

RESISTANCE FORMULAE

With Doug Landau's reply to mine in MP 37½ this topic has changed to Steam Locomotive Resistance. There can be little debate about the Vehicle Resistance of the locomotive, so this letter is about the additional resistance, Machinery Resistance (MR).

A correct analysis of MR has to allow for resultants and offsets. One resultant occurs at Coupled Wheel Bearings (CWB), that of (a) static load vertically, and (b) piston thrusts, propulsive, compressive and dynamic, fore-and-aft, through the drive, at various angles near to horizontal. Item (a) is part of the Vehicle Resistance (VR). If (r) is the resultant of (a) and (b) at any point in a revolution, (r) – (a) is something additional to VR, and part of MR. That is simple geometry and arithmetic. If anyone wants to consider (r) alone, the same Locomotive Resistance (LR) will result, but proper analysis of MR per se will be prevented by some of the machinery effects being bound up in the resultant.

I do not understand why Doug sees a need to deduct cylinder frictional losses and what from. These are presumably of rings on cylinder walls. Such friction is positive and a component of MR. It does not depend on Piston Thrusts (PT) but on the pressure on the rings at each point of the piston stroke. Those pressures are the same as those determining the propulsive and compressive PTs, at the same points.

MR arises only after the effects of forces which oppose one another net out. MR is therefore MR, and net is superfluous. Doug thinks MR as a function of speed is more practical. He does not say than what or why, but presumably thinks thus because such would be simpler than a function which allows for the components of MR per se, (again presumably) so that it can be easily added to a VR to give an LR equation of the $a + bV + cV^2$ form. That seems not worth pursuing if LR is to be even reasonably soundly established, because the influences on MR are not dependent on weight, and the V^2 element in MR has to do with various masses, whereas the V^2 in VR depends on vehicle cross section area. In addition, the relevant masses differ considerably from engine class to class, on account of the differing extent to which reciprocating masses are balanced, the number of cylinders, and if more than two, the way they are arranged. Further, MR decreases or only slightly increases at higher speeds as VR increases (see further below on constancy of MR). True, the effort being developed at various speeds needs to be known (Doug's reference to an assumed IHP) to estimate the MR, but that problem can be overcome simply by iteration (described in my paper mentioned on p 213 of MP 34½, available on application to me at johnk.pb15@virgin.net). I have no practical problems dealing with MR separately from VR. Indeed, in arriving at the ITE of a steam locomotive I establish all other resistances first, those to the coupled wheel rims (rail tractive effort, RTE), and then add MR.

Many aspects of LR have only a modest sensitivity to the determinants (Doug's reference to sensitivity to effort). That is very likely in MR. Effort is high, friction coefficients low. The latter are mostly below .05 (a handful above), so that would be expected. A particular force (especially piston thrusts working through the drive) can act in full or part at several places where friction occurs, however, multiplying the rate of variation.

Knowing the fixed and slightly varying effects properly is as important as knowing those which vary strongly. A considerable proportion of MR is dependent on piston thrusts, especially at lower speeds. The extent of MR in total, its variation with effort for various efforts, and what proportion it is of ITE and LR for an LMS Class 5 can be appreciated from the following table for two levels of output at three speeds, estimated as shown in that paper. The first IHP at each speed represents about the best usually observed steaming rate at the speed, and the second half that rate. VR, MR, LR and ITE are in lbsf.
MR, VR and LR of LMS Class 5

	30 mph		50 mph		70 mph	
VR (still air)	770		1240		1900	
IHP	1375	688	1500	750	1550	775
MR	1420	940	1060	920	1100	900
LR*	2190	1710	2300	2160	3000	2800
MR as % ITE	8.2	11.0	9.4	16.4	8.3	6.7
MR as % LR	65	55	46	43	37	32

*LR = VR + MR

There are other influences, especially coupled wheel diameter and number of coupled wheels. The Queensland Railways C19 4-8-0 with 4ft diameter CWs working at full effort (about 67% cut off) with full load on a gradient at the usual 10 mph, had a VR of about 460 lbs and an MR of about 1420 lbs, MR 75% of LR. Dependence on V^2 is low in both VR and MR at 10 mph. If an engine can be judged from the RTE to have been working hard, the speed is in the range of 200 – 350 rpm, and the engine carries 225 – 250 lbs working pressure, MR can be approximated satisfactorily by using 8 lbs mean effective pressure in the tractive effort formula, down to about 6½ lbs at 160 lbs pressure. This shortcut assumes an average percentage of reciprocating masses balanced – it is unsuited to low or zero reciprocating balance. The 30 mph column above, and the paragraph above show that at lower speeds, at which maximum ITEs are developed, an average or constant MR is unsatisfactory. Above 350 rpm or so, the same applies, because the V^2 element in MR becomes considerable.

Doug states that errors of 100lbs in MR or a part of it are tiny in horsepower terms. The statement requires a comparator of 100% accuracy to identify errors, and its import depends on the number of hundreds in the error.

Doug defends the mathematical fitting (trial and error basis decided by the analyst) of resistance curves, and excuses negative coefficients on terms which should from first principles be positive as refining the answer. I do not deny the likely calculation effort or decry the intention, certainly for the period, but deplore the claim. The desire should not be to best reproduce the data, but provide the best scientific (statistical) fit to it, together with the test statistics, which allow establishing the probability that the values of the coefficients and of the answers differ significantly from results of other analyses, or from zero. The same applies to the trendlines which EXCEL allows. These are chosen at will by the user, and might (often do) mean nothing. Doug should not be concerned about a proper regression line (rather than an EXCEL trendline) not passing through the actual data. A best fit will often not pass directly through any of the data. No method of analysis can make up for poorly measured/inaccurate/inconsistent data or improper specification of the equation to be fitted. If the equation is based on the proper physics of the problem, then any failure of the equation to live up to expectations is almost certainly the result of unsatisfactory data.

Par excellence, it is not hard to show (details on request) that the data of the pull on the Amsler dynamometer at the drawbar of locomotives tested on the Rugby Testing Station cannot be right. A series of articles *Locomotive Testing at the Rugby Plant BR*, appeared in the *Locomotive Railway Carriage and Wagon Review* in 1957. No author was named, but directly or indirectly, D R Carling, Superintending Engineer of the plant, was almost certainly the author. In the December issue, pp 233-4, it is said that despite all the favourable circumstances, it is *not* (his italics) possible to measure the internal friction (ie MR) of a locomotive accurately on a test plant, only to confine that value within comfortably wide upper and lower limits. As lower limits measured were negative, which is technically impossible, the comment on the lower limit at least is not helpful.

