Hz.

(6) There were factors in his calculations which meant that one did not need to posit such "very poor road" input. In particular if Mr Anderson adjusted two of his input factors a considerably smaller acceleration input would be needed to explain the damage:

(a) The S-N Curve calculation of stress range necessary to cause failure was a standard deviation curve, that is to say the stress range at which 50% of the welds would be predicted to fail and 50% to survive. If one applied a mean minus one standard deviation curve, that would be equivalent to a failure rate of about one in six, which was consistent with the pattern of damage.

(b) Application of a lower damping factor for the tubes than the 2% initially posited by Mr Anderson in his calculations would result in a lower acceleration input needed to create the same stress range. Dr Hunt had used a damping factor of 0.24%, which Mr Anderson thought was too low; he thought the likely damping factor was some way between his initial 2% and Dr Hunt's 0.2%. For his recalculation he took a figure of 1%.

(7) Applying these two adjustments he calculated the necessary acceleration input as being $0.0048g^2$/Hz, which was well within the range predicted by Dr Hunt in his first report as that which would be produced by a "very poor" road so as to cause vibration at 4.4Hz to 5Hz. The adjusted amplitude of vibration would be ±27mm. This would not be so large as to involve "clanking" with a tube hitting the tube in the adjacent row.

(8) He also used the stress ranges calculated by Dr Hunt to make fatigue calculations to check his conclusions. He had "considerable reservations" about the validity of Dr Hunt's finite element analysis model. But he carried out such fatigue calculations based on the results from Dr. Hunt's analysis and using the mean S-N Curve from BS 7608. He calculated the latter to minus one and minus two standard deviations. He produced figures for trailer suspension frequencies of 2Hz, 3Hz and 4Hz on the basis that he considered that to be the range of trailer suspensions likely to have been applicable. Mr Anderson's calculations show that Dr Hunt's analysis predicts about the right number of cycles to failure, bearing in mind that Dr Hunt's model would underestimate the stress in the welds because it assumed the lower stresses which would result from all of the tubes vibrating. In particular:

(a) At vehicle speeds of 72 km/hr and assuming mean minus one standard deviation, the number of cycles to failure at trailer suspensions of 2Hz, 3Hz, and 4Hz respectively were 137,053 cycles, 106,096 cycles, and 74,974 cycles;

(b) At a vehicle speed of 36km/hr, mean minus one standard deviation and trailer suspension of 2Hz, 3Hz and 4Hz the figures are 233,380 cycles, 126,626 cycles and 91,891 cycles respectively.

(c) At a vehicle speed of 36km/hr, mean minus two standard deviation and trailer suspension of 2Hz, 3Hz and 4Hz the figures are 154,441 cycles, 83,796 cycles and 60,810 cycles respectively.

(9) The references above to “very poor road” have been put in inverted commas because that was how Dr Hunt characterised the road quality which had generated the stress range and frequency data which he had extracted from ISO 8608. However Mr Anderson identified that the data in fact represented the boundary in the Standard between a very poor road and a poor road. If one recalculated the cycles to failure using data which was in the middle of the “very poor road” spectrum (again before correcting for any underestimation in stress resulting from Dr Hunt’s model assuming all the tubes were vibrating) the figures became the following:

(a) At a vehicle speed of 36km/hr, mean minus one standard deviation and trailer suspension of 2Hz, 3Hz and 4Hz the figures are 82,860 cycles, 44,966 cycles and 32,420 cycles respectively;.

(b) At a vehicle speed of 36km/hr, mean minus two standard deviation and trailer suspension of 2Hz, 3Hz and 4Hz the figures are 54,833 cycles, 29,757 cycles and 21,454 cycles respectively.

(10) He concluded, therefore that the fatigue cracking could adequately be explained by the journey between the Vulcan factory and the port of Constanta assuming a speed of 36 km/hr and trailer suspensions in the range of 2Hz to 4Hz. At a higher speed the fatigue stresses would be higher.

94. The main criticisms of Mr Anderson’s modelling analysis were as follows.

Mr Anderson’s method

95. It was said that a proper finite element modelling and analysis of the economisers being carried on a trailer show that, even if packing were missing, there were insufficient stresses generated at the weld joints which cracked for the damage to have occurred during road transport on even the worst of possible road surfaces. This assumes the validity of Dr Hunt’s modelling, which for the reasons I have given, I do not find a secure starting point for any analysis. The calculations by Mr Anderson, set out above, demonstrate that sufficient stresses could be caused. What matters is whether the assumptions and data on which those calculations are based, or the process of reasoning, can be shown to be flawed. It was said that in trying to prove that damage could have occurred during transport, Mr Anderson has started by assuming that damage occurred during road transportation and worked backwards from that conclusion; and that such is not a legitimate method of scientific inquiry. I disagree with that characterisation of his method. He has posited a hypothesis of some missing packing and conducted an analysis

to see whether sufficient vibration could have been excited by road roughness during the relevant period to have caused the damage. Having found that it could have been, he has then subjected the necessary elements of data input into the analysis to an assessment of whether they are credible. He has concluded that they are. He has then cross checked his results against Dr Hunt's model which he concludes supports the conclusion once adjusted for the critical input criteria. If his conclusions are justified by the assumptions which support them, I see nothing objectionable in his method. Again what matters is whether his assumptions and reasoning are flawed so as to invalidate his conclusion.

