Locomotive Resistance – Reply to John Knowles' Letter November 2020

As previously, emboldened words in inverted commas are John Knowles' own. The many points raised may not follow the same chronological order as his letter. Some of his themes keep reappearing. In particular a seeming obsession with unbalanced masses, which in reality were only of minor significance.

Critical Speeds

"Doug seems to think that it was the result of high traction forces, by which I think he means high piston thrusts at low speeds which caused falsification." "…it was considered that damping falsified the readings"

Not so. The falsification was entirely down to malfunction of the dashpots, not damping per se. A problem not confined to particular speeds. The drawbar pull was observed to increase with the engine working in steady state conditions. At one time the idea was mooted by the test staff that the problem arose because the drawbar pull did not follow the expected sine wave. Numerous dashpot modifications and experiments reduced the falsification, but failed to eliminate the problem and the dashpots were decommissioned by the end of 1950. Experience had proved the dashpots were an unnecessary over precaution. A dysfunctional belt to the braces provided by the Belleville Washers.

The 'Critical speed' problem was attributed to high and asymmetric traction forces at low speed when the frequency coincided with the natural frequency of the test plant. The very high unbalanced reciprocating mass forces at speed were regarded of trivial consequence, notwithstanding these forces could substantially exceed tractive effort, as enlarged upon below.

On Test Run 126, 7th November 1949, WD 2-10-0 73788 (no reciprocating balance), was run up to 45mph with the dashpot drained of oil. Slipping curtailed tests below 30 mph. The peak transient accelerating force vector on the locomotive mass per cylinder was 0.2G. No problems encountered.

Dynamic Forces in the Machinery Resistance

"An alternating force along the locomotive and train through the drawbar, the result of unbalanced reciprocating masses in the mechanism. Work is necessary to create this force."

The first point is that on the test plant, the train and tender are absent and the locomotive is stationary. He has previously suggested the vibrations generated would disturb the accuracy of, and possibly damage the Amsler dynamometer. No such damage is on record. The frictional losses resulting from the centrifugal force loadings of the reciprocating masses on the various motion crankpins, are the same whether balanced or unbalanced. Obviously, these frictional losses would diminish the available drawbar pull accordingly, as do any other frictional losses in the power transmission machinery along the way. The accelerating forces applied to these masses cancel out, both hindering and helping in the course of each revolution.

An alternating shuffling force however, will obtain as suggested, and this can indeed substantially exceed the tractive effort, but the maximum horizontal reversing force vectors arising from imbalance only occur fleetingly in the course of a revolution. High though these momentary forces are, the available acceleration G forces are low relative to the locomotive mass. The imbalance shuffling effect was of little practical concern under all normal operating conditions. In the unlikely event that these reversing forces did not exactly cancel out, the slight net effect on tractive effort, positive or negative, would be real, not some kind of falsification as appears to be suggested.

Unbalanced Reciprocating Mass Force per Cylinder

Class	Coupled Wheels "	Stroke " d	Unbalanced Mass Lb	Percent Balance	MPH	RPM	Force lb	Loco Mass lb	Loco Mass/ Force Ratio
9F	60	28	519	40%	15 30 60	84 168 336	1,448 5,791 23,166	198,240	136.9 34.2 8.6
WD 2-10-0	56.5	28	1298	Nil	15 30 45	89 178 268	4,089 16,358 37,081	175,392	42.9 10.7 4.7
Black 5	72	28	467	50%	30 60 104	15 29 52	3,641 14,564 44,968	161,504	44.4 11.1 3.6

$F = (W\varpi^2 r/g)\ \mathrm{Cos}\ \theta$ ϖ = Angular velocity radians per second. r = Crank radius in feet.

The maximum transient resultant force vector for the 2 cylinders is □F x 0.8325

In 1939 the LMSR conducted some stationary slipping tests involving three Black Fives with 66.6, 50 (as per standard) and 30 percent reciprocating balance. At 104 mph the oscillations (shuffling) of the standard engine were described as ‘Moderate’, ‘Excessive’ for the 30% engine at 99 mph, and ‘Nothing abnormal’; for 66.6% at 103 mph. Vertically the situation was more serious, with the coupled wheels of the 66.6% and 50% engines momentarily leaving the track once per revolution, and actually justifying for once that horribly over-egged term “hammer blow”. In the USA it was more sensibly described as “axle-load augment”, a smoothly approaching maximum value in all normal circumstances. No sudden shocks or sledge hammers involved.