John Knowles

25th October 2016.

Reply from Doug Landau

The second paragraph positing the "correct analysis" of MR, sets out the salient forces, (a) and (b), producing resultant force (r). It then seeks to isolate and determine (a) (the static loading on the coupled axles), as a component of vehicle resistance (VR). This is utterly pointless, it needlessly complicates matters and leads to miscalculation. There is not the remotest possibility that a design office would treat the matter in this way: from the pistons via the motion to the coupled wheel rims, the losses would be treated as the power transmission system losses, in other words, MR. The outcome, after all, is exactly what is being measured by the test plant dynamometer. The additional resistance to determine the total LR is simply to add the VR of the uncoupled wheels plus the aerodynamic drag and the track resistance (the b term) for the whole locomotive and tender. Why is it thought necessary to isolate an intrinsic element of MR and treat it as VR? Nothing whatever is to be gained by doing so. The resultant of weight, traction and dynamic forces will be less than the mathematical sum of the parts, and cannot sensibly be isolated for the purpose of analysis.

The third paragraph appears not to understand (or has misunderstood my point), that piston frictional losses will reduce the connecting rod big end and coupled wheel journal loadings; not a huge quantity but finite nevertheless.

I'm unclear as to how the fourth paragraph was derived from what I actually said. I neither think nor say that MR is simply a function of speed, it can obviously be presented that way once MR has been determined, and as such is pertinent to the determination of LR HP. MR is clearly the product of manifold forces and elements; simple and dynamic, windage and frictional, weight and mass. Speed expressed as RPM is obviously relevant to the dynamic and windage elements of force. To say "the influences on MR are not dependent on weight" is pure nonsense, only propounded by an untenable view of the mechanical reality. Are we to suppose the losses attributable to axle load only kick in when the locomotive actually moves along a track and are absent on the rollers? Obviously not, John's pursuit of MR in PMF terms (Pure Machinery friction,) is beyond logical comprehension. On what grounds is the numerical demarcation of simultaneous forces acting on a common point justified?

The MR values in the table are upwards of 50% higher than the values recorded on the Rugby test plant (of which more below). Likewise estimates using the suggested 8 lb MEP formula. The idea that a "scientific statistical" fit is axiomatically superior to the empirical evidence contradicts one of sciences basic tenets; repeatability, something the Rugby WRHP data amply demonstrates. The so-called statistical science is no such thing since it will involve many assumptions in regard to friction coefficients and so on.

John says (last para); "As lower limits measured were negative, which is technically impossible, the comment on the lower limit at least is not helpful." Technically impossible yes, but statistically quite probable. The problem is the relatively small difference between two large numbers which are subject to experimental error. Experiments with a random number generator, where notionally perfect ITE - WRTE data was entered (the answer was always 800 save for the fact the two inputs were randomly varied by up to +/- 2%), showed that negative values would occasionally occur. The programme was such that if a single entry was changed the whole data set of 70 entries was rescrambled, so it was possible to quickly generate numerous simulations of test data. The scatter patterns were very similar to those seen in the *later* Rugby test plant data. Unsurprisingly, when the difference was reduced to 600, the incidence of negative values increased. The +/-2% by the way was as the stated limitations of the test plant equipment and procedures. These simulations were a simplification to the reality on the test plant, where variations in boiler pressure increased the natural scatter when plotting Willans Lines (Steam rate Vs IHP and WRHP). I said *the later* Rugby data because negative MR values were rampant in the early test data (70005/25, 35022 and 73008). For 3 test series from 1951/52 158 MR readings

were recorded, of which no less than 95 (60%) were negative, and most of the remainder were improbably low. For the 12 test series 1953/59, of 572 MR readings 5 (<1%) were negative, in line with the simulation predictions, MR was averaging hundreds of pounds.

Clearly something changed post 1952. The recorded wheel rim horsepowers (WRHP) are consistent across these periods where the same locomotive or locomotive types were involved. The comparative data available is a bit random in the sense that the speeds adopted across the various test series varied somewhat. The BR5 tests with 73008 (1951/52) & 73030 (1953), when fitted with 5.125" blast pipe caps as first built, returned R squared values approaching unity for WRHP Willans lines (steam rate plotted against HP) at 20 mph (20 plots) and 35 mph (27 plots). When 73030's blast pipe caps were reduced to 5" and then 4.875" in the pursuit of improved steaming; the recorded WRHP reduced at each step. Later tests with 73031(1958), 4.875" cap, enabled comparisons with 73030 so fitted, again returning Willans lines of high consistency for 35 mph (10 plots) and 45 mph (12 plots). These comparisons were as for 73031 in standard condition in regard to the superheater arrangements. WRHP Willans lines for the various 9F test series again return R sq'd values approaching unity (>0.99): 92013 (1954) and 92050 Series 2 (1957) at 15 mph (18 plots); 92050 Series 1 & 2 (1955 & 58) at 30 mph (14 plots); etc,etc. The Crosti 9F 92023 was an exception, with higher machinery friction at all speeds, amounting to about 60 HP at 40 mph, a figure confirmed by the Crosti's reduced DBHP established on comparative road tests.

The measurement of WRHP 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. For the benefit of readers unfamiliar with the Rugby test plant, the rollers were set with the coupled wheels sitting directly above set at top dead centre (TDC) using a special gauge. After a warm up period of some 40 to 60 minutes, stable running and steaming conditions having been reached, the test period began. The positioning of the coupled wheels relative to TDC was monitored by a differential gear box which measured the Mediating Gear inch seconds. Provided the fore and aft motion in the course of a revolution was equidistant about TDC, no inch seconds would be recorded, and the same inch seconds would be recorded at the beginning and end of the test period. A test sheet for 70025 at 30 mph registered a start/finish discrepancy of 3 inch seconds accumulated over 3618 seconds representing a negligible average shift from TDC of 0.0008" over the 1 hour test period. The WRHP was determined by the dynamometer integrator HPhrs over the whole test period, not spot readings. The amplitude of the fore and aft motion was moderate, on a demonstration run with 70025 working quite hard on 40% cut-off at 25 mph, it was within 1/8 of an inch. A nest of Bellville washers in the drawbar absorbed the disturbing forces, preventing any tendency for resonance to develop; in the words of test engineer Jim Jarvis, the Bellville washers "breathed". The differential gear box also operated the servo mechanism which automatically held the locomotive via the mediating gear at TDC.