Simplicity of the model

96. Although many criticisms were advanced and a good deal of time was taken in evidence in exploring various elements of the modelling, it was ultimately accepted by Dr Hunt that many were not significant. In cross examination Dr Hunt identified three elements of critical data which needed to be included. They were (1) vehicle suspension, (2) road roughness input and (3) multiple tubes with packing. Mr Anderson's calculations take account of (1) and (2) albeit in a way which is subject to criticisms which I address below. As to (3) the major area of disagreement was in relation to cross coupling, which Dr Hunt thought would be effective through the packing between the rows of tubes and have a significant damping effect.

Cross coupling damping

97. Mr Anderson was of the view that there would be no significant damping as a result of the packing. Indeed it was put to him in cross examination that it would have no significant effect and he agreed. Mr Anderson's evidence was that his view was justified on the given hypothesis that packing was only missing in the tubes which suffered damage (except for the packing at the lower headers); because the lower header packing was at or close to the nodal point it would not inhibit vibration of the relevant tube. Mr Anderson further supported his view by pointing to the fact that when he looked at the difference between the results of his analysis and that of Dr Hunt, it revealed that both their models gave very similar stress ranges if adjusted to assume the same input vibration. He therefore concluded that the various disagreements between them over packing stiffness, damping, modelling methods and cross coupling were relatively unimportant. The two critical differences related to assessment of the vibration input which was dependent upon the choice of road roughness and trailer suspension damping parameters used. I found this convincing and well supported by Mr Anderson's calculations.

98. Mr. Anderson was criticised for having assumed for these purposes that the packing was of infinite stiffness. But I accept his response that this was a reasonable assumption to make when the real stiffness of the packing was so much greater than that of the tubes. He also re analysed his finite element model with the packing stiffness suggested by Dr. Hunt and with a packing stiffness of one tenth of that suggested by Dr. Hunt. This confirmed that the stiffness of the packing had an insignificant effect on his results.

99. Mr Anderson's theory that the lower packing could have been at the nodal point of the tubes was criticised as being inconsistent with (a) the packing being near the end of the tubes and (b) the packing being in (at least roughly) the same place for the alternate long and short tubes which would have had different nodal points because of their slightly differing natural frequencies. This point has some force. But it does not detract from the

possibility of sufficient vibration in those tubes which suffered damage, given what is known about the pattern of damage; nor does it detract from the force of Mr Anderson's calculations using Dr Hunt's model which suggests that cross coupling is not a critical factor in assessing the validity of Mr Anderson's model.

Deck loading damping

100. Dr Hunt said that Mr. Anderson had used the wrong input excitation to calculate the frequency of the vibrations which could have been experienced by the tubes whilst being transported by road. Dr Hunt's point was that Mr Anderson's input was incorrect because it failed to take account of the resonant characteristics of the tubes which were vibrating, which would have reduced the input acceleration by exerting force back down on the trailer deck surface. Mr Anderson's response, which I accept, is that this would not be significant for the vibration in the small number of tubes which was postulated by the theory of damage during transportation. He accepted that if there were lots of tubes vibrating, then the vibration input at the trailer deck would tend to be reduced, although it would be a complicated response: a large number of tubes vibrating at 5Hz would not simply reduce trailer bed acceleration but would excite vibration in the trailer bed and other tubes. But this would not occur in relation to the small number of tubes postulated as vibrating. He was asked in cross examination about the thesis that the packing missing in Boiler 1 Block 1 might only involve packing being missing in six of the gaps between rows, with blocks of rows in between vibrating en bloc. It was suggested to him that this would result in significant downward force on the trailer bed so as to invalidate his trailer deck acceleration input. He rejected the suggestion as "completely incorrect" and by reference to weight calculations said that there would be no significant effect even with a vibrating block of 200 tubes, which would equate to about 17 rows vibrating together. That was even assuming that all the tubes in a row were vibrating en bloc, whereas the theory postulated was packing being missing only for the cranked tubes in a given row, not the straight tubes. Mr Anderson maintained that one could have the vibration input identified in his modelling whether or not a large number of the tubes were vibrating, and in any event with the limited number of vibrating tubes postulated by the theory of damage during transportation. I found this evidence cogent and convincing.