All this is covered in a long paper to the by E S Cox to the Institution of Civil Engineers in 1941: *Balancing of Locomotive Reciprocating Parts.*

Among the general conclusions was “… the additional variation in drawbar pull (traction) is very small and can be ignored at anything but very slow speeds.”

Unbalanced reciprocating mass dynamic forces were transient and adequately suppressed by the locomotive mass under all normal operating conditions. At the highest speeds the accelerating G forces remain fractional. Reciprocating mass as a function of locomotive mass tended to reduce as the latter increased.

Length was also a factor. The Stanier 2-6-4 tanks, weighed in at 87.85 tons, with 5’9” coupled wheels and 66.6% reciprocating balance as standard. The wheelbase at 37’ 1”, was one inch longer than a Duchess. One example had been running for eight years without any reciprocating balance; “No adverse report has ever been made on its riding”.

Throughout this long correspondence John Knowles has been much exercised by his ideas on unbalanced reciprocating masses, with a hand wringing innuendo that only he had properly considered these matters, and the Rugby test plant was by implication a design and operational fiasco. The reality was otherwise; his understanding is based on little more than flawed opinion rather than thorough research. His long explanations of imbalance affects, give insufficient significance to their transient reversing nature and the inertial mass of the locomotive. In summary, his concerns are yet another red herring.

Why he continues to dwell on the performance of the dashpots is a puzzle. They proved wholly unfit for the job, notwithstanding considerable attempts to rectify, and in the end proved unnecessary. They played no part in the later test results that have been subject to challenge.

At the end of this letter is an appendix showing that the design and operating circumstances of the Rugby Test Plant in regard to traction and reciprocating forces were thoroughly understood and accommodated with due diligence.

“Even if the damping devices could absorb 75% and above at a certain speed, the remainder are still considerable.”

Not so, for reason of the inertial reluctance as explained in detail above. Ironically, he does recognise this effect: with his own words; **“resisted by the inertia mass of the locomotive”,** but brushes its significance aside with all the authority of ill informed opinion. The dynamic G force vectors are both fractional and transient!

“I know of no record of the damping employed in particular tests”

Not much information is available to me, more probably exists at NRM. It is however apparent that the number of Belleville washers deployed varied from test to test depending on the locomotive type involved and the planned speed and work rate. Test run 156, 19th January 1950.with Black 5 45218 for example, involved 3 pairs of Belleville washers, dashpot drained of oil.

The Belleville washer hysteresis was very low. The theoretical critical speeds plotted for the BR7 in my last letter (Figure 2) was based on tabulated results. The formula given as: Critical Speed (mph) = V = 4.26 Sq.Rt K. For K, “$ee report 1949/276.” The natural frequency of the plant was low. The disturbing forces of unbalanced masses were insufficient at low speeds given the locomotive’s inertial mass, to set up any resonance, at higher speeds the frequencies were out of step. The reversing acceleration forces of the unbalanced reciprocating masses cancel out to zero.

Any parasitic motion of the locomotive resulting from imbalance effects cannot falsify the *actual drawbar pull*. At any given moment, that pull is the reality, net of all the manifold frictional and dynamic work done losses of the power transmission system, by whatever cause. The force encountered by the dynamometer will be equal and simultaneous.

Water Brake Dynamometers

“The Rugby TS also had water dynamometers, …… but Rugby preferred to obtain what it considered to be the WRTE for the whole locomotive at the drawbar hydraulic dynamometer, because it was easier….. Many powers and efforts are accurately obtained through change in water temperature.”

The prime function of the Heenan and Froude water brakes was to provide a load. Each roller was metered so they could be adjusted to provide a similar load for each coupled wheel. Exact similarity of load was not considered critical. The method of measurement and control was by metering the torque (measured between the water brake stator and rotor paddles), and adjusting a water control valve. The accuracy was nominally +/- 5% minus an unknown missing quantity. As configured the frictional losses of the $10^1/_4$” roller bearing journals of the rollers were by-passed and not picked up by the dynamometers. This being so the water brakes were recording losses relative to a quasi-twelve-coupled engine when a six coupled was on the rollers and no less than a quasi-twenty-coupled when it was a ten coupled. The 4’ 9” roller mechanical advantage (MA) to the bearing rollers pitch circle was low at 4.23. This compares to the MA of 8.7 for a BR5 and 8.1 for a Duchess (plain bearings). That is not the end of it, the extension shaft to the dynamometer has two further bearings with losses not picked up by the dynamometer. They were however under lower vertical loads and of lighter construction.

