4. THE SPANISH CAMP DISASTER - A THIRD VIEW

by

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4.1 THE PRINCIPLE OF "OCCAM'S RAZOR"

This is a principle attributed to the 14th century philosopher, William of Occam, which holds that one should not multiply hypotheses unnecessarily.

If this principle is applied to the Spanish Camp disaster it would lead to the conclusion that one should first look for a single hypothesis which would explain why the tank failed catastrophically. Only if it can be demonstrated that no single hypothesis is adequate to fit the facts, should one then seek an explanation which requires more than one hypothesis.

4.2 THE SINGLE HYPOTHESIS APPROACH

If there is one thing that everyone who has studied this disaster is agreed upon, it is that the ill-fated tanker left a refinery so grossly overloaded with propylene that the ullage was either nil or, at most, was only a small percentage. It was then driven for two and a half hours on a hot summer's day and, either in the vicinity of a campsite at San Carlos, or inside the camp itself, it disintegrated and the subsequent fire killed 215 people.

The single hypothesis I am advancing is that the tanker burst solely because of excess internal pressure and without external causes other than climatic. Other investigators have advanced additional hypotheses. One investigator, Hymes (1985)[1] has claimed that the tanker burst after being engulfed in a fire; another investigator, Ens (1986)[2], has claimed that the tanker burst in the campsite after having been damaged through collision with its boundary wall. The official view, the one which was accepted without hesitation by the Spanish Court which gave judgement on the case, was that it was brought about by its internal volume becoming totally occupied by liquid which had thermally expanded and, the liquid being virtually incompressible, as its temperature rose, the hydraulic pressure generated strained the walls to their bursting point.

If the view of the Spanish Court is to be shown to be correct it is necessary to examine the question, could the tanker have heated up sufficiently in the time span of two and a half hours for this to happen?

4.3 THE ENGINEER'S REPORT

The Spanish Court based its finding that the tanker failed solely as the result of overfilling, upon the evidence of an engineer engaged by the Court who claimed that he had proved by two methods that the tank burst because of thermally induced hydraulic pressure. One of these methods was to show by calculation of the likely theoretical rates of heat transfer into the tank that it would have heated up sufficiently in the time which elapsed between leaving the refinery and reaching the camp. The other method was by a practical demonstration in which the heat-up rate of a similar tanker was experimentally determined.

I have examined the engineer's report in Marshall (1985)[3] and have concluded that the calculations he used were unsoundly based. I have also concluded that the practical demonstration which was carried out by loading a similar tanker with the same weight of water, i.e. just over half full, and measuring the temperature rise after driving it for the same length of time in the same climatic conditions, was invalid. This is because the internal heat transfer coefficient would be much higher in a surging load than it would have been in a nearly full tank where the liquid would be stagnant or nearly so. (It is much easier to mix a half full bottle by shaking it than it is to mix a full bottle by shaking).

In Marshall (1985), which Ens quoted in his article in Bulletin [illegible], I concluded, on the grounds given above, that the engineer had not proved his case, a conclusion to which I still adhere. But I do not now believe that my criticisms proved the opposite, namely that thermally induced hydraulic rupture could not possibly have occurred.

1.2 COULD THE TANKER HAVE RUPTURED HYDRAULICALLY IN THE TIME?

I have carried out calculations since Marshall (1985) which suggest, using assumptions which differ from those of the Spanish engineer, that hydraulic rupture after two and a half hours was in fact feasible. I consider that Ens's observation that the tanker's initial temperature was probably higher than the 4°C assumed by the engineer is very apposite. For example, if the filling started with the tank being full of propylene vapour, the latent heat would have to go somewhere and that somewhere would be for it to appear as sensible heat of the liquid. Rough calculation suggests that it would raise the temperature of the liquid by 2°C. Adding work done in pumping and heat entering from the surroundings, an initial temperature of, say, 8°C seems more feasible. The further possibility Ens suggests that filling only ceased when nothing more would go in may also be true but is not provable. His further suggestion that the true capacity of the tank may have been slightly less than the plated capacity is also feasible and may even be provable from the service records of the tank. Unfortunately, I did not have access to these.*