Tube damping

101. Mr Anderson was criticised for taking a damping factor of 1% in his revised calculations, being between the 0.24% taken by Dr Hunt and the 2% he had initially posited. On behalf of the Defendant it was suggested that Dr Hunt's 2% was only relevant to the tubes for the purpose of the wind excitation theory, with the tubes hung vertically, whereas the position is different for the transportation damage analysis, in which the tubes are horizontal. Mr Anderson responded, quoting Professor Burdekin, that "the economisers don't know whether they're being transported or on site, do they?" His point, which seems to me a good one, was that the damping which is here being addressed is not that through cross coupling or packing, but merely the inherent damping from the material in the tubes, which is the same wherever they are and whether in a vertical or horizontal orientation.

Narrowband Resonant Response

102. The Defendant relied heavily upon the fact that Dr Hunt's model produced a broadband response of vibration whereas the experts agreed that the observed damage was caused by

a narrowband resonant response of the tubes to a source of excitation i.e. cycles of loading which caused repeated vibration of the tubes at the same frequency. Dr Hunt's model did not predict the single washing line vibration mode at about 4.4 to 5 Hz which Mr Anderson's analysis required. The Defendant submitted that Dr Hunt was not challenged on the conclusion that a broadband resonant response would be observed in the tubes; that Mr Anderson's reports contain no criticism of Dr Hunt's findings in this respect; that Mr Anderson has provided no evidence that a narrowband resonant response would be experienced by the tubes in an economiser; that accordingly, the only evidence before the Court is that the vibration or output excitation of the tubes is a broadband response as demonstrated by Dr Hunt; and that dynamics of vibration aside, Dr Hunt's results accord with common sense: where a complex structure comprising vehicle, frame, packing and tubes of different lengths is driven across a non-uniform surface such as a road, the tubes are not all going to respond in the same way at the same frequency absent some very unusual form of frequency filtering or channelling; there will be a multiple frequency or broadband response.

103. Attractively though this argument was put, I can not accept it. It depended upon the validity of Dr Hunt's modelling, and in particular his assumption of cross coupling through the packing between the rows. Once it is recognised, as Mr Anderson explained and I accept, that Mr Anderson's theory assumes missing packing for some only of the rows and that narrowband vibration could be expected in those tubes which were unsupported, I see no reason to accept Dr Hunt's broadband output results based as they are on the cross coupling effects of a different set of assumptions. Mr Anderson accepted that he had applied the input excitation that he used for his model (deck acceleration) in two places (at the upper header and at the lower header packing) and had applied the input "in-phase" i.e. at the same synchronised input, whereas there would be a mixture of in phase bouncing and out of phase pitching mode experienced by the economisers and therefore the tubes during road transit because the road surface was random. However he said he had done some analyses with the inputs out of phase which made, he said, very little difference. I see no reason to reject this evidence. In any event this criticism does not touch Mr Anderson's calculations of peak stress range based on Dr Hunt's modelling with its broadband response.

Trailer Suspension Frequency

104. There is a factual dispute as to what would have been the suspension frequency of the trailers used to transport the economiser blocks. With one exception the make (and therefore characteristics) of the actual trailers used were not capable of being identified. Mr Anderson used a range of frequencies of between 2Hz and 4Hz in his calculations as those he thought most likely, relying on the papers annexed to Dr Hunt's report addressing road damage, which indicated a typical range of between 1.5Hz and 4Hz for vehicles with leaf suspension. In cross examination he accepted 3Hz as a realistic upper limit and accepted it as likely that some of the trailers used had a frequency of less than 2Hz. Dr Hunt's view was that the trailers are likely to have had air suspension i.e. a suspension with a natural frequency of typically around 1Hz and no higher than 2Hz. Kalimbassieris, the surveyors who visited the Vulcan site and observed one particular vehicle loaded with an economiser block, noted the model and specification of the trailer. This trailer had air suspension. Air suspension has been mandatory on heavy vehicles in the UK since 1990 and is in widespread use by vehicle manufacturers. Further, within Member States of the EU since 1992 vehicles have been required to have road-friendly

suspensions, defined as suspensions below 2Hz (Directive 92/7/EEC). A diagram showing sample data from a Goldhofer 4 axle trailer with air suspension allows one to calculate the suspension frequency of that vehicle as being about 1.4Hz.

105. I conclude that most of the trailers involved are likely to have had air suspension and to have had a suspension frequency of no more than 2 Hz; but that I can not rule out the possibility that trailers with suspension frequencies of up to 3Hz were in fact used for some of the economiser blocks. Were one to assume a maximum trailer suspension frequency of 2Hz, this would not of itself be sufficient to invalidate Mr Anderson's conclusions, which calculate sufficient stresses and cycles to failure using a 2Hz assumption.

