Letter from Doug Landau

7th March 2017

Locomotive Resistance

This is in response to John Knowles letter 2nd December 2016. The many points raised are not necessarily taken up in chronological order. Words in quotation marks are John's own words unless otherwise stated. This letter is longer than was perhaps essential because it contains information that may be helpful to readers unfamiliar with this topic.

You say "I know of no other analyst of the subject other than Doug who considers that the whole of the resultant is part of MR". Who are these other analysts on the subject? As one mechanical engineer well versed in the ways of steam recently opined regarding your procedure; "As you correctly point out, WRTE and the pull recorded on the Amsler dynamometer were one and the same. Also, WRTE has to be net of all the machine friction inherent in driving the locomotive. Axlebox friction forms part of MR, it does not appear in WRTE; it represents part of the difference between indicated power and WRHP. It cannot somehow escape to be part of the WRTE only to be absorbed later, I do not see the logic of that." The relevant force diagrams can be found in Lomonossoff's *Introduction to Railway Mechanics*.

Direct studies of MR as opposed to the subject of LR are a distinct rarity, largely because experimental data on the former is scarce, and such as it is generally unsatisfactory. It is only in recent years that researches at the NRM have brought to light the wealth of relevant experimental data from the Rugby test plant. The available data from the Vitry test plant is very limited in this regard, and amount to some data for the EST 241 - 004 4 cylinder compound.

The Railway Mechanical Engineer (USA) for May 1943 featured an article by Lawford H Fry analysing locomotive test plant MF data for 10 locomotive types dating back to the tests at Purdue University about the turn of the 20th century to the 1930s, involving four, six, eight and ten coupled locomotives. Most of the tests were carried out on the Altoona test Plant. Notwithstanding the extent and diversity of scatter present in the various data sets, Fry sought to reconcile the data notwithstanding "given an uncertainty of 40 per cent", into a formula for machinery friction. The outcome was a function of coupled axle loading, driving wheel diameter and the number of coupled axles. In the event the latter factor was given undue significance, but was perhaps the best he could do with the data available. Curiously Fry was using the 'small remainder' (ITE – WRTE) data for the exercise; a problem he fully acknowledged. Perhaps he thought this was the best way to compare different data sets, or possibly the data available to him was incomplete. The scatter magnitude was uniformly much greater than present in the Rugby data. As far as I know this was the only published study specifically focusing on locomotive machinery friction based on experimental data, or from a purely theoretical standpoint.

It's surprising you cited Ell's comments on the locomotive resistance curve for the Rebuilt Merchant Navy in Test Bulletin No. 20. The curve itself is the same as appeared in Bulletin 15 for 71000. Given the very similar basic architecture of the two types this was not unreasonable, and must have assumed any frictional differences for the different valve gears would be too small to be of practical significance. Ell assessed the resultant frictional augment over the standing coupled axle load losses at about 300 lb, a long way short of the magnitudes you ascribe to your statistical exercises. A constant 300 lb was a bit of a simplification, but in magnitude was not dissimilar to what the WRHP data recorded at Rugby indicates. At an ITE sensitivity of 2.5% (typical value) and say 1850 IHP at 60 mph (a typical express work rate), it works out at out at 290 lb.

In 1944 E S Cox presented a paper on Locomotive Axleboxes to the I.Loc.E, it included an analysis and diagrams of the forces encountered by the coupled axle boxes of a Midland 4F working in 30% cut-off at 15 mph. The exercise was essentially the resolution of two forces, the net axle load which was a constant defined as the 'vertical load', and the combination of piston thrusts, a variable. The resultant axlesbox loads in the course of a revolution were quite nuanced, a situation involving the cross couples between the RH and LH pistons phased at 90° and the axleboxes. The resultant peak RH and LH loadings were about 80% or slightly less of the summed forces. The overall "work factor" for the RH box was 18% higher than the LH. Details are also given for an outside cylinder arrangement, this equalised the RH and LH workloads. This exercise and its modelling is indicative of how MF would have been tackled had there been an interest in estimating it.

You say "How could the gear (mediating) react several times per second to movements in both directions, i.e. was it capable of keeping up with the frequency of the sources of variation in DP?"

The mediating gear and servo mechanism did not operate in the way you describe. Firstly the dynamometer, of the hydraulic type was exactly the same as fitted to the LMS dynamometer car No.3 commissioned in 1948. Such dynamometers are more than capable of absorbing, measuring and integrating the variations in drawbar pull during the course of a revolution. The mediating gear and servo mechanism played no part in adjusting to these transient forces; this was not its function. Variations in drawbar pull could be guite severe relative to the mean value. The offical report on the 1948 Locomotive exchanges contains a number of drawbar pull (DP) traces; some trace a sharp zigzag profile in the course of a revolution. The WD 2-8-0 for example, not blessed with any reciprocating balance, delivered a very spiky trace, with an amplitude of +/- 7% about mean pull.. The GW 28XX 2-8-0 and the LNER O1 were not much better. These traces were in the 17 - 26 mph range. The Stanier 8F was much smoother, about +/- 2%, but there were some random intermittent spikes about double this. The LMS Class 5 trace showed minimal ripple at 55 mph, but the mean pull was undulating. The B1 at 53 mph was not quite as smooth as the LM 5, and again delivered an undulating trace. The GW Hall was notably uneven in one example which is captioned "Increased oscillations encountered at 31 to 38 mph (+/- 10%). All the multi cylinder engines delivered smooth traces A slight exception here was the GW King, with intermittent periods of with undulations. zigzag present in the trace. These undulations likely reflected local changes in gradient, curvature and track condition. The various dynamometers used in these trials were evidently sensitive to all the locomotives could throw at them.

