Bridge of the Month No53, May 2015
Preston on the Weald Moors

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It is long past time I got back to reviewing bridge tests. It will be clear that I am doing this in no particular order. The bridge at Preston on the Weald Moors is a classic of the canal era. A stone, three centred arch, slightly skewed, supporting brick infill and spandrels. The spandrel walls were both tilted ad curved, creating a very stiff structure. The various records of the test do not describe the structure in detail which is quite hard to forgive. One reason for writing this is that "I was there" and I see a real need to put some stuff on record.

This picture shows the bridge in 1964 when there was still water in the canal. By the time of the test, that was long gone as can be seen below.

The test report gives the span as 4.95m square 5.18m skew. That is (near enough) 16ft 3 in square span 17ft skew span. Unusual for the skew span to be the round figure in this application. The skew of 17 degrees is presumably computed from those dimensions. The rise is quoted as 1.64m which is also an odd dimension at 5.38ft. Well, that last is just nonsense. 16.5 by 5.5 would be 3:1 which sounds more likely, but that leaves the question of whether the shape is really an ellipse, or three centred. Three centred would be more suitable for a canal bridge because it would give a higher shoulder for a horse to pass under on the towpath.

Looking at the string course in the view above, it is obvious that there is a modest drop at mid span so the rise is reduced to some degree.

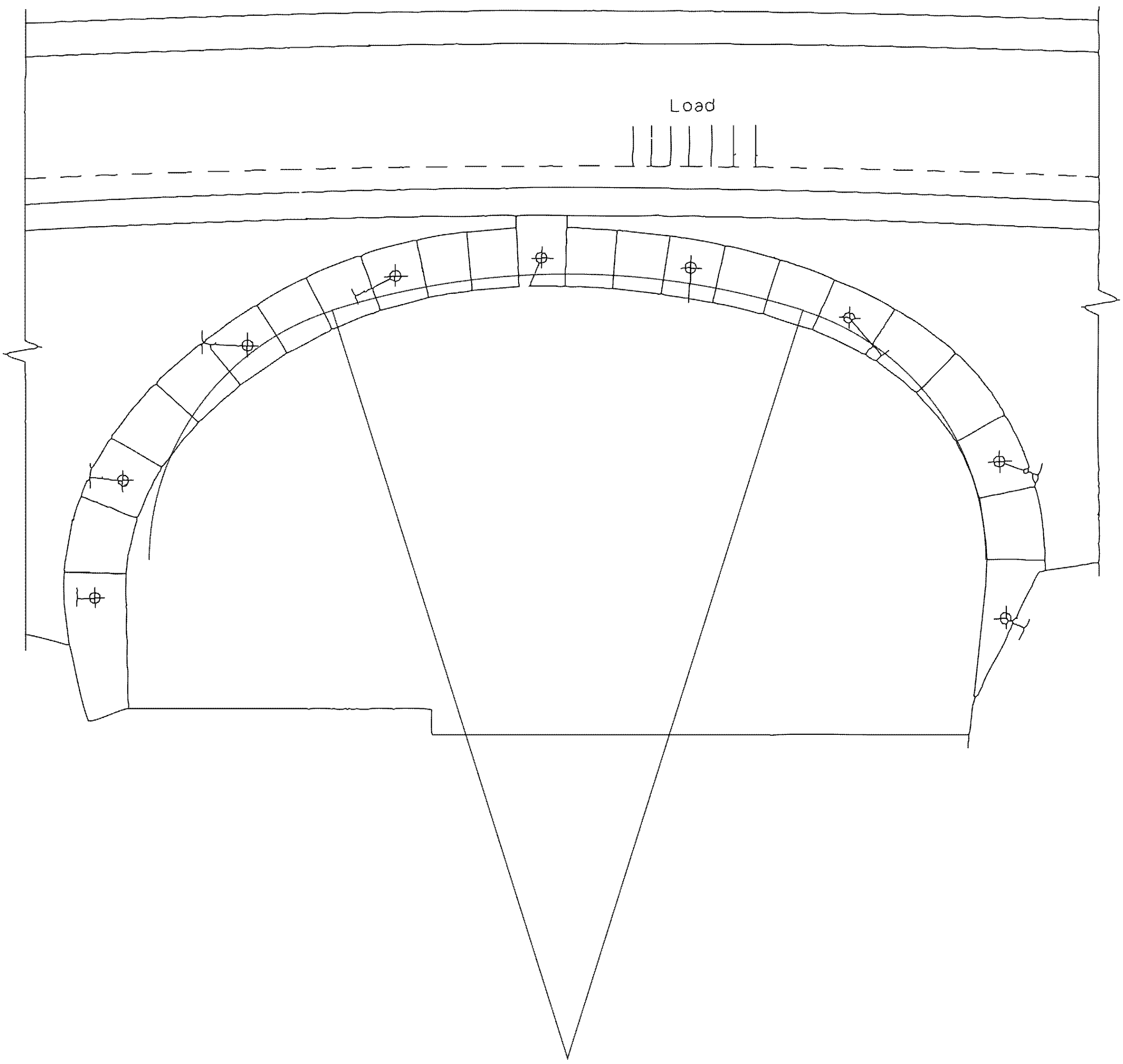
So, let's look for some other clues.