Amplification Factor

106. The amplification factor is used by Mr Anderson to describe the factor by which the amplitude of vibration of the input excitation can be multiplied in order to determine the amplitude of vibration postulated in the tubes themselves. In Dr Hunt's modelling this is not an input but can be calculated from his results. His results suggest an amplification factor of no more than 3, and usually much lower i.e. for a given displacement of the trailer bed the displacement of the tubes vibrating at a given frequency is no more than 3 times the trailer bed displacement and usually less. It was argued that Mr Anderson's use of an amplification factor of 5 is about double the worst figure that Dr Hunt has calculated such that for the given amplitude of vibration the economiser block would have been noticeably jumping off the trailer and that he has over-estimated the weld stresses in his single tube by at least 50%.

107. This criticism seems to me to be unfounded. Dr Hunt's figures depend upon the validity of his model, which, for the reasons identified, does not seem to me to be a secure starting point for any calculations in relation to the theory under discussion. Mr Anderson's view when challenged on this point in cross examination was that the amplification factors in Dr Hunt's model were heavily affected by the fact that he was modelling the whole bank of tubes in the economiser as vibrating with missing packing for all of them. As he explained: "*the reason why the amplification factor is so low is because all the tubes are vibrating. You are inputting into this huge bank of tubes, 1,000 tubes, in simple terms, energy at the bottom. That energy is trying to vibrate the tubes. That energy is being shared over 1,000 tubes. If some of the tubes were packed and weren't able to vibrate, the energy would seek out those tubes that weren't packed - the cranked tubes where the packing had fallen out - and the amplification factor would be greater.*" Mr Anderson's amplification factor of 5 would not involve trailer bed amplification of more than a few millimetres to account for the vibration amplitude he derives from his calculations.

Peak Stress Calculation

108. This criticism was addressed to Mr. Anderson's calculation of fatigue stress calculations based on Dr Hunt's model (which produced a broadband response) not his primary analysis and calculations based on his own model, which posited a narrowband response. Dr Hunt expressed the view that because for a broadband response there are many stress levels varying randomly it is not possible simply to multiply the RMS stress range by $2\sqrt{2}$ (2.83) when converting from RMS to stress range; that is only appropriate

for narrowband response. Mr Anderson said that he thought it was a reasonable factor to use because, if anything, it would underestimate the RMS peak stress, a point he made good by producing a simple calculation based on Miner's Law which showed the appropriate factor was in fact 2.90. Dr. Hunt said that he was not aware of Miner's law being applied in this way, but did not say that it could not be applied in the way that it was applied by Mr. Anderson. He did not suggest that a lower factor should be used. I prefer Mr Anderson's evidence on this point.

Journey Times and Road Roughness

109. Mr Anderson estimated that the vibration frequency in the washing line mode would be somewhere between 4.4Hz and 5Hz. The times necessary to accumulate the number of cycles at that those frequencies would have been, by my calculation:

(1) For 20,000 cycles, between 67 and 75 minutes;

(2) For 50,000 cycles, between 2 hours 47 minutes and 3 hours 10 minutes;

(3) For 80,000 cycles, between 4 hours 17 minutes and 5 hours 3 minutes.

110. The road journey between the Vulcan factory and the port of Constanta was about 215kms. Mr. Barber obtained a video and commentary of the road conditions on 20 March 2009, and a schedule summarising the road conditions as shown on the video was produced. Neither party invited me to watch the video but I heard evidence from those, including the experts, who had seen it. The driver who provided the commentary on the video confirmed that the road conditions in 2007 were worse than those in 2009 as a number of road improvements had been carried out in the interim.

111. At a realistic average speed for the lorries carrying these wide loads of 30 to 40km/hr, this would have taken about 5 to 7 hours. I accept Mr Anderson's view that it might have taken as much as 8 hours. Of this journey a total of about 65km was on poor urban roads (at each end) and about 150kms on a "motorway" which has been described as reasonably smooth, although it had a rumble strip which might have been regularly driven over because the economisers were particularly wide loads. The speed on the poor urban roads might have been expected to have averaged say 25 km/hr. At that speed the urban road legs would have taken over two and a half hours. That would easily be long enough to allow sufficient cycles to failure. Assuming 20,000 cycles to failure and a frequency of 4.4Hz to 5 Hz, it would require resonant vibration for a little over half the time spent on urban roads.

112. Mr Anderson also took account of the possibility that fatigue cracking had been substantially initiated and progressed during the Bucharest/Constanta leg of the journey, but further propagation took place in the two to three hour road journey at the other end between Southampton and the Lakeside Facility. I agree that this can not be ruled out.