In the normal way of things any disturbing forces resulting from transient changes in drawbar pull were dissipated in parasitic motion (swaying, rolling, hunting) of the rolling stock. Some of this behaviour, as clearly observable from inside an underground train, is down to ride characteristics and track imperfections. This situation is also sensitive to the drawgear arrangements. As first built, Britannia hauled trains were soon receiving complaints of "shaking effects" from passengers. After mathematical analysis the solution proved guite simple; a reduction in the initial compression of the tender drawbar spring. "The rogue W.R. two-cylinder engines were found to be just as amenable to this arrangement as were the BR engines themselves "(E S Cox). The situation on the test plant with the dynamometer anchored solid is rather different, any potentially resonant forces have nowhere to go. In the absence of any damping equipment as first built, the French test plant at Vitry dating from the 1930s, encountered severe resonance problems with 2 cylinder locomotives, a situation largely resolved by the addition of Bellville Washers (springs) to the test plant drawbar. These achieved a satisfactory damping effect. This lesson was well understood when the Rugby Test Plant was in the planning stage. Jim Jarvis commented that the more solid anchorage of the Rugby dynamometer brought further improvement (written communication). The damping deflections involved were slight, within ¹/₈",

The function of the mediating servo mechanism was solely maintaining the locomotive at top dead centre (TDC) and correcting any drift from this situation. It was insensitive to any drawbar pull variations of shifts from TDC in the course of a revolution or even many revolutions. Key to its function was a differential gearbox, its two wheels rested on a disc and were friction driven by its rotation at constant speed as a function of time. Provided the wheels were equidistant about the disc centre the gearbox output shaft was stationary. The faces of the wheels were transverse to the fore and aft shift. The set up was such that the gearbox moved back and forth about the rotating disc centre line in equal magnitude to, and in synch with the fore and aft motion of the locomotive. Provided the motion was equidistant about TDC the fluctuations of output shaft cancelled out to zero, and no "inch seconds" would be recorded. Should TDC not obtain the "inch seconds" would be added to or deducted from the recorded value dependent on whether the TDC shift was fore or aft. The second function of the differential gearbox was to drive two moveable electrical finger contacts. These were interposed by a third contact that moved back and forth between these contacts at the identical amplitude (typically less than 1/8") of the locomotive's fore and aft motion. This shuffling contact was fixed to the mediating control rod connected to the locomotive; this rod was not subject to any stress or stretch and incorporated positional adjustment provision to suit any locomotive type. The distance between these outer contacts was such that in the TDC situation no contact would be made between the two differential controlled contacts and the shifting middle contact. The two outer contacts swung, too and fro pendulum fashion in the course of a revolution as the differential gearbox picked up the fore and aft shifts. In the TDC situation the deflection would be equidistant about the zero datum line, the swing per revolution remaining equidistant left and right. In the event of a shift from TDC, the finger contacts swing would be biased to increased swing in one direction, building up the swing bias such as to eventually make contact with the centre contact, initiating remedial plus or minus action by the dynamometer dependent on the initiating contact, the other contact will have become more distant. I have no details of the Amsler circuitry, but this transient contact will have closed a control relay with a time delayed drop-off, in other words the dynamometer was given a nudge for a finite period of time. At this period of history such timed relays were dashpot controlled and adjustable, so the optimum timing could be fine tuned during the commissioning phase. Such nudges would occur at intervals, reducing the rate of swing bias until the "inch seconds" reading stabilised, remaining constant. This situation was probably well in hand by the time the warm up period was complete and the test period commenced. A continuous paper trace plot of shifts about TDC was recorded. The operation of the mediating gear can be summed up in one word – 'measured'. The test sheets also included a provision for mathematical correction should there be an "inch seconds" discrepancy. As first supplied the mediating gear was over reponsive, the differential gearbox ratio was reduced as a consequence.

Amsler's conditions of contract included performance guarantees. The dynamometer was guaranteed to within 1% in regard to pull. Carling believed it was well within the guarantee and that it was consistent to even finer limits. Work done was guaranteed to within $1^{1}/_{2}$ %.

You say: "My difficulty is that I think the Rugby data poor/inadequate, only a handful of the world's locomotives were tested at Rugby, and I work at MR and LR more generally, for application to other locomotives. What is easy for him in principle for a handful of locomotives is only a tiny part of the need for well informed MR and LR."

In regard to MR, quite where this body of alternative of "well informed" MR data comes from I cannot think, a problem as alluded to above. Regarding locomotive resistance there is certainly plenty of data, world wide if you want it, but such as it is can fairly be described as a minefield of disparity. LR is after all a variable, subject changes in effort, wind speed and direction and track condition. Regarding the latter some tests in the USA using the same set of rolling stock on three different railways found significant changes in rolling resistance which

was attributed to differences in track and track bed formations. Locomotives would be similarly affected.

MPH	PRR	C & NW RR	UP RR	Davis Formula	
60	89	100	109	100	
70	86	101	104	100	
80	89	103	100	100	
90	90	102	96	100	
100			87	100	

Some of the French compounds displayed extraordinarily high locomotive resistance compared to other continental types. The disparity is too high to be explained by attributing a few percentage points inaccuracy by the indicting equipment. The recorded MRHP for the EST 241 mentioned above is likewise high.

It is ironic that I am accused of insulting scientists when your letter is riddled with attempts to disparage Carling and associates at every supposed opportunity; no matter how speculative, or ill informed. Likewise, Amsler, at the time a world leader (if not the world leader), in the field of scientific instrumentation metrology, are, by implication, similarly rubbished. This dubious collection of non sequesters does not survive scrutiny (your text in inverted commas).

1. "The Rugby figures are not the same as MR properly called, however. When the CWBR is removed to give MR per se, they become lower, and more become negative. As a further test, I have then excluded estimates of the resistance from the V² effects, and the constant of MR, leaving mostly the sources of resistance due to piston thrusts and rings. Almost all of these remainder observations are thereby reduced to values so low that they imply implausibly low friction coefficients, ie that Rugby data are generally low."