Water brake dynamometers can indeed function by the determination of water flow and temperature rise as suggested, but for the Rugby Test Plant set-up this would have in involved lagging the dynamometers. Not a very practicable proposition. They would however be second hand measurements based on the conservation of energy principle. The losses of the roller journals, as described above, would still have escaped measurement. It would have been quite an involved experiment to set up a rig in the manufacturer's works to determine these losses under simulated working conditions. Such an operation apparently was not thought worth the trouble, since the function of WRTE determination was entrusted to the Amsler dynamometer. The likelihood that the Froude system could be more accurate would remain very low. The Amsler, along with its servo mediating mechanism, remained essential to hold the coupled wheels over top dead centre. So, all in all; not a sensible proposition.

The application of water brake dynamometers to IC engine testing is considerably more straightforward than a loco test plant, with a simple direct coupling of the prime mover and dynamometer drive shafts. Froude water brake dynamometers are still going strong, and still function by torque measurement and not water temperature rise.

"…..the Rsqd he uses in many graphs is misleading or wrong, as in purely demonstrative (Figs. 7, 8, and 9 are not causal), "

The determination of R^2 relationships is the statistical "least squares" mathematical outcome of the data provided, so how is that 'wrong' in the pure mathematical sense? High values are indicative of low scatter, a desirable characteristic of test data more likely to reveal relationships than chaotic scatter such as routinely occur in small remainder outcomes. Obviously high R^2 values are not proof of accuracy, a poorly calibrated instrument may perform with respectable consistency. Another potential polynomial curve fitting hazard is using insufficient decimal places as previously exampled in my December 2019 letter; significant mathematical errors may occur.

To say the relationships displayed in the graphs cited is not 'causal' seems something of a moot point. It is apparent that 'No steam' = 'No horsepower', but since the relationship is clearly not linear, some underlying factors beyond Q must also obtain. It is a 2nd hand relationship. The shape of the Willans lines (Fig.7), indicate a trend of initially rising then falling efficiency over the working range.

"**WRHP against speed on a graph does not explain anything."**

Really? Figure 11 for example, shows a very tidy set a WRHP Willans Lines at various speeds fully reflecting the expected pattern given the inherent characteristics of IHP v speed efficiency trends and Willans Lines. Such order was something the related data for the IHP Willans Lines seems to have been unable to replicate at the period of the test plant history.

Some General Points

The prolific use of 9F machinery friction data at 30 mph in my last letter was simply because most data was available at that speed, and involved 3 individual 9Fs and 5 test series. Close agreement of the WRTE v ITE outcomes was evident, indicating degrees of uncertainty falling within the Amsler contractual guarantee. In addition, the 26 test runs for Crosti 9F 92023 at 30 mph were also predominant. The irrational 9F WRTE positive constant only occurred in the instance of 92166, hence the weeding of some data as explained above. Data below 14,000 lb/hr was not available for 92166.

The linear relationship of WRTE v ITE unequivocally obtained for all other 9Fs at other speeds examined along with negative constants, as it did other locomotive types tested at Rugby. A similar relationship is evident from the limited test data available from the Vitry test plant for EST 241-004 at 37, 50 and 75 mph, which returned 3 parallel lines.

As demonstrated in my last letter, the MF outcomes for 3 9Fs and 5 test series were within 1% in the middle power range, notwithstanding variations in the formulae coefficients obtaining (Figure 30). The latter are highly sensitive to the random variations in the scatter of the individual data sets.

The variable coefficient representing effort sensitivity generally falls within the 2 to 3% range. When the variable is lower, a higher negative constant follows, as the number crunching juggles with the scatter, and vice versa. The sensitivity of the linear slope and therefore the variable to the plots at the extremities of the plotted range is high. In his *Tribology and Lubrication,* L D Porta put the frictional losses sensitivity in the 2 to 3% range.