The performance of the Farnboro indicator equipment at Rugby was somewhat chequered in the early years of operation. The "balanced pressure" sensors (for details see *From Shovels to CTs*, page 21 on the RPS website), that were key to the production of the indicator diagrams were mechanically and electrically unreliable and failure was frequent. Some correspondence with Ron Pocklington, who was involved with the operation and improvement of the equipment, spells out the various tribulations in detail: "I endeavoured to sort it out to become reliable and precise, including an accurate assessment of the dead centres as a reference and the compilation of the stroke diagram and its IHP assessment". In January 1953 some comparative tests were carried out between the Farnbro indicator and two mechanical types (Maihak and Dobbie McInnis) provided and operated by Swindon engineers. The initial results found the mechanical readings about 7% higher than the Farnbro, the resulting check found the Swindon calibrations to have been in error. After correcting for this the Maihak readings were consistently 2.3% higher than Rugby, the corrected D & M error averaged 3.9 % high but had the curious characteristic of being inversely proportional to steam rate; 7.2% high at

the lowest rate falling to 0.7% at the highest. Some further comparative tests were carried out in early March 1953 between the Rugby and Derby versions of the Farnbro indicator. While both operated on the same basic principal the Derby model used a piston rather than a diaphragm as the balanced pressure interface. Vis a vis Rugby, the Derby results were scattered on, above and below, averaging 2% higher. In summary, the Rugby indicator was the lowest reading of the four indicators tested. Perhaps Carling and associates found this persuasive; the IHP curves in the Britannia test bulletin (Fig. 15) are measurably higher than the Rugby experimental data. Over time a process of trial and error achieved improved reliability and sensitivity, a modified diaphragm "produced the standard of diagram so long sought after". In 1955 some further comparative tests between the Rugby and Derby indicators on 9F 92050 showed closer agreement than previously, the Derby readings were 99.3% of the Rugby average, reversing the earlier result of Rugby being the lowest.

The tabled LR and MR values for the Black 5 are high relative to the empirical evidence. Report L116 reconciling 92050 road test steam rate anomalies includes a 9F LR curve, at 30 mph LR is 1680 lb, equating to steam rate 16,000 lb/hr, 1100 IHP. At this work rate (IHP), pro rata John's table, the Black 5 MF and LR works out at 1230 and 2000 Lb respectively, the latter *19% higher than the 9F*. At 5.6% the Black 5 MF sensitivity to Indicated Tractive Effort (ITE) is high relative to the test plant results for BR5 73031; 49 plots of WRTE v ITE at 30 mph return a sensitivity of 2.7%; at 1100 IHP the MF is 790lb. The R squ'd value was 0.9956 reflecting the low scatter. This is of particular interest since the 49 plots involved a wide range of superheat, with steam temperatures ranging from 450 to 750 Deg. F. At the lowest temperatures for a given IHP cut-offs were about 2.5% longer than at the highest.

The last paragraph citing Carling's observations regarding the uncertainty surrounding the determination of machinery friction omits his preamble. Here he dwells on the small remainder problem, setting out a numerical example. Writing in the in the Model Engineer, 7 November 1980, he again addresses the small remainder problem and gives a similar numerical example, a problem he describes as "very vexed" and "notorious", the difference is that on this occasion he was talking about locomotive resistance not machinery friction. Given the greater potential for variables, any upper and lower uncertainty limits for LR should be set wider than is the case with MR. Freed from the small remainder problem Carling regarded WRHP readings as a reliable bench mark of performance, and used them to monitor the before and after performance of the 9F 92015 regulator modifications, as published in *The Locomotive*, November 1958. The effect of the modifications proved insignificant, the minimal scatter of the before and after WRHP Willans Line plots was clearly evident.

Yours Sincerely,

Doug Landau.

Steam Locomotive Resistance

John Knowles

I comment on Doug Landau's letter of 2.12.16.

Doug's testy first paragraph remarks on my isolating the addition caused by piston thrusts to the coupled wheel bearing resistance (CWBR) of the vehicle resistance. He claims that this is utterly pointless, needlessly complicated, miscalculation, not what would be done by a Design Office, nothing to be gained, cannot be sensibly done. At least I have not been accused of treason! I disagree on all counts. His strong words are not accompanied by any examples of the terrible effects of my supposed error. I challenge him to show how there can be a miscalculation. Of course the resultant is less than the sum of the parts, but that does not mean that the effects of each part as additions to CWBR of the vehicle alone cannot be

isolated. I obtain the addition – I do not make an arbitrary division of the resultant. I have found it useful to isolate the addition to CWBR in obtaining from first principles the parts of MR subject to piston thrusts, as in my process (1) below. In my terminology, MR excludes the CWBR of the vehicle resistance, but includes any extra loading thereon from mechanical effects. My approach cannot make any difference to LR. Further it has to it the logic that the locomotive is first a vehicle, and that without the vehicle the mechanical functions cannot be applied.

I know of no other analyst of the subject than Doug who considers that the whole of the resultant is part of MR. I can see that if he wants combined MR and the CWBR of the vehicle resistance for an LR and he is confident that the Rugby data is correct, he will do it his way. 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.

My fourth paragraph was about statistical analysis by regression, which cannot have been a misinterpretation of a comment by Doug, because he has never used it, and appears not to understand it. He claims to have fitted an equation to some Rugby WRTE data (actually DP, dynamometer pull) and obtained values of r^2 of almost one, presumably as an indicator of how good the Rugby DP data is. What variable he chose to fit DP to, what form the equation took, and the results are not revealed, nor any statistical tests. I presume the work is really an Excel trendline of DP on Q, the steam rate, of a shape chosen by Doug, and not a regression at all. I suspect that if he compared ITE fitted to the same Q in the same way, he will have found that the difference between the two trendlines, an apparent MR, also varies strongly with Q, something which would never do for Doug, who advocates that MR is all but constant across the output and speed ranges. He can check that for himself. Such a way of using Rugby data to obtain MR is not valid, however; the direct ITE – DP data for each test is the source of MR plus CWBR (see below). (I know that Rugby used the term WRTE, but it was DP which was measured, and as will be considered below, a Damping Resistance could well intervene between the WRTE and the DP).