113. As to road roughness, Mr Anderson initially used a NATO Standard which is used for calculations involving military vehicles. His analysis set out above, however, was referenced to ISO 8608 which was the standard used by Dr Hunt. Using that standard his

analysis required road roughness which was on the border between poor and very poor within that Standard.

114. The only evidence about the roughness of the road over which the economisers would have travelled came from the video referred to above, which is of a journey two years later and after what are said to have been improvements. The road surface may in fact have been worse at the time the economisers were transported.

115. Mr. Anderson has concluded that the roads could properly fall within the category "poor" or "very poor" within the meaning of ISO 8608 when one also takes into account the fact that the width of the Economiser Blocks and the narrowness of the urban roads would have meant that the driver would have had to position the wheels outside of the tracks or on the rough edges of the road. The Defendant submitted that one could see from the video stills and the record of the journey that the road was not very rough, particularly from the absence of puddles despite it raining, but I did not find it possible to draw any conclusions to that effect from this material about the state of the road now, still less its roughness at the time the economiser blocks were transported.

116. The Defendant argued that given that the ISO Standard is an international spectrum for road roughness it can be assumed to cover all types of road surfaces, from the best motorways to the worst mountainous dirt tracks, so that the Claimant's theory had to posit, unrealistically it was said, an urban road in Romania being as bad as the worst road or tracks in the world. This struck me as unrealistic and was not supported by the standard itself. It applied to roads.

117. On this exiguous material it seems to me that the road between Bucharest and Constanta may well have exhibited the necessary roughness to have caused the vibration which Mr Anderson's calculations assume.

Missing Packing

118. There are some photographs of blocks taken at various stages, including some taken whilst under construction at Vulcan, some prior to shipment at Vulcan, some when on a trailer in Romania, some when on board a vessel, some prior to dismantling at Lakeside and some during dismantling. These are however a tantalisingly random and incomplete record for the purposes of the inquiry in hand. What can be said is that they provide cogent evidence that some elements of the packing in some economiser blocks was missing during transportation. In particular, photographs of blocks taken at the factory when prepared for shipment, and taken at Lakeside before any dismantling, suggest that blocks were shipped with incomplete packing at least at the headers. What is less clear from the photos is whether central packing was missing, and sufficiently so to account for the extent and pattern of damage having been caused during transportation.

119. I have reached the conclusion that there is credible evidence to suggest that sufficient of the central and upper packing was ineffective and/or missing. My principal reasons are as follows.

120. The packing between any given pair of rows was not made up strips of rubber laid in layers on top of each other. Generally there appear to be about five packing strips per gap; but there appear from the photographs to have been different thicknesses of packing used in different gaps: the packing was not applied evenly across the structure. The gap

between the tubes is generally about 70mm, meaning that each packing strip is about 14mm thick. Mr Anderson's estimate was that each strip was about 15mm thick. His calculations had determined that in order to explain the fatigue cracks the tubes have to move by about ±27mm. It therefore only requires two strips of packing to fall out or be missing for sufficient movement to occur.

121. The photographic evidence suggests that packing strips of different lengths were used; that is to say that the packing in the gap between any given rows did not comprise strips extending the full length of the row, but rather some strips for part of the length of the gap and separate strips for the remainder of the length of the gap. Accordingly if a shorter packing strip fell out or was missing, there would be some of the tubes in a given row supported and some unsupported. The tubes which were left unsupported in a row could vibrate. Moreover Mr Anderson said in his evidence, and I accept, that if packing were missing above and below a small series of rows of tubes, all the tubes in the series could vibrate.

122. There is good reason to think that the manufacturing and packing process left some packing missing at the upper header and central points at the time the economiser blocks left the factory. In particular:

(1) The manufacturing process was conducive to that outcome. There appear from the photographs to have been different thicknesses of packing used as between the gaps between particular rows: the packing was not applied evenly across the structure. The photographs suggest that the packing was inserted between the rows whilst they were being welded to the headers, row by row, in the horizontal position. Because the cranked tubes had to be lifted up to be welded, this would have made it difficult to have single strips running the length of the rows; and would have made it difficult to replace or repair packing after the welding process. Mr. Anderson identified the potential difficulties during the manufacturing process in fitting the packing between the cranked tubes. There is one example of photographic evidence of packing having sheared/collapsed and not being effectively in place during the welding phase of manufacture. Dr. Hunt speculated that the central packing could have been installed after the rows had been constructed by two men, each holding one end of the packing and pulling the packing up the row of tubes as they walked down the side of the economiser blocks. But this could only have occurred if there were full length strips running the length of each row, whereas the photographic evidence and the manufacturing process suggest this was not the case; and in any event it would be difficult to add individual strips making up one or more of the layers of packing in the gap between any given rows.