It is notable that such a mechanically determined iteration of the complex shifting force vectors, cross couples, dynamic effects, windage and the shifting resultant frictional loadings in the course of each revolution should resolve into such a simple relationship as a function of ITE.

$$\text{WRTE} = \text{ITE}x - C$$

Where x is analogous to the overall frictional sensitivity as a function of effort, and C is a variable constant as a function of speed or RPM and among other things. The R^2 values for these plots uniformly approach unity. By comparison the small remainder derived machinery friction outcomes are a randomised confusion of reality about as useful as bingo results. To flatter them with statistical analyses as sources of scientific revalation is ridiculous.

"He makes a big thing out of elimination observations to improve the Rsqd. already 0.99, although no other statistical tests are given. Rsqd. being the only test he uses"

No so, the improvement in R^2 values was purely coincidental, not the aim. The statistical test was the elimination of a positive constant, which was a technical impossibility, thus changing it to a negative sign. It was a process of irrefutable logic unhampered by mindless dogma. The rest of the paragraph that precedes and follows the quotation above is a misrepresentation of what I have said and shown. The positive constant was a solitary aberration attributable to the hazards of random scatter among numerous satisfactory date sets. Linear trend line outcomes with a single variable are not within my control, it's simply the way the plots resolve. The various Willans Lines plotted are quadratics.

"Figure 5 has MR falling from 15 mph to almost zero at 75. A little thought will show that this is rubbish."

The explanatory caption below Figure 5 notes: "The overall trend, clearly and illogically, is saying that MF is an inverse function of speed." It was presented as an example of the troublesome experimental outcomes that were evident at that stage of the test plant history. JK's comment adds nothing.

This, as are the bulk of my presentations, was a demonstration of what the empirical data was showing, warts and all.

“The things to use to try to explain TSAMR are at least and at first, piston thrusts and speed squared, because from first principles they are the major influences on it.”

The iteration and resolution of forces: static, traction, dynamic, inertial, frictional and various vector shifts of the locomotive power transmission are far more complex than John Knowles assumes above, and cannot be reliably dissected. In the absence of the complex force diagrams and numerous complex iterations involved, he falls a long way short of the standards of presentation, diagrams and analysis that would pass muster in any design office. To work from first principles, it is first advisable to understand them, and most important, be aware of what is unknown or uncertain. The net significance of piston thrust is far less than he seems to suggest.

The abstract below is a remarkable example yet another fallacious concept, presumably derived from JK’s supposed “First Principles.”

“On a Testing Station like Rugby, there are problems in measuring the WRTE, which is in principle, DP deducted from the ITE to give MR. At Rugby, this was measured not at the CW rims, but by a dynamometer at the end of the drawbar, on the assumption that the pull on there (DP) ant the WRTE are equal. This assumption was obviously not true/ The CWBR, usually considered part of the Vehicle Resistance, occurs between the ITE and the DP, and its constant value can be calculated, from the bearing dimensions, the mass borne by them, the resulting pressure, the appropriate coefficient of friction (Cf), for that pressure, and the ratio of bearing diameter to the CW diameter, and should be deducted. So long as this is understood, it matters little whether the CWBR constant is deducted from the raw data or from the regression result. In addition, at Rugby, an unknown DR was incorporated between the ITE and DP.”

“The unknown DR between ITE and DP” did not and could not exist. On the test plant, under steady state conditions, that being running at constant speed, the wheel rim tractive effort and the drawbar tractive effort will always be exactly the same. It cannot be otherwise. It there was a mismatch constant speed stability would no longer obtain. The DP is the passive slave of the WRTE, and vanishes should a slipping incident occur. The forces encountered by the dynamometer and the drawbar and coupled wheels are inevitably equal and opposite at all times under constant speed conditions. John Knowles’ “unknown DR” affecting MF is just another of his several fantasies

“No tests were conducted at Rugby with all the reciprocating masses balanced solely for the tests. and the DR eliminated.”

DR, Drawbar Resistance, a supposedly interposing disturbance of the drawbar pull when encountered at dynamometer cannot exist as explained above. What does not exist cannot be eliminated. Reciprocating balance brings with it its own problems which is why engineers sought to keep the percentage balance as low as practicable. The tests with full balance suggested would be pointless

The next part of his submission turns to his supposed scientific analysis. It begins with: **“1. The TSAMR is so low in itself, and does not accord in any respect with the factors which should, from first principles explain it.”**

He goes on to adopt the test data for 9F 92250 as his choice for statistical analysis on the grounds that as the last locomotive tested on the plant **“the testing procedures should have been as perfected as they were to be”.** The data presented comprises four small remainder MF sets at 20, 30, 40 & 50 mph.