The next picture shows the shape a little more clearly, but also shows different voussoirs lengths in mid span and at the ends. To me, there are 5 wider voussoirs at the ends and 11 narrower ones in the mid span. There would be no good reason for doing that if the radius were changing continuously, so it seems likely that we are looking at a 3 centred arch. Now we need to try to reconstruct dimensions. If the original span really is 16ft then what radii might be expected. Let's begin by assuming 16 ft radius. If the rise is actually 5.5ft, then we might guess at side radii around 4ft to 5ft.

Because the bridge is distorted before testing, the best we can hope for is to fiddle around with dimensions till we get something sensible. After a good deal of fiddling I settled on 16 ft span, 16ft main radius, 5ft side radius, which gives a rise of 5.417ft (5ft-5in) or 1651mm. I have no way of knowing whether the rise was measured from the higher, the lower of the average springing level but a drop if only 10mm doesn't seem a lot.

But then, look at the drawing published in the TRRL report.

It shows a lop-sided arch but I suspect it might be a 15ft central radius and then a whole lot of distortion, and, of course, we cannot know whether this is a skew or a square elevation.



There is a lot there that is unsatisfactory. The basic shape is scanned from a published report and then electronically traced, which is why there is a little cusp at each junction. I don't find the drop and spread disconcerting, but why has the mid span point moved further to the left than the left springing. Well, there has been a certain amount of unwrapping to allow the spread and settlement shown here.

The underlying message here is that we don't have the data. Any review of the test must stop short of numerical because the numbers just aren't there.

Oh, and, one thing it definitely isn't: elliptical.

Anyway, here is a picture of a partially completed test. It raises the really interesting question (interesting to me at least) of why the most damaged masonry is on the soffit, right under the load. Here you would expect wide cracks on the soffit. In fact there is a pile of masonry on the floor but no fill has followed it down. That is because the upper shell of stone is still complete. The stuff on the floor has fallen out because the stone split on a ring line and the flexing of the arch was enough to let the wedges fall out.

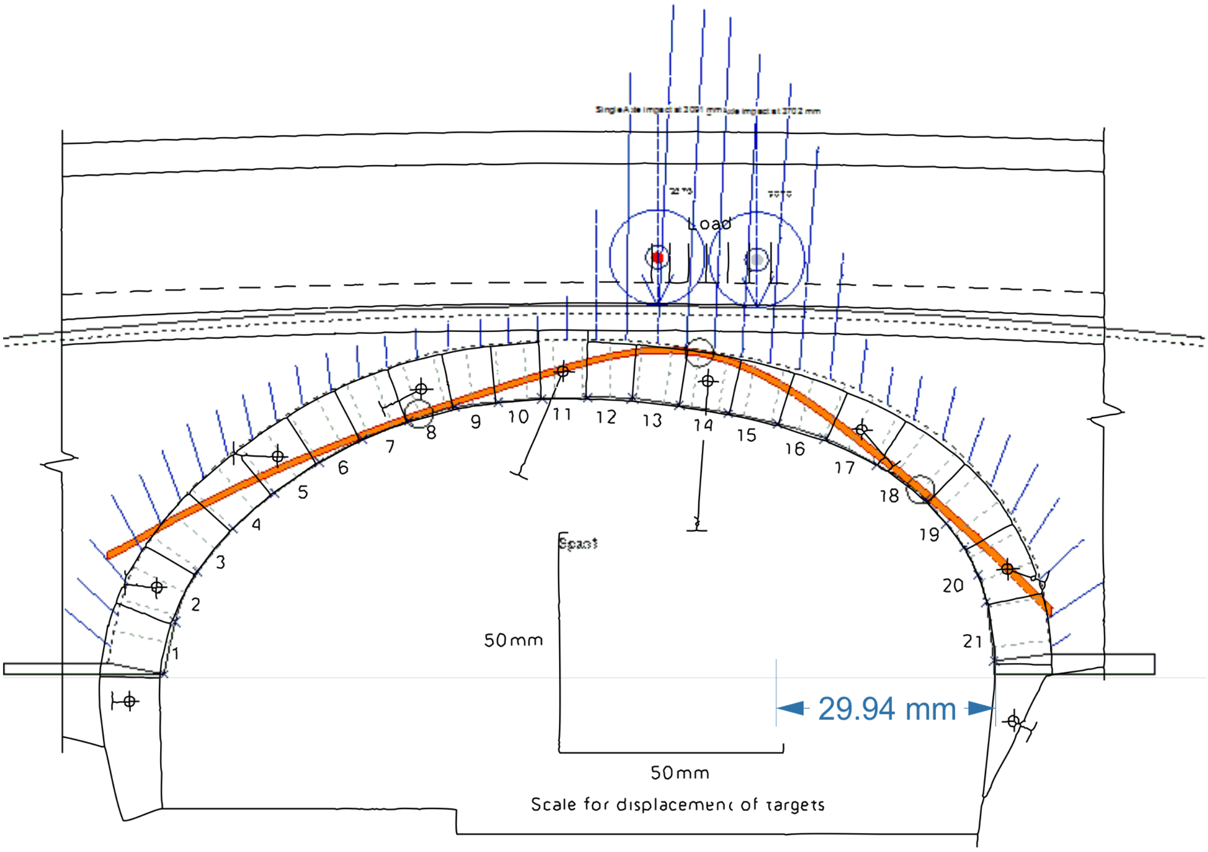
There is also modest crushing at (or near) the points where thrust touches the intrados 6 joints up from the left and 3 up from the right.