(2) The whole was kept compressed by a compression bar connected to the frame applying compression to the row of tubes as a whole. If packing were missing or ineffective between particular rows, or tubes in particular rows, it would be difficult to achieve compression across the block to the degree desired. After construction the blocks were turned on end in preparation for shipment. If any packing fell out at that stage through gravity it would be impossible to replace.

(3) There are photographs of a block at the factory in its vertical orientation prior to shipment. There appears to be packing missing quite extensively at the location of

the upper header and packing missing, or having slipped, between some rows at the central location.

(4) There is a photograph of a block on a trailer which appears to be missing packing between a large number of the rows at the upper header location.

(5) There is a photograph of a block at the Lakeside Facility with the clamping bar still in place which appears to show central packing missing between a number of the rows (albeit at the straight tube side of the block).

(6) Many of the photographs of the blocks at Lakeside show central packing missing but these were taken in the course of dismantling, after the clamping bars had been removed, so that they do not provide any direct evidence of missing packing. What they do illustrate, however, is how easily the packing falls out when the economiser blocks are in the vertical orientation unless effectively restrained by the clamping bar.

123. The Defendant sought to rely on a photograph attributed to Boiler 1 Block 3 taken during unpacking on site, as showing that all its central packing was in place, whereas this was one of the blocks in which the largest number of cracked tubes were to be found. This photograph does not however assist the Defendant's case for two reasons. First the attribution of this photograph to Boiler 1 Block 3 is insecure. The series of photographs, whose source is Takuma, have amongst them photographs of a name plate identifying the block number. The Defendant's argument assumed that the photographs which followed the photograph of the name plate were of that block. But it can be determined that some photos of the block which follows such a name plate must be of a different block. The metadata on the disk of photographs with which I was provided suggests that the photographs are a selection of those taken, not a complete and systematic record. Secondly, the photograph shows the side of the block consisting entirely of straight tubes, not the side where the cranked tubes are to be found, where the packing seems to have comprised separate shorter strips unconnected to those shown on the photograph.

124. The Defendant also relied upon an "Economiser Block Delivery Log", appended to Takuma's provisional report on the failures dated 25 February 2008. This suggests that the "package and components" were subjected to visual inspection three times after having been packed: once before departure from Constanta by Insurers and/or Bioener; a second time upon arrival at Southampton by Bioener and/or Vulcan; and finally upon arrival at site by Takuma and/or Vulcan. Mr Barber's Preliminary Report of 2 March 2008 recorded "*Certainly the blocks would have experienced vibration in transit, although the Client's Engineer told us the blocks arrived very carefully packed, and it took a day to remove the packaging from each block*". There is no evidence to suggest that particular attention was paid to the presence or otherwise of the packing strips. On the contrary, the photographic evidence, which demonstrates a more than minimal element of missing packing, suggests that packing between the rows was not the focus of these inspections and the absence of any recorded findings of packing deficiencies is not inconsistent with sufficient missing packing between some of the rows at some points to account for the damage.

125. The Defendant contended that the pattern of damage to the tubes in the blocks was inconsistent with the postulated mechanism for damage occurring during road transportation. Two points were made. First it was suggested that the existence of

damage in particular blocks was inconsistent with photographs of such blocks showing the packing in place. But all the individual points made in support of this contention started from the false premise that one could identify which block was the subject matter of a particular photograph. Secondly it was submitted that the missing packing would have had to be more extensive than was feasible to explain the damage in the most heavily affected (or known to be affected) blocks such as Boiler 1 Block 1. Mr Anderson's response when this was put to him was that one only needed a minimum of 6 gaps in that Block to have missing packing (of at least two strips worth) to account for the damage because of the possibility of a series of rows of tubes vibrating together; and that such was feasibly consistent with the evidence of missing packing. I accept his explanation and agree with his conclusion.

126. The Defendant submitted that Mr Anderson's explanation in this respect involved "walking a tightrope" when placed against two elements of his modelling, namely his justification for using deck acceleration, and his treatment of the effects of cross coupling.

(1) Dr Hunt's criticisms included the point that Mr Anderson's use of deck acceleration ignores the resonant characteristics of the tubes, which will have reduced the input acceleration by exerting force back down on the trailer deck surface. Mr Anderson's response was that he was postulating the movement of single cranked tubes only, or a small number of tubes, and not many tubes en bloc, and so the reaction force would be small. But, it was said, the larger the number of tubes vibrating together, the progressively less valid his use of deck acceleration as the input excitation becomes.

(2) Dr Hunt's evidence was that cross coupling would have a damping effect on the vibrating tubes through the packing between them. Mr Anderson's response was that this would be insignificant, one of the reasons for this being that only a few tubes are vibrating and those would not be coupling with tubes where the packing was intact. If the tubes were vibrating en bloc, there would be tubes vibrating with packing in tact so as to bring cross coupling into play.