He goes on, inter alia: **“Four** (of the MF small remainders) **are less than the expected constant value of CWBR of 228lb.”**

As my randomised small remainder experiments have clearly demonstrated, the range of small remainder scatter evident in the 9F test data falls within the predicted range of possibility given the understood metrological limitations, and that includes the two negative MF outcomes. Less than 228 lb is of no statistical significance within this realm of possibility

More significant, the constant CWBR referred to is fundamentally flawed on two counts. Firstly, it is not constant across the speed range, the friction coefficient will increase as function of speed as I explained in my last letter: □□= ZN/P. Secondly the axle load and piston thrust force vectors are in directional misalignment, with the latter constantly changing in magnitude and angularity, constantly changing the resultant force resolution. Thus, the resultant bearing stress and friction will be less than the mathematical sum of the forces involved.

In other words, there will be significant mitigation of the losses involved. Lomonosof, who was no slouch when it came to complex analysis, hesitated when it came resolving this complex iteration mathematically.

“…..experiments that turn out badly should not have been recorded.”

How can an experiment be deemed to have turned out badly until its results are determined? Was “rejected”, rather than “not recorded” the intended meaning?

Surely negative machinery friction small remainders are failed experiments, but they remain in the data he adopts for statistical analysis. They would certainly pass the Grubbs Test justifying outlier elimination (not available in the 1950s), but simple logic alone is sufficient in such circumstances. Given the vestigial R^2 values of MF small remainder data sets, subjecting such data to regressions can be guaranteed to return unfavourable statistical outcomes. In the past John Knowles has opined to me personally that for a data set to worth looking at, the R^2 value should be at least 0.4 and the higher the better; the degree of scatter and inconsistency being inversely proportional the R^2 value. In this regard the options available for analysis stack up as below as exampled by the Graphs as below that appeared in my last letter.

Figure12. 9F MF Small Reminder MF Outcomes – All Speeds R^2 0.0042

Figure 13 9F Mechanical Efficiency - All Speeds R^2 0.4090

Figure 46 9F WRTE v ITE - 30 mph R^2 0.9974

The three outcomes above involve the same data set. The second option increases the R^2 value almost one hundredfold and the third well over two hundredfold. The sensible matrix for scrutiny seems rather obvious.

“Equations for TSAMR in lbs”

“(1) -73 + .0417; Rsqd , 285, SEE 279, tI -043, t 24.81
(2) 498 +.0145 ITE; …….” Etc, etc.

The above runs to 12 formulae, each of differing make-up, The explanatory notes are poor, I have no idea what the first number in each set represents for example. No matter, it is apparent from the preamble that the data under examination is the small remainder data set; about as useful as the results on a Bingo night. Again, the spurious constant, CWBR 228lb is referred to, and, lo and behold, the non-existent DR from the world of fantasy appears in the text rated at 172lb! The whole exercise is worthless. It is notable that he ascribes any apparent MR anomalies or improbabilities that fall out of his flawed analytical approach as solely attributable to dynamometer error. As the difference between two large numbers both subject to randomised scatter, it is witless. This implies the ITE data was perfect and totally devoid of scatter. The division of supposed error cannot possibly be sensibly determined by such a fundamentally flawed procedure.

"D R Carling, Superintending Engineer of the Rugby Plant, considered that what the plant registered was DBTE and not ITE."

What is this is supposed to mean? Both values were determined at Rugby, assuming DBTE refers to Plant DBTE, in other words, WTRE. ITE is determined from the measured IHP at the cylinders, so inevitably the effort at the drawbar and rollers is reduced. If as stated, Carling was simply stating fact.

"He considered that the ITE recorded there was suspect."

A citation is needed here. I cannot recall or trace such a statement from Carling. Interesting if true, he would have effectively been calling the measured cylinder performance into question. He did however say the indicator "spread of values was in the order of 3%", and "the mean values for each test was probably within 2%.". In relation to comparative tests with the Derby version of the Farnboro' indicator and the two mechanical indicator types used by Swindon, he did express regret that the test were only conducted up to 60 mph, and that systematic errors "might have become apparent at 80 or 90 mph." Something he thought more likely to afflict mechanical indicators.

If "ITE" was intended to mean the WRTE Carling simply said "We got the results right."