"Properly called"? Only by a definition of your own creation, the absurdity of contriving a number that is incapable of verification by actual measurement, and discarding one that was is proper? Treating the driving wheels as passive objects is a fundamental conceptual error; I can see that this approach might help you find the number you first thought of. "My difficulty is that I think the Rugby data poor/inadequate," Your comments on the higher incidence of negative MR outcomes, having reduced same by deducting your assessment of CWBR seeks to undermine the Rugby data by implication. The reality is, as my Experimental Error paper shows: the lower the actual remainder between two given quantities at stated limits of uncertainty, the higher the statistical incidence of negative outcomes. As to; "the remainder observations are thereby reduced to values so low that they imply implausibly low friction coefficients": the corollary of this is that your own frictional assessments are too high. It is apparent from the Rugby WRHP data that most of the track ride losses incorporated in LR formulae (the B term, are absent when running on the test plant rollers. This is no surprise; when running on more solid foundations the track deflections, track bed deflections, and rail joint percussive losses are not encountered. "Axlebox heating was a very serious problem on the Vitry plant; it was greatly reduced when the roller-pedestals were mounted on large rubber pads, such as were incorporated at Rugby from the start, probably due to Vitry's experience. This was in fact giving the plant some of the elasticity of the track without its irregularities, which if kept small were not entirely harmful.' - Carling (my italics).

2. "D R Carling said that they ultimately got the (DP) answers right, but he did not say how that was done, nor how it was known the answers were correct. No mention is ever made, there or elsewhere, of using the proper dynamometer, that applying the braking on the plant, to check the DP measures."

The first sentence appears to imply Carling was hiding something. He was not appearing in a court of law. Absence of statement does not prove absence of action or deception. As far as I can see his only omission was a detail description of the mediating gear servo control mechanism and how a differential gearbox was fundamental to its function. In the Rugby Test Plant publicity brochure it is simply referred to as 'a special device'. This was an elegantly simple solution; mention of it could only have reinforced his case. Your second sentence again infers guilt by omission of action, and is nothing more than speculation that something never happened. Having designed and commissioned a number of control schemes for a variety of industrial processes, and control and protection schemes for high voltage generation and distribution networks, I can assure you that when trouble shooting, no stones are left unturned. Most problems prove routine; some can be quite challenging. You seem to regard the Rugby staff and the Amsler test engineers as a bunch of incompetents.

The metering of the hydraulic brakes (speed and torque) was primarily to facilitate the equalisation of work between the coupled wheels and the detection of slipping. Obviously it would be a useful, though approximate cross check with the Amsler dynamometer behaviour during the commissioning phase. However to describe the hydraulic brakes were the "proper dynamometer" to verify drawbar pull is optimistic. I don't have the figures for Rugby, but the similar Heenan and Froude brakes at Vitry were guaranteed at +/- 5%, actual performance was assessed at +/- 3%, someway short of the Amsler dynamometer performance inside the guaranteed +/- 1%. I have heard it suggested drawbar power could have been determined from the temperature rise and mass flow of the brake units cooling water. This is unlikely to have proved very accurate, aside from the obvious problems of thermometry and mass flow assessment; there would have been significant radiation losses from the brake unit bodies.

3. "Important, however, and not mentioned by Doug, Carling was clear that they damped to protect the recording devices from the effects of resonance, not to perfect DP readings. Doug places a very favourable gloss on all of that. He omits mention of the dashpot, which after oil was removed from it, had air in it, and the frequency and magnitude of the forces affecting the apparatus."

The Amsler dynamometer was a recording device, and the only one that potentially could be damaged by resonance. Such damage is unlikely to have left accuracy unaffected, thus there was every reason to protect it. For readers unfamiliar with the history of the Rugby test plant, the resonance problems at Vitry were cured by incorporating Bellville washers in the drawbar, as specified for the Rugby plant based on that experience. At the suggestion of LMS research department an oil filled dashpot, was added to provide additional damping; the proverbial belt and braces solution. The dashpot incorporated a controllable by-pass to regulate the damping effect. It was operated by a bell crank arrangement connected to the drawbar. For the commissioning tests a WD 2-10-0 was selected, two were used, the first having proved 'an old bag of bones'. Having no reciprocating balance, from the resonance standpoint, it was the severest test the plant was to encounter. The LMS research department had assumed the drawbar pull waveform was sinusoidal; the reality proved otherwise, the fore and aft wave forms of drawbar pull proving dissimilar in shape, and amplitude. As a consequence the dashpot, falsified the drawbar pull. Note that the falsification of drawbar pull was clearly apparent from the available instrumentation. Modifications to the dashpot, considerably enlarging the bypass capacity proved no solution. In his Newcomen Society paper Carling explained 'In the end they simply took the oil out of the damping dashpot and left it with air in it, which damped sufficiently to prevent any damage, had resonance ever occurred. Afterwards no trouble of that kind had arisen and they got their results right. Being wise after the event he considered that, had the whole dashpot system been suspended on the drawbar, not fixed to the foundations, it would have acted as an inertia damper, there could have been no falsification of mean pull. It would have involved a major engineering modification as was not justified.' Writing in 2005 Jim Jarvis recalled: ".... The oil was drained from the dashpot and care taken to check that no untoward effects arose. In the event, the revised drawbar &

dynamometer etc, characteristics avoided any significant disturbance even when the dashpot was made ineffective. It was considered that the plant-drawbar pull figures were accurate after the dashpot problem had been settled."

4. "The effect of the Belleville washers, air dashpot and mediating gear operating much more slowly that the fluctuating forces, must have regularly allowed the to and fro forces free rein, and at others resisted them."