127. Both submissions are logically sound but not sufficient to undermine Mr Anderson's evidence on either aspect. He did not accept, and nor do I, that the force exerted down on the trailer deck surface would be significant if there were only six gaps of missing packing in Block 1. Moreover Mr Anderson did not accept, and nor do I, that the evidence of missing packing meant one could rule out the possibility of it being sufficiently extensive to involve sixteen gaps in Block 1, that is missing either side of each of the rows in which cracked tubes were found. So far as cross coupling was concerned, he explained that if packing were missing at only the cranked tubes, not the straight tubes, that would constitute the small number of tubes, even if some were en bloc, which militated against any damping effect of cross coupling. He had also given further reasons for his view that cross coupling was insignificant as a factor creating damping through the packing, which did not depend upon whether the theory postulated packing missing either side of every row in which a damaged tube was found. In particular he said that the insignificance of cross coupling was demonstrated by Dr Hunt's modelling, in which changing that parameter did not make a significant difference; and by the fact that the packing at the lower header is close to the nodal point so that coupling between rows would be small.

128. Mr Anderson was not treading a path as narrow as a tightrope, and his views kept their balance.

Corrosion

129. The Defendant submitted that absence of seawater corrosion to the samples which were inspected after the cracking was discovered necessarily pointed to cracking having occurred after arrival at Lakeside because cracking during the road journey to Constanta would have resulted in salt water corrosion during the sea leg of the voyage.

130. For these purposes it relied upon the following:

(1) The Bureau Veritas Report of 27 October 2008, prepared by Stephen Rowbotham, in which he explained that the fractures visible on the 13th March 2008 *"were bright and exhibited negligible corrosion"*.

(2) The draft E.ON Report of September, 2008 which explained "*Scanning electron microscopy and EDAX analysis did not reveal any unusual elements i.e. evidence of sodium chloride from sea water …*" .

(3) The Force Reports of February, 2008, the first of which recorded that no *"unusual corrosion products"* were detected at the crack surfaces. Mr Hughes explained in his evidence that in his view, had pitting corrosion been present that would have been regarded by Force as unusual corrosion.

(4) Photographs of the two samples which were inspected by Sumitomo and Force.

131. This evidence was very limited. It was explored in cross examination with Dr Allen and Mr Hughes. Mr Hughes was of the view that while the evidence of lack of corrosion was limited, it was *"compelling"*. Dr Allen's conclusion was that there is insufficient metallurgical evidence to draw any reliable conclusions from it as to as to when the damage occurred. I prefer Dr Allen's evidence. The sample is so limited (only two fracture surfaces), and the conclusions to be drawn from it so heavily dependent on imponderables such as the potential width of the crack at the time of shipment, the lighting and quality of the photography and the conditions under which the samples were stored prior to inspection, that it does not advance the arguments for either side in this case.

Conclusion on causation

132. For the reasons set out above, I have concluded that damage during the road transport leg of the journey in Romania, possibly exacerbated by damage during the road leg of the journey in England, is a realistic and credible possibility. It is much more likely than the alternative of damage by wind excitation after arrival, which I have concluded is not a realistic possibility. Although there are gaps in the evidence available, and uncertainties in some of the science which is applicable, this is not a case in which such gaps or uncertainty render it impossible to reach a conclusion on the balance of probabilities. There has been sufficient evidence in the case for the Court to be able to embark upon an informed analysis of the two possible causes of the damage and to reach a reasoned conclusion as to the probable cause. I find on the balance of probabilities that the damage

occurred prior to arrival of the economiser blocks on site as a result of resonant vibration during transportation from the factory in Bucharest.

Inherent Vice

133. The Marine Policy incorporated The Institute Cargo Clauses (A) which expressly exclude inherent vice from coverage. Clause 4.4 provides:

> "*Exclusions*
> *4. In no case shall this insurance cover*
> *4.4 loss damage or expense caused by inherent vice or nature of the subject-matter insured*"

134. The Defendant argued that if, as I have found, resonant vibration during transit was a cause, nevertheless there was an additional proximate cause comprising one or more of (a) the non zero mean stress imparted into the economisers by reason of the jacking up procedure necessary to complete the welds between the cranked tubes and the headers in the overhead position, (b) the quality of the welds themselves and (c) the set on design of the cranked tubes to the headers doing away with the stub pipe which was retained only for the outermost tubes. These, it was said, amounted to inherent vice and excused the Defendant from liability under the principle that where a loss has two proximate causes, one of which is within the policy and the other expressly excluded, the exclusion takes effect to exempt the insurer from liability.