"What happened in May – June 1967?" Obviously 1957 was intended.

The Perform Programme

I am criticised for questioning the use and veracity of *Perform* as a tool for dissecting the Rugby experimental data, apparently in the guise of piston thrust determination. For a start this was perfectly possible before the advent of *Perform*, and simply done using the recorded test plant data. The substantive problem is that the *Perform* and test plant outcomes are routinely disparate, they cannot both be correct. Take your pick. John Knowles has consistently maintained the Rugby IHP data as sound (contrary to some evident conflictions), almost it seems, to the point of being unimpeachable. The published estimates for *Perform* as cited do not replicate the test plant in regard to both steam rate and IHP at a given speed and cut-off, with the results falling above and below the test data. The steam rate estimates are all out of step and by too much to be explained by the minutiae of valve setting. Carling considered the water and steam rate data as better than 1%.

The steam rate outcomes are at variance ranging from -27.9 to 6.7% with an overall negative trend averaging -5.7%. In these circumstances the test plant IHP data is unlikely to be replicated. The closest, within 2.3%, 500 Lb/hr, was the BR7. On the basis of IHP specific steam consumption, that would be worth nearly 40 HP. The *Perform* programme somehow contrives to make it 130 HP. The average SSC for 8 types on the plant is 14.27 and 13.85 lb/IHP.hr using *Perforrn*; a difference of -4.4%. Not bad perhaps, but too large in the context of machinery friction determination. None of the *Perform* estimates come closer than 4%. Plant and *Perform* cannot both be right.

The Crosti 9F

"It is almost standard for those writing about these engines to say that they had higher TSAR then the standard 9Fs, on account of reduced depth of frame stretchers, to fit in the preheater drum below the boiler barrel. That is not why I think they had higher TSAR, it is because they had very restricted blast nozzles in the chimneys, which create very high back pressure in the cylinders, as can be seen using Perform, and calculating the piston thrusts."

The evidence that clearly contradicts his conclusion, has been ignored. As demonstrated by Figures 45 and 46 in my last letter showing that, notwithstanding the significantly higher back pressure of 9F 92250 in double chimney guise compared to the Giesel ejector application, no discernable difference in WRTE at a given Indicated Tractive Effort was apparent. (The area of the Giesel blast arrangement by the way is 30.2 sq.in. not 302 as was shown). For the record, while the difference in back pressure at an ITE of 28,000lb for 92250 with double chimney and Giesel ejector was about 5lb, there was only a 2lb difference between single chimney 92050 and Crosti 92023 at this work rate. Any increase in back pressure would *reduce* the net piston thrust and frictional implications, not increase it.

It is evident from the numerous examples in the Rugby data that WRTE resolves into the simple relationship WRTE = ITEx – C, the constant C varying with speed. Coefficient x, the factor responding to effort sensitivity mostly falls within the 2 to 3% range and the formula routinely resolves into a linear trend line. The limited Vitry data displays similar characteristics. This variable is a summation of all the frictional sensitivities subject to the forces of effort, weight and dynamic effects obtaining. These are subject to the complex resultant mitigations of apposing forces such as the constant vertical coupled wheel axle load, piston thrusts (at moments opposing) and the shifting force vectors of the dynamic masses and cross couples. It's all a rather complicated resolution of forces which the power transmission system, without any resort to mathematics or computation, resolves and delivers a single and equal force at the drawbar and wheel rims. It cannot do anything else.

It is worth noting in regard to coupled wheel bearing stresses, that they are similar to those for passive wheels or even slightly higher; for example, 255lb/sq.in. for the Duchess coupled axleboxes as against 221lb/sq.in., for the trailing truck. The expected augmentation of bearing stresses when under power, are evidently relatively insignificant. An exception is bogie and pony wheels which routinely involve lower bearing stresses, 150lb/sq.in being typical. These axles are subject to significant lateral forces.

Small Remainder Regressions

A statistical fiasco, spurious outcomes guaranteed.

"With Fig, 17 the reader is informed that WRTE is linear, but that is not said in the headline , and is not shown to be universally so, being exemplified only at 30 mph. As ITE at a given steam rate is not linear with speed, and as MR is composed of elements which are not linear with speed, such linearity would be surprising, indeed wrong."