If the tank were full at the start, by how much would the temperature of its contents have to rise to rupture it? The Spanish engineer assumed that the tank had perhaps 4% ullage when it started off, but concluded that rupture would occur immediately the tank was full of liquid when even the slightest increment of temperature would cause it to burst. This is obviously incorrect, as Ens and I both agree, as there would be an appreciable stretching of the walls of the tank before failure occurred.

It might be useful at this point to look at the degree of stretching associated with various levels of pressure. A standard approximate dimensionless formula for computing the stretching of a thin cylinder subject to internal pressure and within the elastic limits is given below:

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* **Editorial Comment:** It is confirmed by the reports of the Spanish Court which state that the tank had 'a theoretical volume of 45,000 l and a real volume of 44416 l (Report of H.G. Stinton, p.3)

$$V_f/V_1 = (p/E \times r/t) \times (5/2 - 2/m) \quad \ldots \ldots \ldots \ldots \ldots \ldots \quad 1$$

where
V_f = final volume of tank
V_1 = initial volume of tank
p = internal pressure
E = Young's Modulus
r = radius of cylinder
t = thickness of walls
$1/m$ = Poisson's ratio

p, E, r and t must be in consistent units.

For the tank in question the operative dimensions were $r = 1.15$ m, $t = .008$ m, and the elastic constants for a low carbon steel could be taken as $E = 2 \times 10^6$ bar and $1/m = 0.25$. Thus the formula reduces to:

$$V_f/V_1 = p \times 1.4 \times 10^{-4} \quad \ldots \ldots \ldots \ldots \ldots \ldots \quad 2$$

where p is given in bar

On this basis, to achieve 45 bars (653 lbf/in^2), which Ens considers to be its yield stress, the tank would have expanded by ca 0.6% of its initial volume. The IChemE Physical Properties Data Service has given me data for the thermal expansion of pure propylene as ca 1.003 per 1^oC in the range 10^o to 30^oC. Thus a temperature rise of 2^oC from the disappearance of the vapour phase would achieve the yield stress and 3^oC would go well beyond it if the compressibility of the propylene were neglected. This agrees perfectly well with what I said in Marshall (1985) where I had no doubt that such a temperature rise could have been achieved in the journey time of the vehicle. The doubts I expressed in Marshall (1985) were whether there was sufficient time for thermally induced expansion of the contents first to fill the 4% ullage which the Spanish engineer had assumed to be present at the beginning of the trip. This, in rough terms, would have taken about five times as long to achieve as it would take to expand the liquid from its volume at zero ullage to its volume at yield stress for the vessel.

By pointing out that the starting temperature was higher than I had assumed in Marshall 1985, Ens has argued that the ullage was less than the Spanish engineer and myself had assumed and has made it more feasible that the yield point could have been achieved in the journey time. If Ens was right about the vessel being full at the start then there need be no doubt whatever that the yield stress would have been reached quite early in the journey.

4.5 CAN A VESSEL FAIL BEFORE ITS ULTIMATE TENSILE STRESS IS ACHIEVED?

I would agree entirely with Ens that if the tank had to expand until the ultimate tensile stress of the material had been reached, (and which would take us outside of the range of applicability of the equation used above), then this could not be brought about by thermal expansion alone in the climatic conditions existing. Ens argues that a properly designed, manufactured and tested tank will not fail before the stress in its skin approaches the ultimate tensile stress of the material from which it is made. Can we assume that in design and construction, sufficient care has been taken to ensure that the material is evenly stressed and local stress concentrations are avoided? If this was the case, no vessel would fail on test and we would know, without having to test it, that the strength of any pressure vessel would be that worked out theoretically by its designers.