Doug's third paragraph responds to my point that a practical formula for LR, a simpler one than addition of a VR formula and an MR formula, which is difficult, for reasons I gave. Had he read my paper on Steam Locomotive Resistance, he would have seen that I have considered the subject. He says, without reference to my general point, that MR not being influenced by weight (of the locomotive) is pure nonsense, only propounded by an untenable view of mechanical reality, and that my pursuit of MR in terms of pure machine friction is beyond logical comprehension. On the last, if he searches the same paper, he will I use the term MR throughout, except to note in passing that Eil, a BR officer involved in the BR testing at Swindon, made an extremely low estimate of the MR of a Bulleid engine and called it PMF to distinguish it from other measures.

The rest of his remarks here are essentially the same as those in his first paragraph, except that these are richer in their insults. I have already explained that I see MR as the addition to VR. There is nothing wrong with that, it is capable of logical comprehension, so far as I know, by everyone interested except Doug Landau. The mechanical reality is not explained, but if it excludes what I do in isolating the addition to the CWBR of VR, it is not reality. Indeed, as Doug avoids the point of my remark about practical formulae for LR, that draws attention to the three term formulae for LR which he uses for calculations of steam locomotive output as far as IHP, which typically contain (so far as I can see) a constant MR in lbf at all speeds and outputs.

Doug claims that my approximation to MR for certain circumstances is upwards of 50 per cent higher than Rugby. As Rugby MR is low (see below), I think worldwide evidence on MR, such as it is (a subject in itself), is on my side, and that Rugby offers no basis for comparison.

On his fourth paragraph and what follows, I have done three things which bear on the Rugby evidence, which will avoid Doug jumping to conclusions or at least enable him to sort out what I have done.

(1) I have established from first principles what MR might be expected to be, using various alternative assumptions in some cases for friction coefficients, a general approach for all steam locomotives, published in my paper. Doug is known to have attempted the same himself, but from what I know of it (it is not published), it omits some important influences. Such is not scientific statistics at all, as Doug believes (does he really call it that?) but applied mechanics. I have supplemented this by seeking empirical proof of the MR and LR from the literature. It is for this purpose that I isolate the incremental effect of different piston thrusts at the CWBs.

(2) I have analysed the TSMR (ITE - DP) data from Rugby to see how it compares with these principles, noting inter-observation consistency. I apply TS (Testing Station) to MR because such data from a TS includes CWBR from VR. This I have done for all engines tested at Rugby after 1954 where there were at least a dozen observations at any one speed (V). One test is simply to graph TSMR against PTTE. This reveals tremendous ranges in TSMR for a given PTTE, and precious little of the repeatability Doug claims that the Rugby data possesses. In this context, Doug says that for the 12 test series from 1953 until 1959 when steam testing ceased, of 572 MR readings only 5 (<1%) were measured negative, and in line with the simulation predictions, (TS)MR was averaging hundreds of pounds. As the simulation depends on the actual, it would be troubling if it did not predict the same. 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 V^2 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. Only 19 of the 158 observations in the constant speed data I examined could be said to show that they were the result of reasonable friction coefficients.

(3) I have analysed the same data as in (2) by statistical regressions, mostly all at one speed but in some cases across all speeds. This is where Doug makes some wild, sweeping and ill-informed statements. He claimed that this is a so-called statistical science, which requires so many assumptions such as friction coefficients to be no such thing. These remarks are quite wrong, an insult to the many people who apply statistical regressions in testing experimental data in all the sciences, and in establishing criteria for eg rejection of materials. The friction coefficients are used in (1) above, not (3), although they are used also in (2) as a criterion for the reasonableness of the Rugby data as just explained. Furthermore, Doug does not seem to be aware that such regressions are carried out on the observed empirical data.

In both (2) and (3) I too have analysed the effect of possible error ranges in the ITE and DP data. Doug's use of random numbers to show that these are what would be expected formalises that, but it makes no difference in the sense that the data are the data, and must be the basis of any analysis. Even knowing these ranges, the effects of the small difference between two large numbers problem could well prevent satisfactory data and analyses emerging.

I wrote about statistical regression in paragraphs 3 to 6, partly to avoid this knee-jerk reaction against it. Doug certainly did not seek to find out more about regression. There is a lot on the

web about the subject, from simple to advanced, and many good books. The empirical data is used and tested in toto for its reliability. Regression provides a best fit to the data, and provides various tests which can be used to say how much confidence can be had in the results, in other words to say whether equations derived from the data can or should be sensibly used.

Regression of the ITE data (against Q and V) from Rugby is generally very good. Its consistency does not prove it to be right, however. Although I agree that the Farnboro' Indicator was eventually excellent, some of the ITE figures appear a bit low when tested by the Perform program. More apposite, I regressed TSMR (ITE – DP) against PTTE (piston thrusts expressed as tractive effort, this including propulsive, compressive and to and fro forces) at a particular speed where there are sufficient data at that speed. The logic is that an equation in TSMR should in those circumstances have a positive coefficient on PTTE and that the rest of TSMR should be included in a constant. The results for MR, however, are overwhelmingly disappointing, in terms of sense (ie behaviour and signs) and magnitudes, with wide standard errors of the estimate, low t scores on coefficients, high significance F values, and values of r^2 as low as 0.1. Neither the equation, nor the analysis is at fault, it is the poor, inconsistent data. Further, because the ITE data are generally good, the apparently erratic TSMR must be the result of the erratic DP data. With these results, no confidence can be placed in the Rugby ITE – DP data and results for obtaining MR.

I have also used Rugby data to apply the input/output approach to MR for a couple of classes, as used in obtaining the approximate MR of internal combustion engines. These yield MRs which are far too high. As in my last letter, all these results and a commentary thereon are available on request.

In his *Locomotive Testing Stations*, (IMech E and Newcomen Society (1973)), his last major statement about Rugby, 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. More 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 to and fro forces came to an abrupt end at the ends of strokes several times per second (for a 9F, at 60 mph, this was 11 times per second at 60 mph, 3.7 times at 20 mph.), and could not be damped. It is impossible to dampen forces resulting from V^2 with a system operating in V.