The main substance of the second sentence is correct, but that is the very reason Figure 17 was at constant speed, so it is not wrong or misleading. The linear relationship holds at any given speed, with the constant increasing with speed. Numerous examples are available from across the Rugby data. This relationship, in a different setting, appears in Figures 21, 22, & 29 of my December 2019 letter. Several components of machinery friction at a given speed are constant independent of effort, hence the constant coefficient.

"He should also calculate MR from first principles based on its causes or sources, I have done that."

I did that back in 2005, of which he is fully aware, having been informed of my analysis as it evolved. Such exercises can only be estimates amounting to "the likely order of magnitude". My process involved the summation of nine factors, each individually sensitive to the nature the of forces and losses involved in various ways. While the determination of forces, bearing stresses, dynamic loads and so on can be readily obtained, the selection of friction coefficients had to fall on the available empirical evidence and technical data sheets. The idea was to err on the pessimistic; I had no idea on how the numbers would stack up. The first runs came out in hundreds of pounds, not thousands, in other words similar to the Rugby data. A number of engine types were examined. In the light of more detailed analysis of the Rugby I would now approach such an exercise a bit differently.

I have no knowledge of the MR estimates claimed by John; given his serial mechanical misconceptions they seem unlikely to reflect reality.

"I consider (based on all the evidence he says and does in these posts, especially the claims he makes) that Doug Landau is not competent in technical and statistical terms to derive MR from the Rugby data or to express any judgment on the value of that data for such a purpose. He seems to believe that MR is a function of ITE. …. his view has not been refereed and will mislead others.

MR = ITE – WRTE; nothing more nothing less. I think I can manage the maths.
Of course, in essence, MR is a function of ITE: When ITE = 0, then MR - 0.
I think what he means is that it is not the sole cause of MR which is perfectly correct, but that is not what I say or the data says. The sensitivity to ITE is solely a function of the first term of a very simple equation; The negative constant second term (variable with speed), represents the manifold losses independent of effort. This is just another episode of misspeak and misrepresentation. On the refereeing point I'll leave that for the moment.

Conclusions

I'm afraid an appraisal of John Knowles latest contribution to this correspondence can be nothing but harsh. It is not a situation that would be willingly chosen. He just digs deeper into a charade of serial misconceptions, cursory scrutiny, and a misguided and erroneous statistical approach posturing as science. His request to be refereed could not survive informed scrutiny. He might even consider it to have been already done, likewise the suggestion that I have not been refereed. When I gave for comment my last contribution, December 2019, and this one, along with copies of John Knowles' submissions including the most recent to Fred Rich C.Eng., M.I.Mech,E, he opined: "Well that demolishes John Knowles." Fred's career started out an engineering apprentice at Brighton Works, he worked at the Rugby Test Plant from June 1957 to October 1958. His summary view of John Knowles' submissions was dismissive.

Perhaps Andre Chapelon could have the last word when he said of the Rugby test data for the 9F and Crosti 9F; "The most consistent and accurate in his experience": *Riddles and the 9Fs*, Colonel H B C Rogers, Ian Alan, 1982. That is not to say the Rugby test data constitutes an impeccable anomaly free record, it was far from that. Much time was expended in the earliest days trying to achieve dashpot functionality, an endeavour that ultimately proved futile. Fortunately, its abandonment was of no operational consequence. The evolution of the Farnboro' indicator to a satisfactory level of performance and reliability was a protracted process with setbacks along the way. Ultimately the expected standards of all round accuracy were not consistently achieved until nearly half way through the plants operational history, excluding the early commissioning period. Even then the occasional hiccup could occur. Accuracies within 1% are impressive, about as good as then possible, but that's still enough potentially to significantly alter outcomes, especially small remainders, if it’s a systemic error of constant sign. Thus, that some uncertainty remains is in the very nature of such tests.

Doug Landau

18 February 2021

Appendix 1

Introductory Note
Considerations of potential plant resonance were evaluated in the early stages of the test plant project. An early report on this topic is as below (my copy incomplete, date and authorship missing, some clipping).

THE REPORT OF THE OSCILLATING FORCES APPLIED TO A STATIONARY TEST PLANT BY A STEAM LOCOMOTIVE IN MOTION

Due to variation in traction force in the course of a revolution.
Due to the forces set up by the unbalanced reciprocating parts.

Introduction

This memorandum contains an investigation of the vibrations that may be set up in various parts of the dynamometric equipment of the Locomotive Testing Station about to be built by the L.M.S.R. and L.N.E.R. companies at Rugby.