What we would have had at 45 bar (654 lbf/in^2) which was quite feasible to achieve by thermal expansion of the contents, was a pressure 50% above the recommended test pressure of the tank. Yet Ens argues that the vehicle even then could only burst if it were subjected to an additional disturbance which caused the ultimate tensile stress of the material to be reached locally. He suggests that mechanical impact caused by the crashing of the tanker into the campsite wall is the most probable additional disturbance.

In practice a vessel so stressed, if it contains a point of weakness, will give way at this point. There is no certain evidence as to what the weak point was in the vessel at San Carlos but it could have originated for example in embrittlement consequent upon clandestine use for carrying ammonia.

4.6 WAS THERE EVIDENCE THAT THE TANKER ENTERED THE CAMP?

Ens relies for direct evidence that the vehicle entered the camp upon statements in the German illustrated magazine, "Stern". I have carefully examined the aerial photograph in the magazine and can see nothing to support these statements and find myself in entire agreement with the arguments which Hymes puts forward in Bulletin 068 to show that the tanker could not have entered the camp. I would add four further points to those made by Hymes. I too have had an opportunity to examine photos of the scene and have noted that there was a line of trees just inside the breach in the wall which, though charred, were still standing. Secondly, and I shall return later to this point, the wall is laid flat with the cracks being mainly longitudinal and it does not exhibit the damage which one would expect from the impact with a large vehicle which would have burst through scattering bricks and carrying some right into the camp. The third point is that the Spanish engineer in his report stated that there was no evidence of prior collision. The fourth point is that there are numerous photos which show the tractor shortly after the incident at the roadside near the disco.

4.7 CONCLUSIONS ON THE HYPOTHESIS OF ENS

I go a long way with Ens but part company with him over his argument that a vessel, so highly stressed, could only fail through mechanical impact. Also I see no evidence for, and a great deal against, his hypothesis that the vehicle burst through the wall and into the campsite and that it was the impact with the wall which finally brought about the rupture of the vehicle.

4.8 THE HYPOTHESIS OF HYMES

In Loss Prevention Bulletin 061, Hymes argues that a leak developed on the tanker, the driver stopped to investigate, the leak caught fire, the tank was enveloped in flames and that then a "BLEVE" occurred.

The first thing to be said is that this scenario does not discuss, as Ens and myself have done, the detailed mechanism by which the leak was initiated but I presume that Hymes accepts that the crack came about because the tank was over-stressed.

The second thing is that Hymes uses the acronym BLEVE in his explanation for the disaster. Unfortunately this is a chameleon word which changes its meaning to accord with its context. As propounded by Wilbur Walls in the seventies I understand that it meant an entire scenario which included the weakening of the walls of a fire engulfed tank above the liquid/vapour interface by flame impingement to the extent that, even in the presence of an operating relief valve, the pressure in the tank produces a "petal" fracture. (A petal fracture arises from a bulge in the locally weakened and softened wall.) Now this scenario was not applicable at San Carlos as you can't have, at one and the same time, a totally filled tank and part of the tank wall not in contact with liquid. There was also no evidence of a petal fracture. I go along entirely with Ens's rebuttal of Hymes's claims that there was a fire before the tank disintegrated.

In the more restricted sense that some have given to the term, BLEVE means the phenomena which accompany the loss of containment of a flashing liquid. In which case the vast majority of the BLEVEs in history are those which have accompanied the rupture of steam boilers which mostly operate at pressures up to about 15 bar (218 lbf/in^2). These have been purely physical explosions. That these have resulted in local overpressure cannot possibly be denied. It is just impossible for a vessel containing a liquefied gas to lose containment without local overpressure. Though such overpressure attenuates quite rapidly with distance, close in, it will not be much less than the storage pressure of the liquid. There has been a tendency on the part of some investigators, because this disaster was clearly not a vapour cloud explosion, to argue that there was no blast wave which arose from the disintegration of the tanker.