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. The recording and calculation were separate from the mediating gear, which moved the engine as needed to keep the CWs on top of the rollers. To do that, it pumped oil into or from the hydraulic system which was the Amsler dynamometer used to record DP. How did it control (Doug's word) DP? How could the gear react several times per second to movements in both directions, ie was it capable of keeping up with the frequency of the sources of variation in DP?

The effect of the Belleville washers, air dashpot and mediating gear operating much more slowly than the fluctuating forces, must have regularly allowed the to and fro forces free rein, and at others resisted them. This would explain the large fluctuations recorded in DP at a given speed and PTTE and the erratic TSMR. 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. Indeed the delayed reaction could have added a damping resistance to the components of TSMR (as an extra positive item between ITE and DP, but not from the working per se of the mediating gear, the energy for which was outside the ITE – DP system).

Some comments on other remarks of Doug's. 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? Carling did not comment again about the plant being unsuitable for determining MR in the Model Engineer article in 1980, but the damping and measurement situation had not changed from his 1957 mention, so if he had commented, why would his opinion have changed? Avoiding damaging resonance was the prime function of the damping, and would have been first in his mind. Indeed, as he did not know that the DP results were right, he probably interpreted positive as being right. While I have read all of Carling's writings in the hope of guidance, I find little help from searching the runes.

There is little science about MR in Doug's letter, more criticism of my approach without troubling to read or understand it. I find it beyond belief that he feels so strongly about something that does not matter a scrap for correct LR.

STEAM LOCOMOTIVE RESISTANCE

DOUG LANDAU'S SPREADSHEET

I refer to this spreadsheet placed on the Society Website in January 2017.

General

Doug Landau is quite correct that any way of obtaining empirical evidence of steam locomotive MR, indeed of LR, is subject to the problem of that evidence being the small difference between two large numbers which are themselves subject to measurement errors, variations or defects in method. This problem is well known, not only in testing locomotives, and is dealt with in statistics textbooks. It is one of reasons why D R Carling thought the Rugby testing plant would not yield satisfactory figures for the internal resistance of the locomotive (MR + CWBR) (see *The Locomotive Railway Carriage and Wagon Review* December 1957 p 234).

What Doug Landau terms WRTE is DP, Dynamometer Pull, and what he terms MF is TSR, Testing Station Resistance, ie ITE – DP as measured on the station, Indicated Tractive Effort less DP. (ITE – DP) in turn equals MR + CWBR + DR, respectively Machinery Resistance, Coupled Wheels Bearing Resistance (as if part of vehicle resistance, but excluding enhancements due to resolving PTTE with it, and Damping Resistance if present). If any friction in the damping is built into achieving the damping, and the damping is perfect, ie any net to and fro (TF) forces in the drive are completely neutralised, then DR is simply the work done in achieving that neutralisation. Damping is very relevant in considering Rugby DP data, and is considered in its own right below. I convert all HPs to TEs for consistency, and abbreviate the Small Difference (between two large numbers) Effect to SDE. PTTE is followed if necessary by an S if the propulsive and compressive effects of steam on the pistons is the subject, and by V^2 if that from unbalanced reciprocating masses; if the sum of the two, then simply PTTE.

Randomised TSR Simulations

Nothing is said about the purpose of the exercise set forth in the spreadsheet, why it is necessary to simulate where there are actual TSR data, the reason for introducing randomness, and the extent to which the conclusions depend on the randomness or simulations. Indeed, demonstrating the existence of SDE does not require randomness or simulations. It can be shown by taking proportions of the average range in the data. To show the SDE is fine, but what then? Is SDE the only reason why Rugby TSR values are erratic? If so, how is that taken into account?

Is the purpose simply to say that the range in the Rugby TSR is what would be expected, under certain circumstances such as those assumed, that is also fine. If however the intention is to justify the terrible TSR and by implication DP values from Rugby, enlarge the sample, home in on the average of the enlarged sample, then it is not. The TSR data from the instruments at Rugby are the data which are to be analysed for what they reveal, not some corrected or improved version, or a much increased number of simulated observations. The TSR is assumed to be 800 lbs at all speeds and efforts, so it is not surprising that the average of many trials yields almost exactly 800 lbs. A lower assumption, say 600lbs, and a higher, say 1200 lbs, would do the same, although there would be an effect on the significance of the results of any analysis (size of sample and variation from average are major influences on significance, as that term is used in statistics). Further, the procedure does not treat the real area of uncertainty in the Rugby data, the DP. It works on ITE (which, see below, is generally consistent in Rugby data) and an assumed constant TSR, completely certain so far as the procedure is concerned, as a result of the assumed constant value. The treatment of SDE brings a range of uncertainty into the simulations, but that means the real source of uncertainty in the Rugby data, the DP, is ignored.

Steps in the Procedure

The typical ITEs are not given, but can, with some study be implied from the tables. It is not said what engine is the example. The constancy of TSR at all speeds and efforts is a further major assumption, a doubtful one. This results, with large numbers of simulations, in assuming what is hoped can be obtained from analysis of the data, even allowing for the SDE. It also assumes the nature and behaviour of TSR, variation with other sources ignored.

Carling, who did not analyse the Rugby DP results for the extent to which these sources of variation in DP applied, did not have a confidence level. Rather, he stated, on an impressionistic basis, how accurate he thought the measured results were, on which see below. (A confidence level in statistics is the end of a range over which certain conclusions can be drawn about probabilities of results occurring by chance, the levels and range suggested by test statistics).

Further, he had no way of knowing the true ITE resulting from tests at Rugby. He did not say that ITE measurements were +/- 2% accurate. Rather, he said that during a given test (constant boiler pressure, regulator setting, and cut off, hence speed also), results were typically in a 2% range. Indeed, the ITE readings for a test were averaged. Some comparisons were made with other indicators. The accuracy of ITE is however unknown. Similarly, he had no way of telling whether the DP measured at Rugby was accurate. He said that the manufacturer claimed the Amsler dynamometer was +/-1% accurate, and that when the instrument was statically tested at Rugby it was accurate to within +/-1%.

The accuracy of the Amsler in use, however, was unknown. The difference between the two items measured on the plant, ITE - DP, or TSR, in turn comprised $MR + CWBR + PTTEV^2 + DR$ if any. The plant was not designed or operated to achieve accurate DP readings, but to avoid resonance damaging the plant and the equipment. Nor were the Amsler readings compared with the other dynamometer on the plant, that providing the braking of the rollers, which provided the resistance against which the locomotives under test worked. The DR is unknown, and was never tested or measured. On account of its importance for DP measurements, damping is considered further below in Seeing Sense in the Rugby Data.