135. I reject this argument for two reasons. First, it is clear that the condition of the economiser blocks when they left the factory was such that they could reasonably be expected to survive the transportation to Lakeside, if properly packed, and perform in service at the site, without fatigue cracking:

(1) Professor Burdekin inspected over 100 of the welds. His evidence, which I accept, was that the quality of the welding was to an acceptable commercial standard.

(2) It was common ground between Professor Burdekin and Mr Hughes that the "set-on" design of welding the cranked tubes directly to the headers without a stub pipe was not of itself objectionable.

(3) It was common ground between the experts that the fatigue cracking could not have occurred during the journey had the packing not been missing;

(4) the maximum stress level to which the cranked tubes would have been subjected in service was 63.5MPa; and the tubes were not expected to be subjected to resonant vibration in service.

136. It was an express term of the Marine Policy that:

> "*In the event of a claim being made for loss or damage which is alleged to be caused by insufficiency or unsuitability of packing or preparation of the subject matter insured, Underwriters hereby agree that they will not use such alleged insufficiency or unsuitability as a defence against the claim in any case where the packing or preparation was carried out*

> *by a party other than the Assured and the insufficiency or unsuitability arose entirely without the Assured's privity or knowledge. …"*

137. There was therefore nothing in the inherent condition or design of the economisers which could be described as a proximate cause of the loss. The case is no different from that of any other potentially vulnerable cargo whose damage during transit is attributable to the inadequacy of its packing, where the insurer has assumed the risk of inadequate packing. The proximate cause of the loss in this case was resonant vibration during transit resulting from the inadequacy of the packing. There was no other proximate cause.

138. Secondly, it is clear from the authorities that where it is established that a proximate cause of the loss is a fortuity occurring during the period of cover, there is no room for inherent vice to be treated as another proximate cause of the loss. This is because a loss by inherent vice is a loss caused by the inherent characteristics of the cargo **not involving any fortuitous external accident or casualty**: see ***Soya GmbH Mainz Kommanditgesellschaft v White*** [1983] 1 Lloyds Rep 122, 126 and ***Global Maritime Systems Inc v Syarkat Takaful Malaysia Berhad ("The Cendor Mopu")*** [2011] UKSC 5, [2011] 1 Lloyds Rep 560 at paragraphs 45-46 and 80-81.

139. In this case the damage which occurred during transportation was proximately caused by resonant vibration which was an external fortuitous accident or casualty. There is therefore no room as a matter of law for inherent vice to be an additional proximate cause.

Quantum

140. The dispute on quantum was a narrow one. The sum claimed by the assured was £5,746,108.08. The claim was settled by EAR Insurers at £4.6 million, of which the Claimants' proportion claimed against the Defendant in these proceedings was £3.68 million. The Claimants must establish that the assured had suffered an insured loss of at least £4.6 million. The Defendant accepts that they have established that the assured had suffered an insured loss to the extent of about £4.3 million.

141. The Defendant relied upon Mr Barber's report to insurers dated 29 June 2009. In that report he described the losses as supported to the extent of £4,309,562.63 and as unsupported to the extent of £999,653.29.

142. A number of items were put in the unsupported category not because they were regarded as unreasonable in amount or irrecoverable in principle, but because the exact amounts had not been vouched. These included:

    (1) £101,304.92 in respect of engineering and inspection costs. Mr Barber identified that this figure came from a spreadsheet and observed that there was no reason to doubt that these costs had been incurred and were recoverable but that the assured were still trying to find the invoice.

(2) £100,267.05 in respect of Takuma Head Office costs. These were the costs of Takuma's employees' investigations into the cause of the damage, including in large part the costs of both physical and computer modelling. Mr Barber described the number of hours spent as "*not too outrageous*" and the cost per hour as based on Takuma's in house accounting system which he had had demonstrated but whose accuracy he had had to take on trust.

(3) £229,236.75 in respect of Takuma site personnel. Mr Barber's comment was; "*This is admitted by Takuma to be an estimate. The hours do not seem to be particularly unreasonable. The rates have not been evidenced, but again do not seem outrageous*".

(4) £517,147.52 and £51,697.05 as "Overhead". Mr Barber's comment was: "*Overheads are claimed at 12%. We have no way of knowing if 12% is a reasonable figure without extensive analysis of their costs and contributions. However, most costs are presented as net so some overhead would attach. We don't know if the salary rates are net of all overhead elements.*" In his evidence Mr Barber said that they were at a "*very typical rate for contractors*".

143. From the comments of Mr Barber on these figures, I am satisfied that at least a further £300,000 of what Mr Barber put into the "unsupported" category is proved on the balance of probabilities, so that a claim in excess of £4,600,000 is proved.

Conclusion

144. Accordingly the claim succeeds in full and there will be judgment for the Claimants for their respective proportions of £3,680,000.