4.9 IS THE BLEVE HYPOTHESIS NECESSARY?

I believe that the catastrophic disintegration of a tanker containing liquefied gas and stressed to, or near to, its yield point and in which a crack then develops, needs no hypothesis of external influences.

In my view this highly stressed vessel gave way on its bottom weld whilst travelling past the camp. Initially the stresses needed to propagate the cracks came from the relaxation of the strain energy of the contents and of the walls. The process of disintegration was completed by the internal pressure of the flashing liquid which was able to spread the initial longitudinal crack to produce two circumferential cracks which resulted in the complete break up of the tank.* The fire-induced BLEVE hypothesis adds nothing at all to this scenario.

4.10 HYDRAULIC RUPTURE WHERE THE LIQUID IS INVOLATILE AND HYDRAULIC RUPTURE WHERE THE LIQUID IS A LIQUEFIED GAS

A point which I consider to be of importance to the discussion is to draw a distinction between the behaviour of a container subject to hydraulic rupture when firstly the rupturing liquid is involatile and secondly when the rupturing liquid is a liquefied gas.

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* **Editorial Comment:** The crack propulsion requires little energy since in a bi-axially stressed vessel it occurs almost without plastic deformation. Relaxation of strain energy of walls and contents provides enough energy for the disintegration. Flashing of the liquid starts only after the pressure has dropped, that is, after the walls have started moving away and by then the disintegration is already substantially complete. The tremendous energy of the flashing liquid is spent mainly in the propulsion of tank fragments and contents.