It is said the scatter patterns look remarkably familiar compared with those in the TSR data. For that to have any meaning, the two patterns need to be compared, eg standard deviations, and correlations between actual and simulated values. No mention is made of that having been done. The creation of a larger sample than that given by the Rugby data amounts to creation of extra data to reinforce the actual data, reinforcing some preconceived idea of the

best explanation of that data, assuming that there are no other considerations to take into account in explaining the behaviour of DP. On the same theme, there is mention of normal experimental error as an adequate explanation of some characteristics of the data. How much is normal? To what extent is it a real error or something inherent in the running of the locomotive on the plant? And, very important for considering the data, how random and large are the errors?

Such testing of the apparent similarity in scatter patterns is not a reason for accepting the data (actual figures and characteristics, especially the distribution) as adequate to explain anything. It is perfectly possible for the data to fit the SDE argument but not to be suited to explaining anything, especially finding a reliable TSR, its values and characteristics. No matter how many simulations are made, the Rugby data are not likely to reveal sound TSR, for reasons to emerge below.

Mention is made of 2 – 3% sensitivity to effort. What is the origin of that? If the subject is MR, MR displays considerable sensitivity to PTTES, not ITE, in the range of 5 to 7%, as I have mentioned here before. PTTE is a large number, so even 5% is considerable in MR.

An outlier envelope is introduced. Outliers are values which are considered to be out of place on account of their extremely high or low values. Outliers should not be discarded, but examined for reasons why they are so high or low. If there are good explanations, they should be left in. The outer lines so far as I can see were by means not stated fitted to the highest values. They are all very well, but what are the averages, and the one, two and perhaps three times standard deviations on each side, to indicate the distribution (fortunately, from other diagrams, it can be seen that lots of simulated values cluster close to the averages).

Conclusions on the Method

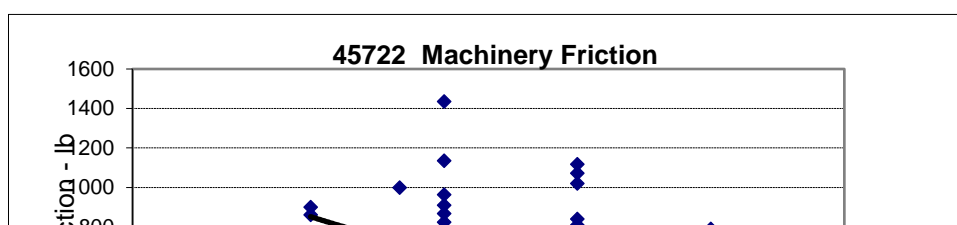
If it was the hope or intention that the randomised approach used by Doug Landau allows the Rugby TSR data to be corrected or improved so that it allows a supposedly sound TSR to emerge, that cannot be the case. For one thing, the way it was done means that the TSR is that assumed by the analyst, which cannot be correct. Most importantly, the method is not the correct way of analysing the data, including allowing for scatter, measurement error, SDE generally, and other influences on the elements of TSR..

The only way it can be shown how good TSR data are, is to regress TSR against its components, PTTES, PTTEV² and CWBR (a multiple regression). SDE will remain a problem. If the measurement errors of SDE are truly random and small relative to the TSR, and there is a high number of observations, the randomness will have little effect on the results. If those errors are large in magnitude relative to TSR, are not random but are biased or erratic, and the number of observations is small, then the components of TSR will not emerge with any (statistical) reliability or significance. Indeed, no sensible values of the components will emerge, ie the Rugby TSR data will not reveal anything at all about locomotive MR. The latter is the way things turn out, on which see below. It is possible that there are other influences than those so far determined from first principles which might affect the determination of TSR.

In that case, the residuals (data unexplained by the relationship so far fitted) are studied, often by graphing, also by further regressions, to see if that is the case. I have tested all likely explanations of TSR measured at Rugby post 1953, and all are wanting, likewise any residuals.

Part 3 of the Spreadsheet, Examples of Rugby TSR Data

Doug Landau presents this graph and the following sentence:



Notwithstanding the scatter, the trendline shown reflects a speed/magnitude relationship roughly in line with theoretical expectations.

His Machinery Friction is of course TSR, or MR plus CWBR. Nothing is said about the form of the trendline (the equation to it) or how it was fitted, and there are of course no test statistics. From inspection of the graph, despite what Doug says, there is no speed/magnitude relationship. For there to be, the data at each of the six speed points would have to be tightly placed along the curve shown. Rather, there is observably much more variation in (his) MF at each of the speeds (about 380 to 1400 lbs for example at 35 mph) than there is, in highly averaged terms, in speed alone (circa 600 to 800 lbs along his trendline). 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. There are sufficient points of data at each of those speeds to test any hypothesis he might have, for example how hard the engine is working, and he believes the Rugby TSR data to be good. Notably, as no equation is given for the trendline, so there is no guidance on how the approach can be applied to the vast majority of locomotives which were not tested at Rugby, or anywhere else.

To test his MF/speed relationship, I fitted a regression equation to the very same data for 45722, $TSR = cV^n$, in logs $\ln TSR = \ln c + n \ln V$, V speed, c and n constants, in \ln terms in order that there was least constraint from the form of the equation. Being a regression, my equation emerges with test statistics.

The result is $\ln TSR = 1857 - 0.29 \ln V$, or $TSR = 1857/V^{0.29}$. That relationship has an odd form. What, in terms of TSR, does the constant mean? What does the low power of speed in the denominator mean (its value is 2.38 at 20mph, 2.92 at 40, and 3.56 at 80 mph)? The test statistics show that no empirical relationship at all exists between TSR and V in Doug's trendline (r^2 is .06, Significance F is .05 and the ranges in the results at which they are significant at reasonable levels of probability very wide). Nor is there a theoretical expectation that TSR varies with V alone. $TSR = ITE - DP = MR + CWBR$. $MR = C + aPTTES + bPTTEV^2$.

(To obtain TSR requires the addition of CWBR, taking care that all relevant forces are resolved as necessary.) The line is continually decreasing from 800 lbs at 20 mph to 530 lbs at 75 mph, ie there is no turnout or shallow U as speed increases. How could such an equation, however valid for engine 45722 be made useful for other locomotives?