In such a plant the locomotive with its wheels running on rollers is attached by a drawbar to a hydraulic dynamometer, the latter measuring and recording the tractive effort exerted. The plant forms a composite elastic system and is, therefore, capable of being set into vibration by any disturbing force. By reason of the unbalanced reciprocating parts, a periodic disturbing force operates while the locomotive is in motion, and, therefore, the whole plant will execute forced vibrations.

Apart from the general desirability of investigating the possible magnitude of these vibrations, another reason was provided by the experience of the French Railway Companies’ plant at Vitry. When this plant was built, no means of damping the vibrations was incorporated, nor apparently was the possibility considered that the disturbing force might at some speed be of the same frequency as the natural oscillation of the plant.

During the early tests these conditions of resonance were actually attained with the result the whole plant was suddenly thrown into violent oscillation, the trouble was later removed by the introduction of a damping system (Belleville Washers).

Summary

The general nature and magnitude of the alternating forces in action upon a steam locomotive in motion are briefly considered. They consist of two principal components:

The first varies above and below a steady mean value, but always in one, direction whereas the second varies from maximum positive to maximum negative. once in each revolution of the coupled wheels. It is well known that the second component can often reach a value many times in excess of the steady mean tractive effort.

It is matter of common experience that these large variations are not to a train or recorded in the dynamometer car, and it can be proved that they have no significance in the steady

motion of the train. The explanation lies in the fact that the frequency of the disturbing forces is many times greater than the natural frequency of the elastic system as a whole. Complete mathematical analyses of the problem as they affect a locomotive and train in service and conditions on a stationary test plant are presented (These are missing from my incomplete copy of the report as provided by a third party).

The conditions under which resonance can be set up in a locomotive and train by the action of periodic forces are investigated and it is found such resonances only occur at very low speeds. Resonance due to irregularity turning moment occurs at a lower speed than in the case of forces due to unbalanced reciprocating parts, and while the former causes a large amplitude of vibration it is shown the irregularity of turning moment becomes of no importance compared with the effect of the unbalanced reciprocating parts above a speed of about four of five miles per hour.

Irregularity of the turning moment can, therefore, be neglected when considering the conditions to be met on a stationary test plant,

Although is has been shown that in ordinary service on the road resonance will not cause trouble, the natural period of oscillation of a stationary test plant is very different from that of a locomotive and train. It is, therefore, most important that consideration should be given to the conditions under which resonance might occur on a testing plant and the steps necessary to ensure its suppression.

This matter is fully considered by means of mathematical analysis. At the same time attention is directed to the stresses which may expected in various parts of the system and investigation is made as to the requirement s necessary to ensure a smooth record of the mean draw bar pull og the locomotive under test.

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…..value of a particular locomotive, assumed to have considerable unbalanced forces at high speeds have been calculated together with the amount of damping required in the system for its complete suppression**.** It is shown that there will be no difficulty in so choosing the value of the damping coefficient that resonance will not occur under any practicable working conditions.

Further, by a suitable choice of the damping factor, stresses in the drawbar and the dynamometer system will be kept can be kept within satisfactory limits, and, on the assumptions made, it is calculated that the first section of the drawbar between the locomotive and the dashpots should not exceed approximately 12-ft in length with a diameter of $4^1/_2$".

The characteristics of the auxiliary springs introduced for the purpose of ensuring a smooth record of the drawbar pull have been calculated and it is demonstrated that they should have as low a modulus as possible, the limit being determined by the maximum deflection per ton of steady tractive effort which can the dealt with by the compensating mechanism for maintaining the locomotive in its initial position over the vertical centres of the supporting rollers.

Tables and graphs of amplitudes of vibration and stress in various parts of the drawbar and dynamometer are included (not in my possession). Values are tabulated and plotted for representative combinations of damping coefficients and stiffness of auxiliary springs. They cover the three sensitivity ranges of the hydraulic dynamometer and all speeds up to 70 radians per second.

It should be borne in mind that the calculated values of vibration and stress have been determined on the basis that extreme conditions exist. In the majority of cases the disturbing forces acting on the plant will be much less than assumed in the memorandum as high-speed tests will undoubtedly usually involve multi-cylinder engines, the disturbing forces in which are very much less than in the case of the two-cylinder locomotive assumed for the purpose of this investigation.