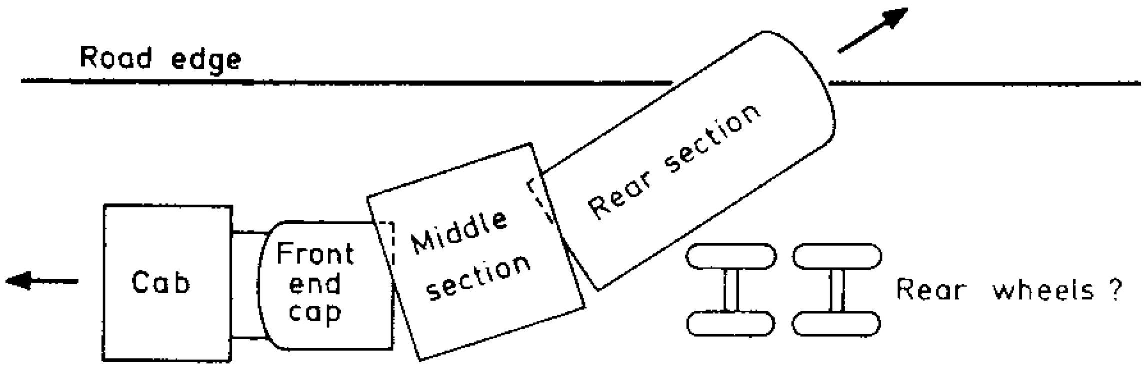

FIGURE 4.1 THE TANK AT THE INSTANT OF DISINTEGRATION

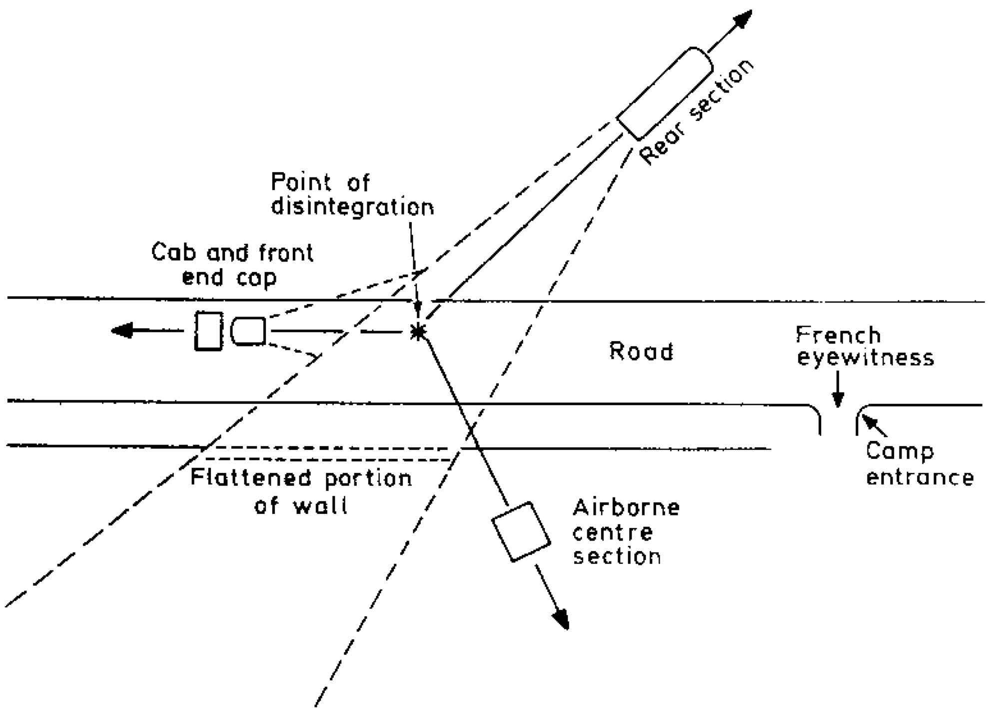

FIGURE 4.2 A FRACTION OF A SECOND LATER

The former case is the familiar case of hydraulic pressure testing where failure usually results in either local deformation of the vessel or in it leaking and where catastrophic failure is rare. Hydraulic testing is preferred to pneumatic testing precisely because the strain energy locked up in a compressed liquid is so small compared with that in an equal volume of gas at the same pressure. In short it requires the expulsion of only a tiny quantity of fluid in a hydraulic test to relax the strain energy in the liquid (though not necessarily in the vessel walls). After that there are no stresses arising from the contents for further deformation or propulsion of the vessel walls or its fragments. With pneumatic testing it is not until the greatest part of the gaseous contents have been released that there is no stress arising from this cause.

Consideration of these phenomena leads to the absolutely correct conclusion that for a pressurised air/water system, if rupture should occur, the amount of energy released at a given pressure is far higher when a vessel is full of air than when it is full of water.

But it is quite erroneous to apply this principle to systems in which a vessel contains a liquefied gas and its vapour. Here exactly the opposite is true. Here the greatest amount of energy released arises when the vessel is full of liquid. Here, once the vessel walls move outward, the volume of vapour which will be generated greatly exceeds the volume which would escape from a tank containing only saturated vapour at the same pressure. Hence the time before stresses at crack tips fall to zero is proportionately far greater. The presence of liquefied gas can be considered to provide "follow through".

The differing volumes which have to escape before the pressure in a tank has fallen to atmospheric, and hence there are no longer stresses at the crack tips, are set out in Table 4.1.

TABLE 4.1 COMPARISON BETWEEN STRAIN ENERGY IN VARIOUS CASES

Fluid filling vessel	Pressure in vessel bar absolute (lbf/in^2)	Final vol/initial vol
Water	40 (580)	1.002
Involatile organic liquid	40 (580)	1.004
Air	8 (116)	8
Propane	8 (116)	95*

* Assuming a theoretical adiabatic flashing fraction of 0.33.

A tank full of liquid propane on disintegration will thus give rise to a volume of vapour ca twelve times the volume of air which would arise from a tank full of air at the same pressure.

4.11 CONCLUSIONS ON THE HYMES HYPOTHESIS

Even accepting a restricted view of the meaning of BLEVE it is a quite redundant hypothesis and there is no direct evidence that one took place. Hymes's hypothesis that the driver got out to investigate does not accord with the account given in the German magazine "Stern" that his mangled body was found in the cab. Arguments derived from previous incidents are not valid as these took place in vessels where there was substantial ullage and thus had dry walls which could be weakened by flame impingement. In the Spanish disaster the vapour pressure of the liquid took over from hydraulic pressure to complete the disintegration (by propelling the parts and contents of the tank).