And, of course, there are cracks running round the ring. Notice the one on the right which is taking off the skew corner of the bridge and which continues, more or less, round to the left, still on that line.

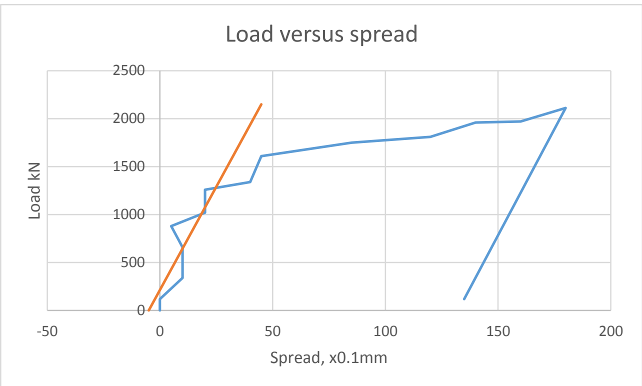
Almost hidden behind the nearest ground anchor cable is the mid width survey target used for deflection measurement.

At this, relatively early, stage of loading there is already the beginnings of crushing and hinge formation at the joint beyond the cables but no sign yet of one to the right. The edge crack along the skew line is already evident. Interesting, though, that it is beginning under the load and working outwards. Could it be driven by the skew end of the load pad?

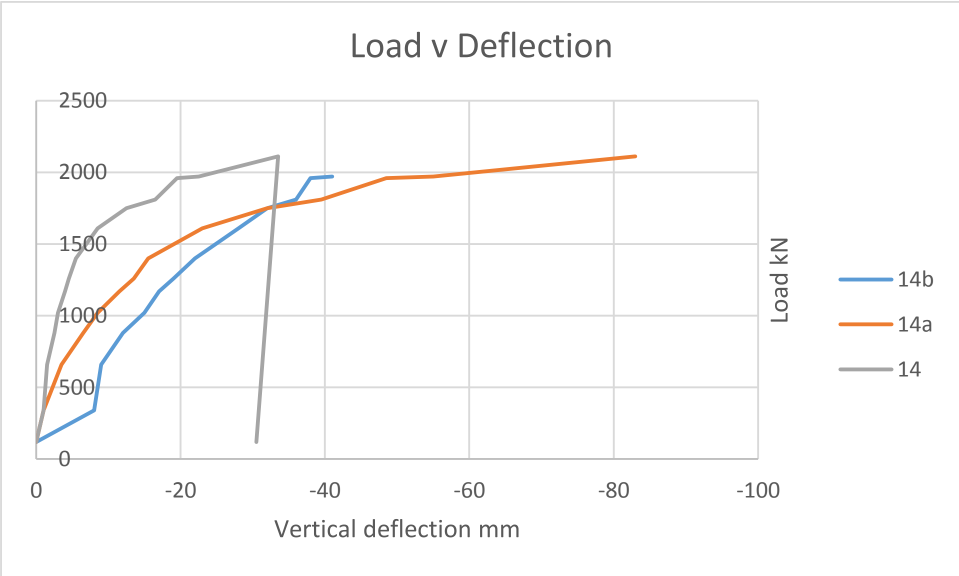
From this view, there is no indication of a crack under the load but at such an early stage it would be too small to see anyway.