Further, his trendline and my fitted equation suffer from an error, which results from the data. The constant of both is at least 1000 lbs. The constant of MR is less than 100 lbs, and the constant of the CWBR of a Jubilee about 150 lbs, in total only a quarter of that figure. That emphasises that his MF/speed relationship does not exist and that there are at least eccentricities in the data. In addition, and of course, my equation like Doug's has enormous spread of data above and below the trendline (his) and fitted equation (mine).

He also says that notwithstanding the scatter, the trendline reflects a speed/TSR relationship roughly in line with theoretical expectations. Elsewhere in the spreadsheet document, reference is made to a shallow U shape for this curve, which probably influenced the undeclared shape chosen for the trendline. He does not say what those theoretical expectations are. There is also no connection between TSR and the dimensions and masses of the engine. The trendline is of no use for estimation of TSR without some characteristics of the locomotive and how it is being worked. Speed enters MR through characteristics of the terms. The propulsive forces tend to fall with speed, the compressive to increase, and the TF forces to increase with V^2 . A great deal depends on the masses of the reciprocating parts, and the extent to which they are balanced in the mechanism. TSR however does not vary with V per se, for engine 45722 or any other.

Testing the Rugby Data

Before research is attempted on any data, that data should be examined closely for its characteristics, and the way it was gathered, measured and presented. In the case of the TSR data, three sensible and useful things can and should be done.

i) Examining the Damping at the Drawbar/Dynamometer connection

R C Bond in his autobiography *A Lifetime with Locomotives* (1975) shows (pp 120-1), that as the first Superintending Engineer of the Rugby plant, responsible for the design, he was well aware of the TF forces from the unbalanced reciprocating masses, and variation in steam pressure on the pistons during a stroke. He relates how on the French plant at Vitry, the frequency of those forces often coincided with the frequency of the plant, which led to resonance being set up, and violent oscillation of the locomotive under test and the plant. The TF forces concerned reached a maximum once in each direction per revolution and formed a resultant with the unidirectional force from the application of steam to the pistons. The Research Department of the LMS Railway was given the task of analysing the problem. The Rugby plant was therefore designed to dampen these forces, to ensure suppression of resonance for any tests likely to be done there. In Carling's 1957 article mentioned in the first paragraph, it is said that it was assumed in the design of the plant that the pull would vary with Simple Harmonic Motion, but it was found in practice that that the pull varied, not in SHM but in a highly irregular and unsymmetrical way, on account of play in the axleboxes and other bearings, the unsymmetrical variation being ascribed to the 90° spacing of the thrusts.

Damping the pull to eliminate the fluctuations falsified the results. Nothing is said about what the damping was, how it was known that the fluctuations were actually eliminated, how the results were falsified, and to what extent. Considering the surviving information, judging from the large number of low and negative values of TSR the falsification of the results continued until 1953. The intention was to damp these forces, presumably either to eliminate them, or to absorb forces in one direction and release them in the other. Until 1953 at least, the damping was poorly designed, and led to most observations of TSR being negative, by several hundreds of pounds in many cases, at least as measured.

It was not the play in the axleboxes and other bearings which caused the highly irregular and unsymmetrical pull, but the TF forces – the effects at the axleboxes and other bearings were a result of those TF forces. Their fluctuation was the result of their movement being interrupted

forcibly by the end of the stroke occurring while their value was still high, ie by the TF forces continuing in one direction when the piston changed direction.

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.

It was not simply a matter of what these devices did, but how well they could keep up with the reciprocation of the locomotives, which at the fastest the engines were run on the plant approximated one stroke per .09 second.

Further, proper damping must balance or neutralise the net forces in one direction with equal and simultaneous forces in the other, ie exactly the same pattern at exactly the same time, and for any friction in the damping per se to be part of the damping, for all four strokes occurring together. The damping which remained at Rugby after 1953 could not do that. What was wanted was opposing the TF forces as they occurred. In each stroke of a two cylinder locomotive, the TF forces changed from assisting the propulsive forces to opposing them, those in one stroke being balanced by those in another, but were still in progress as opposing forces as each stroke ended, the reason for the jerk effect, which was not balanced or opposed. The dashpot with air in it was not capable of dealing with these variations. In any case the TF forces had to be calculated in advance to design proper damping.

While Carling referred to getting the Rugby numbers right after the modifications of 1953, presumably the DP numbers, he did not say how that was achieved, nor could he have known they were right. He emphasised that the main function of damping continued to be prevention of damaging resonance to the plant, rather than satisfactory DP values. Indeed he acknowledged that avoiding the effects of the inappropriate damping would have required complete redesign of the plant. That was not done, so Carling admitted in effect that the damping was not right after 1953, which in turn means the values of DP were not right even then. Because it was not correct in form, damping must have in itself absorbed energy, which would have reduced DP and in turn increased TSR. Even so, as the TSR values are low by comparison with MR + CWBR from other sources, it would seem that the errors from pre 1953 must have persisted, which could well have been in inappropriate measurement.

Keeping the engine on top of the rollers so that that there was no reduction in TSR when it was running downhill and vice versa was achieved by the mediating gear adding to or subtracting oil from the Amsler dynamometer. That was a slow process, but the effect of deviations from the correct were registered, and the recorded DP figures adjusted for them.

In all the analyses I have done of Rugby data, ITE regressed on Q and V, gives good mutually consistent results, and DP very poor results. It is possible the ITE figures are consistent, but all wrong, perhaps all too low. Those I have examined with the Perform program, appear a little low but not a great deal. The problem is therefore with DP, or with one of the constituents of TSR. CWBR should not be in error, hence the PTTE is the problem, not surprising when it is considered that the damping cannot be correct.

(ii) Seeing Sense in the Data

The second approach is to test the data for its sense, a normal practice before conducting any further analysis of it. I used three approaches.

a) Graphing TSR against PTTE

To do the three tests in this exercise I considered the data for every engine tested at Rugby where there were at least 12 observations at any one speed (13 engine/speed combinations), and graphed TSR against PTTE (both sources). The spread of data in all cases was discouraging – what should have been a near straight line of TSR figures from a constant on the vertical axis (see (c) below) spreading upwards and outwards was a confusion of such points, with, in most cases no such pattern.

b) Implied friction coefficient of PTTES induced by steam effects, propulsive and compressive.