4.12 WHAT HAPPENED AFTER DISINTEGRATION

There is a great deal to be said for "running the film backwards" and thus bringing back the parts to the point of disintegration to look at their orientation at that time. If Ens's Figure 4.2 is examined and the point of disintegration is moved on to the road and the parts along straight lines are brought back, the tanker at the point of disintegration would have been, in plan view, as in Figure 4.1.

The rear and largest portion was at an angle to the road and inclined away from the campsite but with its open front end pointing to the wall which subsequently collapsed and to that part of the camp which was most severely burned. It is my view that on disintegration this rear portion discharged a diverging two phase jet. (This would have been, by weight, about 1/3 vapour and 2/3 liquid, and by volume about 100 of vapour to 1 of liquid.) This was discharged across the road and it was this which knocked down the wall. (From its flimsy construction I estimate that perhaps an over-pressure of 0.1 to 0.2 bar (1.5 lbf/in^2 to 2.9 lbf/in^2) would be all that would be needed to blow the wall over.) The jet may well have continued out to sea, (See Figure 4.2)

The instantaneous reactive thrust on this rear portion would have been of the same order as that of the engine of a Space Shuttle. The instantaneous initial acceleration would have been of the order of 20 g in a direction roughly opposite to that in which it had been travelling. This was probably the cause of the rear part of the tank parting company with its wheels which were travelling forward when the tank was being propelled backwards.

The middle portion of the tank, if this, as it seems highly probable, had disintegrated at the bottom weld, would have discharged its contents onto the road surface at high velocity thus producing the damage to the road noted in Hymes's comments in Bulletin 068. It was probably this discharge which swept the rear wheels away. Being relatively flat after disintegration, the middle portion was probably hurled upwards and then "fluttered" down to where it ended up in the site.

The front end, which was pointing down the road, would have been accelerated like the rear end with roughly the same thrust, but this would have been an addition to the existing motion. The acceleration produced would have been less than that of the rear portion as it would also have to accelerate the tractor. Nevertheless it must have fiercely accelerated the latter and may have killed the driver by this cause. Somewhere down the road the front cap over-ran the tractor which was turned over. The front cap then travelled further down the road than the tractor. The jet from this must have interacted with the jets from the other portions to produce the resultant divergent jet which entered the site.

It was because of the orientation of the rear portion which contained half the load that the damage pattern was asymetrical with regard to the road.

4.13 THE BEHAVIOUR OF THE VAPOUR CLOUD

The jet of vapour/liquid which entered the camp would have been intensely cold, perhaps as low as -47°C, and accompanied by a gale force "wind" laden with drops of liquid propylene. (This latter feature has been noted elsewhere. Witnesses at the Mexico City disaster commented on something intensely cold dropping from the sky and which on ignition burned like Napalm.) It is my view that the effects of this blast of vapour must have been so horrific that some people probably died of shock and asphyxiation before the flames reached them. The drenching with liquid propylene may have accounted for the partial cremation of some of the bodies.

There appears to be evidence that there was an appreciable delay between the initial bang and the conflagration and that some heard a second bang around the time flames appeared. The first bang in my view was the sound of the tanker disintegrating, the second was from an internal explosion in the disco. Around this time ignition sources in the flammable zone were igniting the cloud at a number of points more or less simultaneously.