The functioning of the mediating gear as explained above, operated in a 'measured' way; it was not compromised in any way by "fluctuating forces". The deflection of the Bellville washers was consistent as a function of load and instantaneous (Hooke's Law). The amplitudes were slight, typically within $^{1/}8$ ". Given the dashpot was filed with a compressible medium, air (a sealed unit –no hisses), it will have behaved in a similar fashion (Boyle's Law). In this form as a pneumatic damper with very small deflection and a huge clearance volume (in the relative sense), it probably achieved very little if anything. "I have a feeling that sometime subsequently the dashpot equipment was disconnected." - Jim Jarvis

Given the uncertainty of exactly what is being suggested above, perhaps I should point out that the drawbar pull and the dynamometer reaction are always equal and instantaneous.

5. "If the Belleville washers and the air dashpot kept up with the fluctuations, there would have been frequent short hisses from both, rather than sighing."

So that clinches it: the Bellville washers were not making the right noises!

"Of the many values in a considerable range of TSMR in the Rugby data for the various classes at any speed, which does Doug choose to be used as his TSMR, and why?"

I do not understand the question; obviously TSMR will vary according to speed, effort and locomotive type, such matters have to be determined on a case by case basis. The consistency I describe refers to the WRHP Willans lines at given speeds across separate test series with the same locomotive, or with different locomotives of the same type.

"Doug says that the measurement of WRHP (DP as a HP) was the simple product of drawbar pull and RPM, a process automatically recorded, monitored and controlled by a Mediating Gear under the control of a servo mechanism."

I am not saying the mediating gear controls the drawbar pull as you go on to infer. The mediating gear regulates the integrity of the measurement process by sustaining TDC, it measures "inch seconds" to self monitor its performance. Speed (rather than RPM), was determined by distance travelled over the test period. The work done was measured by the dynamometer integrator

"One test is simply to graph TSMR against PTTE. This reveals tremendous ranges in TSME for a given PTTE, and precious little repeatability Doug claims that the Rugby data possesses."

This is just a restatement of the small remainder problem in modified guise, so the scatter described is no surprise. In any event why, by inference, does the measured WRTE take all the blame? The TSMR scatter is the product of two uncertainties, not one, a compound error; moreover, Carling rated the dynamometer accuracy higher than the Farnbro indicating equipment which he put within +/- 3%. The consistency I describe is in the form of Willans lines – plots against steam rate. Carling thought this could be determined to within 1%, although it must be said that within this there were slight variations in pressure and temperature over the course of a test series, introducing an additional source of scatter to both IHP and WRHP.

Additional to Willans line however, where plots ranging from 29 to 45 are available of WRTE against ITE, as in the case of 73031, scatter is very low for 20, 30 45 and 65 mph. Given these data sets embrace enhanced superheat, de-superheated and part regulator working, thus involving variations in steam volume and cut-off, it is apparent that cut-off is of little significance, ITE is. Given that between 15 and 40 % cut-off, an increase of 260% occurs for an increase in valve travel of only 16%, this is no surprise.

From this data set the MF at 30mph, 688 and 1375 HP for a Black 5as tabled in your earlier letter works out at 600lb and 890 lb as against the 940 and 1420 lb shown. Given these differences are as high as 56 and 60%, the slight dimensional differences between a Black 5 and BR5 are immaterial. Since however your values represent an emasculated definition of MR the true discrepancies are even larger. Further evidence of a failure to match the empirical evidence can be found is Report L116, which includes a resistance curve for a 9F at 16,000 lb/hr steam rate as derived from constant speed road tests. At 30 mph, 1090 IHP, the LR was 134 HP, 1675 lb, as against the 1710 lb given for the Black 5 at 688 IHP, and by extrapolation at 1090 IHP the Black 5 LR works out at 1990 Lb, 19% higher than a 9F, notwithstanding the latter's 5 coupled axles as opposed to 3 and a coupled axle load 19% higher. As previously mentioned the plant test MF differences between the Crosti and Std 9F were confirmed in road tests. When asked to adjudicate on the test data for the standard and Crosti 9F, Chapelon concluded it was the most accurate locomotive test data he had seen.

In summary the supposed shortcomings of the Rugby Test plant, its designers and operators are groundless. The available experimental data demonstrates consistent repeatability over time and circumstance. Repeatability is a key indicator of metrological integrity. That is not to say everything is perfect and falls in place in place like a jig saw. Given the understood limits of experimental error, however small, and the random nature of scatter, the real world is more complicated. Exactly the same problems obtain when reconciling the data from road tests. Road tests have however confirmed the differences in test plant MR in the case of the Crosti and standard 9Fs. In other words the empirical evidence derived by different methods remains consistent. A key test of scientific proof is that its claims are consistent with the empirical evidence. The powers of the regression statistical process used by John Knowles fails the empirical test significantly and is thus unsound, supposed statistical integrity notwithstanding.

Yours sincerely,

Doug Landau

PS; I have only just seen John Knowles letter 21 February 2017 on the website, as at 7th March, and have not had time to study it as yet. – see below.

14 April 2017

Locomotive Resistance

This is in response to John Knowles letter 21st February. As in my previous responses the many points raised are not necessarily taken up in chronological order. Words in emboldened quotation marks are John's own, unless otherwise stated and with regular quote marks.

The first point is that in stating "D R Carling thought the Rugby testing plant would not yield satisfactory figures for the internal resistance of the locomotive", John omits to mention that Carling took exactly the same view of locomotive resistance (Model Engineer 17 November 1980), as I have previously pointed out. This does not mean he mistrusted the WRHP data

any more than for the IHP, rather less in fact: he was simply stating the inherent uncertainty of the small remainder problem and experimental error.