From the TSR of the 13 sets of data mentioned in (a), I deducted my estimates of CWBR and the $PTTEV^2$ effects. For any engine class, the sum of CWBR and $PTTEV^2$ should have been constant at each of the speeds considered. That left resistance data varying with PTTES as a residual, which residual I compared with PTTES data. That residual is such a small ratio of PTTES that the data imply improbably low Cfs (coefficients of friction) in the mechanism from steam effects, often less than half the lower set of Cfs I used when assessing MR from first principles, and (by examining what data there are on LR, and by elimination of other sources of resistance, MR. I can also report from having done the above, that that TSRs are erratic at a speed/output combination. I admit that in this exercise I introduce an SDE even more acute than that which occurs in TSR, but the results are very clear. Data on LR and MR from elsewhere in the world tends to justify the figures for MR, hence TSR, that I use, so I consider this exercise shows Rugby TSR to be decidedly on the low side and erratic.

c) Test Equations for Each Engine Class Tested at Rugby where there are at least 12 observations at any one speed.

Where speed is constant, $PTTEV^2$ is constant, as is CWBR. That leaves PTTES as the only component of TSR which at any one speed should show variation with TSR, ie
 $TSR = PTTES + PTTEV^2 + CWBR + constants$ in any of these variables, ie
 $TSR = Constants + b PTTES$

Note that this equation for TSR will include CWBR. It will also include any net DR. This is a simple relationship, easily established if the data are any good. That was found not to be the case, however, not surprising considering (a) above. The constant should be positive, as should the coefficient on PTTES. The equations for most engines have at least one negative.

The t ratios on both constants and coefficients on PTTES are low, the Standard Errors of the Estimate wide, and the values of r^2 low, many less than 0.1. Results from two engines share some outwardly apparently redeeming features. That for the Duchess at 50 mph gives $522 + .015PTTES$. The .015 is too low by far, and the r^2 is only 0.11, ie there is really no relationship after all.

Much the same remarks apply to 9F 92250, the last steam engine tested at Rugby, the data for which gives $227 + .02PTTES$ at 20 mph. At 30, 40 and 50 mph, the constant turns appreciably more negative, as in:

1 Speed mph	2 Observations	3 Equation for DP	4 Value of r^2	5 t on constant	6 t on coefficient	7 Standard Error of the Estimate
20	15	$227 + .02PTTES$.11	2.56	1.24	291
30	17	$-436 + .05PTTES$.23	-0.9	2.11	299

40	12	-1207 +.12 PTTES	.55	-1.94	3.55	195
50	16	-2774 + .22 PTTES	.14	-1.66	2.08	277

The large negative constants in the equations for 30, 40 and 50 mph, and the very high coefficients on PTTES at 40 and 50 mph, show that the plant did not produce reliable DP results. Very little of the data is explained by the form of the equations. Many of the t values are such that little confidence can be placed on the equations occurring other than by chance, reinforced by the large SEEs. The equations provide the best fit to the data, which says nothing for the data.

Further, as the interest is in MR, or in the case of the Rugby data, TSR, the data for Q, ITE and DP, all high numbers, are inter-correlated, and for that reason, should not be used together in attempts to find a relationship for TSR.

All constants for the Jubilee and Royal Scot are negative. The data for 9F 92166 at 30 mph, however, gave $281 + .047PTTES$, with a good t value on the PTTES coefficient, and r^2 0.49. The coefficient on PTTES is encouraging, and the constant exceeds CWBR. If every equation for the set of 13 were like this, then the Rugby data might have been redeemed, but so many other 9F results say otherwise.

Could the values of CWBR and $PTTEV^2$ deduced in my analysis of MR from first principles be too high, rendering the PTTES too low, bias the above results? That is possible, but Cf of the CWBR is fairly well established, and $PTTEV^2$ is modest at low speeds.

More likely is that fluctuating DR is present. There is no way of isolating that.

I also tried the input/output (Willans line) approach to obtaining TSR (MR + CWBR). The article by S J Pacherness, *A Closer Look at the Willans Line*, in paper 690182, Society of Automotive Engineers, International Automotive Engineering Congress, January 1969 explains the underlying idea. ITE is regressed on DP, the opposite of the usual cause and effect representation. The resulting regression line is projected back until it intersects the DP line in the negative range, ie left of the ITE line. That negative section with sign changed gives TSR, which minus estimated CWBR leaves MR. For 92250, all relationships linear, this gave 333 lbs TSR at 20 mph, of which 229 lbs is estimated CWBR, leaving 104 lbs MR, and 247 lbs at 40 mph, which after the same CWBR leaves 18 lbs for MR. These MR values are obviously far too low. At 30 and 50 mph, the TSRs are too low to give any MR at all.

For Duchess 46225, a linear equation gave an MR of 370 lbs at all rates of working at 50 mph. I also fitted a curve to the same 50 mph data (ln ITE on ln DP), differentiated it, and found the slope at various values of DP, all within the data range. For a DP of 7000 lbs, MR is 228 lbs, for 10,000, 419 lbs, and for 16,000 lbs, 813 lbs. These MR values are certainly too low at DPs of 7000 and 10,000 lbs. All these equations for the input/output approach had good test statistics except for the constants, on which the t test measures were poor, in turn leading to large standard errors of the estimate, and considerable uncertainty in the values of TSR.

These results all point to the low values of the Rugby TSR data for analysis of that subject.

Conclusions

The Rugby TSR data are so poor that sound values of TSR will not emerge from them. The equations are not to be blamed for these results; they are the result of the unsatisfactory data. Or, for those not aware of the niceties of fitting relationships to data, the data are such that sound relationships, or relationships that might be expected, cannot emerge.

From all points of view, I would consider the Rugby TSR data of post 1953 to be too low and too erratic to be credible, let alone useful. SDE is only partly responsible for those conclusions. I would ascribe much of the reason for that to be the improper damping and measurement of DP. 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. As previously related (first paragraph above), it was the view of D R Carling, Superintendent of the Rugby plant during its operating life, that the plant was not suited 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. Rugby TSR data should not be used for deriving TSR or MR, indeed for anything. It is strange that Doug Landau should defend the Rugby results so stoutly. If the data are not satisfactory, no good can come of playing with it.

Full results of any of my analyses mentioned are, as previously, available on request. I will also make the relevant Rugby data available to anyone who wants to investigate the subject.

John Knowles