4.14 CONCLUSION

Only a single hypothesis is needed to explain the disintegration of the tanker. Following overfilling it disintegrated, as the result of thermally induced hydraulic pressure and perhaps around the yield point of the steel of which it was made. The disintegration started at a point of weakness at the bottom weld of the middle section. *(But is this not the second hypothesis? - Editor)*

Following the disintegration, reactive forces generated by the flashing liquid propelled both front and rear portions hundreds of metres. These acted as jet engines and the resultant two phase jet pushed over the camp boundary wall and drenched the centre portion of the camp with liquid propylene as well as producing a cloud of propylene vapour. These later ignited with dreadful consequences.

Though I have disagreed sharply with them on some points I must also register agreement with both Hymes and Ens on many of the points they make and which I think add greatly to our understanding of this most shocking affair. I would hope that a consensus could be reached by putting our three articles together.

Perhaps the French tourist cited by Ens in his section 2.3 may be allowed to have the last word on whether the tanker ran into the camp or whether there was a fire before rupture. ".... the tanker passed, started to snake and then tipped over. This was followed almost immediately by an explosion and a severe fire." (For the position of this eyewitness see Figure 4.2).

4.15 ACKNOWLEDGEMENTS

I would like to pay tribute to two people, Harold Stinton of the Hampshire Fire Service and Norman Scilly of the HSE, who visited the site, and without whose reports and photographs this article would have had to be much more speculative.

REFERENCES

1. Hymes (1985); Hymes, I., "Up-date on the Spanish Campsite Disaster', Loss Prevention Bulletin 061, February 1985.

2. Ens (1986); Ens, H., "The Spanish Campsite Disaster - A New Scenario", Loss Prevention Bulletin 068, April 1986.

3. Marshall (1985); Marshall, V.C., Doctoral Thesis, University of Bradford. "Major Chemical Hazards" 1985.

IR. H. ENS'S COMMENTS ON "THE SPANISH CAMP DISASTER - A THIRD VIEW" BY DR. V. C. MARSHALL

SUMMARY

Marshall does not achieve what he sets out to do: prove that a single fault (hydraulic pressure resulting from overfilling) caused the tank to disintegrate. He needs as a second hypothesis the same assumption I made: the presence of a "weak spot" in the tank wall. The difference between me and Marshall is that I think the weak spot was generated at the moment of the accident by the mechanical impact of the tanker crashing into the campsite wall, whereas Marshall assumes the weak spot to have been created by previous abuse of the tank - such as illegal carrying of ammonia.

My assumption is supported by the evidence Scilly gathered at the site of the accident. There is no supporting evidence for Marshall's assumption and it would lead to a failure-mode which is contradicted by the only eye-witness of the explosion: the French tourist cited by Marshall, who saw the tanker "snake and tip over".

4.5 FAILURE-MODE OF THE TANK

I am certainly not saying that no tank ever fails before its entire skin has been stressed to the ultimate tensile stress of the material from which it is made. I never make that kind of sweeping statement.

What I do say is that in designing a vessel care is taken to distribute stresses as evenly as possible throughout the material used - so as to use the tensile properties of that material as well as possible. Of course every construction fails at its weakest point, and that weakest point should in a well built vessel not be much weaker than the value I gave for the ultimate strength of the material - just like in a well-made chain no link should be much weaker than the others.

Thus in order to make a vessel fail at a stress level considerably below that ultimate strength, something else must be seriously wrong; in other words: a second hypothesis is necessary to explain the failure. In the case of the Los Alfaques disaster my second hypothesis is mechanical impact due to a road accident. Marshall's second hypothesis is a "point of weakness" in the vessel which he suggests could have originated by embrittlement consequent upon clandestine use of the tanker for carrying ammonia. There is no evidence of such use at all, and to me it seems highly improbable - bearing in mind that the quantity of ammonia transported by road tankers is miniscule in comparison to the quantity of LPG transported, that carrying ammonia in LPG tankers is forbidden and that the exploded tanker was very boldly and permanently marked as an LPG carrier.