Carling's stated uncertainty for the IHP data was put higher than as for the WRHP: "Practically every instrument used at Rugby was checked in one way or another. A special calibrating device was used for the Amsler, of a kind used for testing large materials testing-machines, and the device itself was tested by the National Physical Laboratory. Amsler's guaranteed the measurement of pull within 1%; he had reason to believe it was it was well within the guarantee and that it was consistent to even finer limits. Work done was guaranteed to within $1^{1}/_{2}$ %, and the indication of power to within $2^{1}/_{2}$ %, but the derivation of power from the recording was to considerably closer limits. More difficult to quantify would be the accuracy of indicating; but, generally, the scatter of values for several sets of diagrams for any one test fell within, or very little outside, 3%." (Carling - Locomotive Testing Stations Part I; Newcomen Society Paper.).

The second paragraph again propounds his ideas on how the damping measures supposedly sent the dynamometer into some kind of a spin. I dealt with his ideas on the damping equipment and mediating gear comprehensively in my letter 7th March, no need to repeat my observations on how the damping equipment etc actually functioned here. I see he still thinks that coupled wheels are not part of the propulsive machinery; it's a wonder trains ever managed to move.

"After the modifications to lessen the value of DR about 1953, the damping was the result of:

- a) Air being sucked into a dashpot, compressed, and exhausted; this could in principle damp TF forces as they occurred. If the orifices were much the same as when oil was placed in the dashpot, it probably provided little damping, but if the air pressure built up before any release, it would have resulted in erratic effects.
- **b)** Belleville washers (sixteen pairs) which could dampen only at a constant rate, and were therefore unsuited to damping the forces and their pattern."

I don't know where John gets the idea that the abandonment of the oil damping dashpot, replacing it with air, did not occur until as late as 1953. Perhaps he seeks to use this date to correspond with the time when negative MF values became a rarity. The idea that it took 4 years to reach this decision is absurd; had it been so, many heads would surely have rolled in the meantime. The reality was that the problem was treated with some urgency during the tests with WD 2-10-0 73788 in 1949. As Jim Jarvis¹ recalled; "We all worked well into the night on at least one occasion. After waiting for stable conditions to exist, the damping bypass setting was altered, accompanied by a significant change in the

recorded pull on the Amsler table. In consequence the oil was drained from the dashpot, and care was taken to check that no untoward effects occurred." (Perhaps I should point out that my many Jim Jarvis quotations are taken from letters addressed to John, likewise citations given in respect of Ron Pocklington²)

At no point in JJ's correspondence can I find any reference the to a dashpot modification opening it to the atmosphere. It cannot have been built that way for obvious reasons.

The tests with WD 2-10-0 73788 took place in four episodes between 22.4 and 19.12.1949, amounting to 59 days and 46 test runs (the previous choice, 73799 having been declared unfit as an 'old bag of bones). The intervals were occupied by D49 62764 for indicator tests of the

Reidinger Poppet Valve gear. It is clear from JJ's comments above, that the damper was air filled by the time these WD tests were concluded.

"Indeed he (Carling) acknowledged that avoiding the effects of inappropriate damping would have required a complete redesign of the plant. That was not done, so Carling admitted in effect that the damping was not right after 1953" (note the spurious date).

He said no such thing; this is just a crude attempt to put words into Carling's mouth. What he actually said was (repeating my previous letter I'm afraid); "In the end they simply took the oil out of the damping dashpot and left it with air in it, which damped sufficiently to prevent any damage, had resonance ever occurred. Afterwards no trouble of that kind had arisen and they got their results right (my italics). Being wise after the event he considered that, had the whole dashpot system been suspended on the drawbar, not fixed to the foundations, it would have acted as an inertia damper, there could have been no falsification of mean pull. It would have involved a major engineering modification as was not justified."

The tests with B1 61353 involved three spells at Rugby, 1950/1. No indicating was carried out, comprehensive WRHP readings were recorded. A couple of Rugby Test Station drawings dated 6.4.1951 show a family WRHP curves Vs speed for steam rate, cut-off, and curves for WRHP (estimated) and WRTE at 18.000 lb/hr steam rate, plus IHP (estimated) and WRHP curves. Such drawings would hardly have been prepared with the damper problem unresolved.

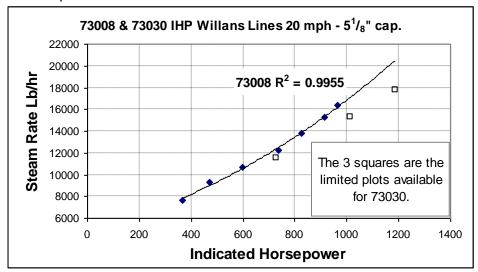
On point "b)", the nest of Bellville washers was adjustable to suit the test programme, as *Engineering* 19 November 1948 reported: "A crosshead at the front end of the dynamometer, Fig. 20, is pulled by a forked member which passes loosely through it; the fork passes through it a number of Bellville washers which act as "smoothing" springs. Some of these washers may be replaced by plain washers if it is desired to alter the compression modulus to suit locomotives of different masses, etc."

The assertion that the Rugby IHP data is "generally consistent" and survives his statistical rigours, whereas in contrast we are told, the WRHP/WRTE data fails it seems, on all counts-"very poor", is not a situation I am able to recognise from the available test data.

On the first count, consistency, the opposite is true. When IHP and WRHP or ITE and WRTE are plotted as Willans Lines, the R² values are uniformly high, typically approaching unity. This value is an index of scatter, a perfect outcome (no scatter) returning a value of 1. Various curve fitting options are provided by the Excel programme. The general shape of Willans lines is known from first principles; the chosen option for Willans Lines is a polynomial. Twenty five plots of IHP Willans lines randomly selected involving 46225, 70005, 73008/30 and 92013/250 returned an average R² value of 0.9853; the same exercise for WRHP Willans Lines returns a marginally higher value of 0.9888. Clearly, on this test, the WRHP data holds its own. Carling put the steam rate accuracy within 1%; even so there was some true scatter for given steam rates from test to test because there were slight variations in steam chest pressure and temperature. In a given circumstance of speed and cut-off, a reduction in pressure of 2lb might reduce IHP by about 1% (function of absolute pressure ratio). These high R² values are not in them- selves proof of accuracy, it is a measure of low scatter, more telling is the test of repeatability. Fixed or systematic calibration errors would not disturb the R² values.