Superimposing the thrust from Archie shows how the force pattern relates to dispolacements, though it is worth noting that the displacements here are measured on the edge of the arch, not underneath where some are much greater. Despite that, a realistic picture of behaviour can be gained. Up to Voussoir 5, the abutment is moving bodily to the left and rotating about a point near the ground level shown here. The right hand abutment has much greater vertical load and is being pushed down and back. At failure, the vertical load to the right is about 35 tonnes per m run against 17tonnes to the left. Looking just at the spread of the arch the peak value is 18mm and measureable growth begins at the first load increment of 34 tonnes total, 22 tones increment.



Of course, the spread is measured on the arch face but with substantial backing we casn reasonably assume that the face and the centre are moving by similar amounts. Graphing progress of the peak spread shows a pronounced change of behaviour at around 1300kN total load. The red lin on the graph projects the recovery line back to the origin. The increments of load are really too large and the available accuracy of measurement too little to alow division of the “curve” into segments but I would suggest that somewhere around 800kN the abutments begin to behave in a way that is not sustainable. Any load factor on behaviour should be based on no more than that.



Turning now to the load deflection curve, we have more basis for interpretation. 14b is a target at the far side of the bridge fixed to the underside of a voussoir. It was obscured by the defection at the centre (14a) at around 40mm, though the difference from the other edge and the step at the start of nearly 10mm suggests either that the intial reading was wrong or there was some sort of local perturbation in that first increment. 14 and 14a separate at around 400kN total load and both are indicating distinctly non-linear behaviour by 800kN.

From this angle, the skew splitting is more evident, though the continuation back to the abutment near the load is not so pronounced, because that is the strong direction. Note that here, the main crack under the load is wide open but has still not resulted in splitting of the stone as seen above. And just how much deflection is there at that point? It is certainly more than the 80mm maximum recorded on the central target but by this stage the central target is gone and the edge ones are no longer following the arch. An estimate says 150mm of deflection but the rotation in the crack is still quite modest.

Looking back to the thrust model the hinges are forming at around voussoir junctions 7/8 and 18/19

The levers between the hinges are then 1.4 and 1.9mm and the combined rotations, would produce an opening of about 35mm, which does not look like an unreasonable estimate.

The bright sunshine for this photo makes the contrast extreme and obscures the details of construction but the level of backing is clear, reaching well up the 5th voussoir. The point at which the spandrel becomes the parapet probably coincides with the top of the string course and that would imply that the wider wall passes right over the arch so that the corners of the load pads are sitting on it. The string course is stone and shows clear articulation in the phot with all the rubble below the bridge, so that ties together. Better photos might show string course damage in different places on the two sides.

Some conclusions

The first comment is that there is a great deal of value to be extracted from even the very limited range of test results published. One assumes that the unpublished report referred to in the published one has more data, perhaps even the original survey.

It is clearly utterly inadequate to base any analysis on the very limited results presented in BA16. Those who suggested doing so should have known better.

For this bridge, I would suggest that damage was being done at a load of 400kN. In terms of a realistic axle load that probably only amounts to 200kN and a sensible load factor on that would reduce the value to around the normal maximum traffic load.

The limit of capacity for this bridge is almost certainly set by the abutments. Movement at a level that can be detected by surveying is beyond what can reasonably be sustained as a regular load.

And some observations

I find myself, over and again, saying "Is it a fair test?" So far as delivering useful information for assessment, and certainly in the form of the output presented in BA16, the answer is a resounding no. The loading system does not represent a real load, not least because it is parallel to the abutments rather than normal to the road, but also because it extends across the full width including on to the top of the spandrel walls, forcing full distribution but also ensuring that some of the load is transferred to the spandrel walls.

Then there is the issue of shape. The bridge is casually described as "elliptical" but it is clearly not that but three centred, which creates a higher shouldered shape with more horizontal thrust.

It is really rather sad that such a large sum of money (around £100k at 1986 prices) was spent on a test where the critical displacements were measured with such a crude system. Sacrificing a frame and a few displacement transducers would have added little to the cost and delivered much more detail. Especially, showing a detailed and reliable load deflection curve at the lower end of the load. From that, it might have been possible to pick a point where the behaviour first became non-linear.

There is clearly much more to say about this test, but I have no more time this month.