As to pressure-testing of vessels: that is done for the express reason of making sure that no weak points are present. The test pressure is chosen in such a way that a weak point will manifest itself either by plastic deformation or by leaking. Catastrophic failure during a test is at best extremely rare and in fact I have never heard of it occurring with a simple cylindrical vessel like the tanker concerned. The test assures that the yield strength (that is the stress at which the material starts to yield) of the entire vessel is above the test pressure. The material used must yield at least 14% before it breaks. Thus if the test has been passed successfully the vessel will fail at a stress level considerably below its ultimate strength only if a serious and quite abnormal weakness is present, and that weakness must have developed after the test. I recently witnessed a test in which a 35 year old vessel, designed for a maximum pressure of 10 bar (145 lbf/in^2) and pressure tested at 15 bar (217 lbf/in^2), was overfilled and warmed up. It failed only after attaining a pressure of 79 bar (1145 lbf/in^2) and increasing in volume by approximately 35%. That is typical for the failure-mode of vessels subjected to excessive internal pressure.

4.6 WHERE DID THE TANKER EXPLODE?

Evidence submitted - amongst others by Dr. Marshall - to me after writing my article convinced me that indeed the tanker did not explode in the campsite but on the road. There is however, substantial evidence that it did not explode because of the hydraullic overpressure only. If that had been the sole cause there would not have been any warning at all.

That failure-mode is contradicted by the only eye-witness who did see the tanker at the moment of the explosion and observed: "the tanker passed, started to "snake" and then tipped over. This was followed almost immediately by an explosion and a severe fire". I am convinced that the tipping over caused the mechanical impact that triggered the disintegration of the tank. Whether that disintegration occurred on the road or in the campsite is of secondary importance. Also a motor-cyclist following the vehicle is quoted as claiming to have seen smoke and fire issuing from the tanker before the explosion.

Further evidence of the tanker crashing before it blew up is found in the report of Scilly, the Explosives Inspector who made a very careful and detailed inspection of the accident site. He examined the fragments of the exploded tank and found: "some quite severe scouring on the offside of the rear portion, running from the bottom rear end upwards and towards the front end at an angle of approximately 45°." After considering several alternatives, Scilly concludes that the most probable cause of this scouring is that "whilst the tank was still intact it passed over a continuously yielding surface such as a wall". From the position of a mudguard of the trailer and several bricks of the wall, Scilly concludes that the "rear end of the trailer must have gone over and through the wall and into the campsite". Other evidence leads him to assume that "at least part of the unit went through the wall and into the campsite and out again". This agrees well with the "snaking" the French tourist observed.

Furthermore Scilly found a sharp crease in the mid-section of the tank which could have been caused by contact with one of the steel "T" bars used to reinforce the wall. He thinks this contact led to the initiation of the crack.

What remains is the question why the tanker started "snaking" in the first place and where the smoke and fire observed by the motor-cyclist came from. here we find another answer in Scilly's report where he states that he believes a tyre fire on the rear nearside caused the tanker to swerve. This belief is supported by the rear axle of the tanker impaled on the campsite-wall, outside of the area affected by the fire, with its nearside tyres severely burnt and the offside ones completely intact. The conclusion that the tyres must have been burning before the crash is almost unavoidable: how could they be ignited later in a place so far from the fire, and if they were how could only those on one side of the axle be ignited and those on the other side remain intact?

CONCLUSION

Marshall and I agree that the tank disintegrated under the influence of excessive pressure caused by overfilling and subsequent warming up. We also agree that the tank disintegrated "cold" - that is: without previous heating by fire. Both of us need one more hypothesis to explain the rupture of the tank. Marshall's is the presence of a weak spot in the tank, mine is mechanical impact of a crash, probably caused by a tyre-fire. This in turn may have been caused by a flat tyre on one of twin rear wheels, which produced a 100% overloading of the other tyre. That overloading would have made it heat up, catch fire and blow out.

Since the main purpose of accident-investigation is to find means to prevent recurrence in future, I think the most important point to make is that Marshall and I completely agree that the disaster would not have occurred if the tanker would have been equipped with a safety relief valve.