When it comes to what might be the called 'handshake' test, plotting combigned data sets from tests separated by time, it is the IHP data that lacks consistency; the WRHP data consistently passes this test. The scope for such tests is constrained by the data available. Suitable IHP data pre and post the improvements to the Farnbro indicator introduced by Ron Pocklington is confined the tests with BR5s 73008 and 73030. In the latter case the suitable data is confined to the tests with the $5^{1}/_{8}$ " blastpipe. The scope for 73030 IHP data at a given

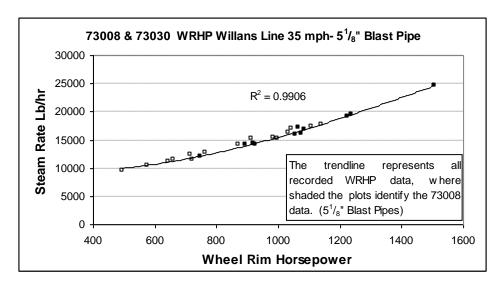
speed is confined to 3 test runs at 20 mph. There is more adequate WRHP data for both engines at 35 mph.



The 3 plots for 73030 (1953) trace a distinctly separate path to the earlier tests with 73008 (1951/52). Note the increased IHP for a given steam rate. A trend line for 73030 has not been fitted since the default resolution with only 3 plots is to return an optimistic R² value of 1.

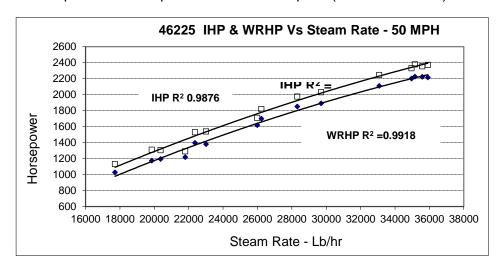
As mentioned in my previous letter, the test bulletin IHP data for the Britannia was uplifted relative to the actual experimental data. Apparently in recognition that the early IHP results where low: hardly an endorsement of "consistent" data.

The chart below demonstrates a firm WRHP "handshake" of consistency between separate test sequences for the same locomotive type at 35 mph. It spans the same time frame as the chart above. This is in clear contrast to the disparate IHP data.



In summary, the recorded WRHP data was consistent over time; many other examples could be given. This was not the case with the IHP data pre/post early 1953. The change here was clearly the outcome of improvements in the indicating equipment, the dashpot problems having been sorted long since by the end of 1949. Clearly repeatability is not in itself proof of accuracy, but it is an essential first step.

Plots of simultaneous IHP and WRHP data against the steam rate base demonstrate a clearly visible 'Master/Slave' relationship between the paired IHP & WRHP plots (see Chart below).



The WRHP plots are clearly sensitive to upward and downwards movements of the IHP plots. Note in this regard the obedience to this rule of the outlying set 4th from the left. This is an example of simple Boolean logic where B = A-x, where x is a variable as a function of effort; in practice both A and B are subject to random experimental error, hence the elasticity of uncertainty as manifested by the varying separations of the paired plots.

"Testing the Rugby Data - I) Examining the Damping at the Drawbar/Dynamometer Connection"

This section covering 1100 words is a further attempt to disparage the Rugby test plant setup and its operators. I have already dealt with the various misconceptions and inaccuracies on offer either above or in my earlier letter 7th March. I therefore see no need to cover this ground in detail again. I will just add that the claim in the last paragraph of "consistent" ITE data is curious given its demonstrated inconsistency. If such consistency is deemed the case with 73008 and other pre 1953 ITE data, the applied statistical tests are clearly un-sound. In contrast the pre 1953 ITE minus WRTE plots return negative MF values; after-wards when positive values emerge, the WRTE data has not shifted, unlike the ITE.

"II) Seeing Sense in the Data"

The arguments stated under this sub heading are not easy to follow given the opacity of the presentation and the surfeit of acronyms. The three steps, a, b, & c set out in an attempt to "test the data" are unsound. The difference between two measured quantities, ITE & WRTE, is reduced by the subtraction of two estimated quantities; the coupled wheel bearing resistance (CWBR) and the plant test tractive effort V² dynamic losses (PTTEV²), this being the losses attributable to rotating and reciprocating mass dynamic forces. This process effectively reduces the remainder from a measurement to the status of an estimate. There is no indication that and how the mitigation provided by competing force vector resultants that are less than their mathematical sum has been taken into account. The actual measured WRTE relative to ITE is discarded.

"That residual (the remainder) is such a small ratio of PTTES that the data imply improbably low Cfs (coefficients of friction) in the mechanism from the steam effects, often less than the lower set of Cfs."

The simple answer is that the two estimates and process in the exercise were wrong. The analysis of dynamic force effects is a complex matter. Far from being a Eureka moment, these results were a case for back to the drawing board.

"I consider this exercise shows Rugby TSR to be decidedly on the low side and erratic. Data on LR and MR from the rest of the world tends to justify the figures for MR hence TSR that I use, so I consider this exercise shows the Rugby TSR to be decidedly on the low side and erratic."

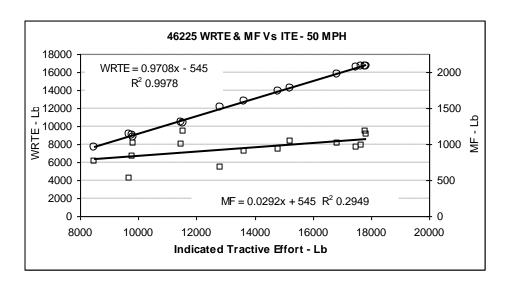
Given that the measured WRTE data from Rugby demonstrates high repeatability, the supposed erratic behaviour only emerges when subjected to the deduction of estimates. By implication said deductions are erratic and inaccurate. It is not clear what TSR is defining here: it surely cannot be the small remainder data; that, inevitably, is erratic.

Citing international LR data as back-up is unconvincing, said data is a minefield of disparity. If any threads can be found sufficient to detect a trend, it would just be one amongst many alternative trends available for use. Take your pick.

Below a plot of the Rugby data for 46225 at 50 mph; the only speed for which sufficient simultaneous values of IHP/ITE and WRHP/WRTE data are available (15 pairs).

Beyond the small remainder outcome (MF) this chart does not display the erratic nature of WRTE claimed by John, but then the values plotted are as measured, not the emasculated estimated values created in the pursuit of an untenable concept.

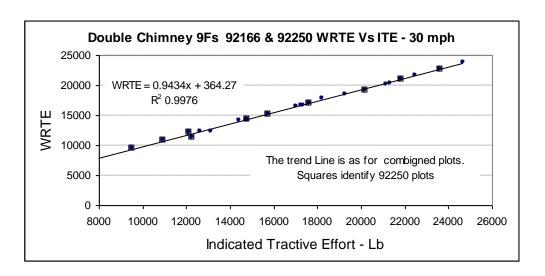
It is tempting to assume the negative value of 545 lb notionally represents the MR when coasting without steam. However in that situation some compression losses will occur in the cylinders, disturbing the projected mathematical trend from when under power. In the absence of said coasting losses, the projected constant would embrace the coupled wheel journal and windage losses and all the cylinder and motion frictional and dynamic losses. The sensitivity to effort implied in this example is 3%, about mid range of typical Rugby values yielded by similar plots; 1 to 5%. This sensitivity occurs on two basic counts; firstly real effects down to piston thrust on bearings and motion according to work rate, and likewise piston and valve ring pressurisation. Under the notional conditions of zero tractive effort, significant residual losses would remain; for example piston and valve friction would not fall to zero, likewise the dynamic losses. The losses attributable at given speeds are therefore x%ITE + constant n. Secondly, potentially false sensitivity and anomalous outcomes down to random scatter patterns, which in effect, falsify what might be dubbed the 'compass setting' by a degree or two.



The lower trend line represents the machinery friction. Remarkably, notwithstanding the low R² value, the formula is the exact inverse of the WRTE formula, returning the same MR values. This, however, is wholly exceptional; typically there is some mismatch between the formulae outcomes of such derivations. I can only think the scatter pattern of the MR plots in this instance is fortuitously balanced. This is far from the usual case, the raw MR plots (ITE – WRTE) are generally not suitable for the direct determination of MR, which in addition are often too limited in number for given speeds to obtain sensible relationships between ITE and WRTE. With just a few plots over a limited power range the scatter produce slopes in the wrong direction; MF seemingly an inverse function of effort.

Other problems are the sensitivity of trend lines to the plots coincident with the lowest and highest abscissa coordinates. This sensitivity can be examined by experimentally removing plots. In the case of the 46225 chart above, removing two plots from the low end increases the residual from 545 to 768 lb. The curve fitting programme and formulae so generated are a mathematical smoothing exercises, and therefore hostage to the randomness of the scatter pattern. Solitary plots at the start and finish of trend lines are strongly trend setting, especially if the nearest adjacent plot is somewhat distant. This can still occur with high plot numbers overall, especially in the case of Willans line polynomials. It is apparent that any formulae fitted by the excel programme that approximately coincide with theoretical expectations, as is the case of 46225 above, are fortuitous. Some more insight into this problem is examined below.

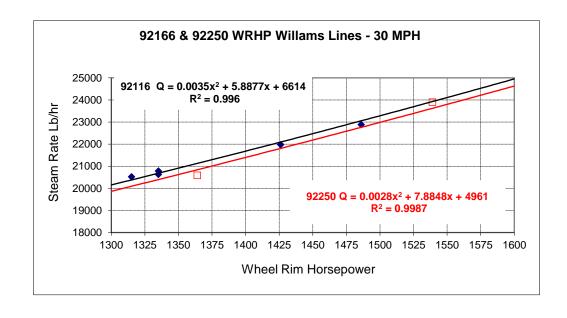
It is curious that John Knowles cites the data for 9F 92166 as encouraging, but condemns same for 92250; the plots of WRTE against ITE for both engines are effectively identical. Both were double chimney engines; 92166 was fitted with a mechanical stoker and $3^7/_8$ " blast pipe caps, 92250 4" caps.



The results for 92166 and 92250 were also plotted separately yielding the results tabled below. If any proof were needed that the Excel curve fitting programme formulae bear no relationship to the causal reality this is surely it. Note nevertheless the trivial difference in outcome these diverse coefficients and constants deliver.

Double Chimney 9Fs 92166 & 92250 WRTE @ 20,000 Lb ITE - 30mph								
Engine	Plots	R ²	Formula	20K ITE MR	MF HP			
			WRTE = $0.9525x +$					
92166	14	0.9978	192.69	757	60.6			
			WRTE = $0.9373x +$					
92250	10	0.9974	476.91	777	62.2			
			WRTE = $0.9434x +$					
92166/92250	24	0.9976	364.27	768	61.4			

Results for 73030 showed a fall in WRHP against steam rate as blastpipe diameter was progressively reduced in the pursuit of free steaming on Grade 2B coal: $5^{1}/8^{\circ}$, 5° and $4^{7}/8^{\circ}$ diameter. Given this phenomenon the outcome on this count was examined for 92166 and 92250. Plotted as separate WRHP Willans Lines over the full working range, the curves are so close as to appear as a single curve. Hence it was therefore necessary to focus on an enlargement as below to reveal the effect of reduced blastpipe caps as below. The penalty here for 92166 over the range shown is about 20 HP. The outcome for 73030 was similar. WRTE is a linear function of ITE; this is consistent with the Rugby data generally.



"Nothing is said about the purpose of the exercise set forth in the spreadsheet" (Experimental Error)

The simple answer is to inform. In that regard I believe that a few charts demonstrating the graphic outcome of the small remainder problem to be far more informative than its mathematical explanation. John Knowles seems unhappy that I have put this up for scrutiny, hence over 2000 words of general irrelevance seeking to pick holes in it. The spreadsheet as presented is straightforward enough, with clear caveats regarding its scope and simplification relative to actual test circumstances, so I will spend no time addressing these comments, other than those referring to the chart for 45722 plotting the machinery friction data recorded at Rugby. To refer to "more variation in (his) MF at each of the speeds" seems to imply the trend line is some kind of concoction on my part and questions the absence of a formula. The trend line is simply the product the excel curve programme, so is presumably the product of the least squares method for the data available. The formula is not necessarily accurate, given the uneven scatter, so is irrelevant. It does however, as the caption says; 'Notwithstanding the scatter, the trendline shown reflects a speed/ magnitude relationship roughly in linewith theoretical expectations.'

Note the word 'theoretical'. Back in 2004 I undertook a theoretical examination of the various elements contributing to locomotive machinery friction and the resulting outcome. The exercise was broken down into nine elements variously contributing to force, friction, dynamic effects, windage, simple harmonic motion etc. The forces were a matter of calculation, the masses known, but obviously the friction coefficients had to be assumed based on published data sheets, technical manuals and some rolling stock empirical data. The values adopted and method erred on the pessimistic. There was no input to this exercise from the Rugby test data or any other similar data. So it was coincidental when the first such exercise was of a similar magnitude to the Rugby data and dish shaped, further exercises for various locomotive types followed this similarity.

John Knowles is fully familiar with this work, so for him to say; "Doug has no idea of how such data might be interpreted and analysed. He should be trying to analyse what causes the variation at those speeds." is wholly disingenuous.

"Doug Landau's approach to the Rugby TSR data is in my view one of wishful thinking about its soundness and hopes of using it, and playing with figures to defend it." It con-tinues later on with great irony; "If the data are not satisfactory, no good can come playing with it."

Really? This is incongruous; throughout this correspondence I have simply reported and plotted the Rugby data as it exists, at no point have I 'played' with it, in direct contrast to the processes set out in "Seeing sense in the data."

"It was the view of D R Carling, Superintendent of the Rugby plant during its operating life during that the plant was not suitable to obtaining the internal resistance of locomotives. In saying that he referred to the SDE, but he also pointed out that the damping provided was to prevent resonance developing, not to provide accurate TSR; indeed it could not."

This bowdlerization of what Carling actually said and thought is not without its absurdity. If the dynamometer was damaged it wouldn't work accurately or even not at all would it? What Carling was talking about was the small remainder problem, not the dynamometer performance, of which he said (I repeat): "they got their results right". As previously cited, Carling considered the determination of locomotive resistance equally problematical because of the small remainder problem. If the scatter patterns of MR and LR data are considered as statistical crime scenes they share a common felon; Indicated Horsepower. John seems unable to acknowledge that IHP played any part in the Rugby MR data scatter.

"My difficulty is that I think the Rugby data poor/inadequate."

In summary, this view has not been supported by the arguments submitted.

- 1. The several supposed shortcomings of the Rugby Test plant set-up in regard to the Amsler dynamometer, have, one by one, been shown as inaccurate and often ill informed.
- 2. The inaccurate attributions to what Carling actually said, wrote and clearly thought can be dismissed as 'spin'
- 3. The various players in the design, manufacture, construction and operation of the Rugby test plant were not incompetent.
- 4. The suggested timescale for de-commissioning the damping dashpot is inaccurate.
- 5. The treatment of the coupled wheels as part of vehicle resistance is pointless, unsound, and degrades a measured quantity to the status of an estimate. This compromises any statistical analysis.
- 6. The consistency of the measured WRHP over time, in given circunstaces, sometimes with different locomotives of the same class, appears to have been disregarded.
- 7. The consistency of the IHP data has been overstated, and does not hold over the timescale involved.
- 8. "Seeing sense in the data": The procedures as described have manifestly sown chaos in places where it did not previously exist. Measurements of high consistency are usurped by a feast of needless, and by implication inaccurate estimates. No wonder improbable results follow.
- Given the controlled environment, the Rugby test station was better placed for the determination of MR than was the case with road tests in regard to LR. The test plant was not subject to the vagaries of wind, track condition and curvature.

Doug Landau

- 1. Jim Jarvis, as his elder brother Ron, were both LMS Derby engineering apprentices. Under BR Ron was promoted to Chief Technical, CM&E, Southern Region. He was in charge of all design work throughout the region, Based at Brighton, this involved the leading design work on the BR 4MT 4-6-0, the 4MT 2-6-4T and the 9F 2-10-0. He was later responsible for the Bulleid pacifics' rebuild design. Jim was assigned to the Rugby test plant from its earliest days, he is present in a photograph of the ceremonial opening and demonstration run with 60007 in 0ctober 1948. By 1951 one he was in the USA serving a two year scholarship with the Norfolk and Western, and attending Illinois University where he gained an MSc in mechanical engineering. On return to the UK he undertook the very successful design of the 9F balancing arrangements.
- 2. Brighton trained engineer Ron Pocklington was in charge of the Fanrbro indicator operation and development at Rugby. In the early days sensitivity

and mechanical reliability was poor, and the electrical circuitry was troublesome in various ways. Progressively, improvements were introduced and problems eliminated. In its final state the indicator pressure diaphragm was sensitive to "the slightest breath applied to the steam inlet could make and break the contact." Exact date unknown.