Locomotive Resistance - Doug Landau Dec 2019

A response to John Knowles letter 4 July 2017, is somewhat overdue. In the interim since my letter March 17 2017 I have undertaken further examination and analysis of the available test data from the Rugby test plant together with material from internal reports, technical papers, correspondence, and the various test bulletins. This has involved two further trips to the NRM archive at York, the latest in March 18 2019. My response, I'm afraid, covers over 26.000 words, of which only part is directly dealing with John Knowles letter. Additional analysis of the available data takes up much of the text. Three examples of the "simple proof" promised in my letter 12th October 2017, are included. The predominant approach remains presentation of the empirical evidence, avoiding the need for estimates as far a possible. Some call on the latter in some circumstances is unavoidable. Estimates can be a bit fluid at times, such as estimating aerodynamic effects subject to natural variation, for example.

The paper trail is currently by no means complete, and further visits to the NRM are required to establish an acceptably complete chronology and record of the various, trials, tribulations encountered, solutions and improvements achieved, during the operating life of the test plant. One thing that emerges from the archive is that the approach of the test staff was meticulous; every aspect of test plant instrumentation was subject to calibration on a fairly regular basis. On occasion outside organisations such as the National Physics Laboratory or manufactures such as Kent Instruments carried out independent calibration tests. Plant tests were preceded by calculations on the theoretical critical speeds for the various Belleville washer options. Calculations were also made of the mediating gear correction required for shifts from top dead centre on the rollers. These also allowed for shifts from TDC of the bogie and trailing truck wheels resting on stationary rollers. Where results appeared suspect, calibration tests, investigations and experiments were undertaken ad hoc.

When tested with the troublesome hydraulic dashpot emptied of oil, of 11 drawbar pulls recorded with 45318 on variable speed test run 156, 19 January 1950, no mediating gear corrections were required When the mediating gear did indicate such a need, the corrections were often as little as 10 lb, sometimes even less; the highest noted from a very limited sample is -54 lb at 20 mph (3 HP) for 45218 on test run 148/2 on 12 January 1950. Corrections recorded were both positive and negative, so the shift was not always forward as might be expected from a locomotive trying to break free from its tethers. By this time, whenever the dashpot was operating with oil, the test sheets also record a 'differential pressure' correction recorded by a manometer. This first appeared in the record for test run 128 on 9th November 1949 with WD 2-10-0 73788. This provision did not appear on the test sheet for run 126 five days earlier (no oil). The manometer, apparently appearing in the interim in an attempt to correct for the wayward behaviour of the dashpot damper when operating with oil. The damper was not given up readily, not only was it seen as potentially of operational benefit, it had become an intellectual challenge. Various combinations of by-pass and pump pressures up to 15 psi were tested or with the pump not running. This produced a variety of outcomes with both positive and negative corrections indicated; the highest discovered was – 1,587 lb at 45.7 mph (-193 HP) on test run 130, 10th December 1949. The day before at a similar speed the correction was +779 lb (95 hp). In both instances no mediating gear correction was required. When not filled with oil there was a fixed drawbar pull correction of +60 lb, to allow for the non buoyancy of the dashpot pistons.

The apparently satisfactory situation with the dashpot emptied of oil notwithstanding, intermittent dashpot tests occurred for some time, as new ideas, tweaks and different types of oil of were tested to no avail. In the end a satisfactory solution appears to have defeated the best brains at Rugby, the Derby research department and the manufacturers Heenen & Froude.

The visit to the NRM archive in September 2018 produced some interesting material, and significant dates. .

Dashpot Removal

A test sheet for Black 5 44862 12th December 1950 was revealing. The significant point being that the items recorded no longer included any corrective adjustments for dashpot "differential pressure", as when the dashpot was still in use following experimental modifications, or compensation for "buoyancy" when operated filled with air; such adjustments being as included in the test sheets earlier that year. The absence of these tabulations is taken as evidence the dashpot was no longer in operation, confirming Jim Jarvis's recollection that he "thought it was eventually removed" A letter to the Railway Executive dated 15th January 1951 headed <u>Damping Dashpot Investigation</u> confirms this, it begins: "In connection with the experiments in hand to establish streamline flow of the oil, it has been decided to transfer the experimental equipment, rigged at Rugby, to Derby, where greater resources are available and more continual attention can be given."

44862 Test Run No. 422 12 December 1950 15% Cut-Off - Part Regulator						
MPH	Pull from Work Lb	Med Gear Correction	Corrected Pull Lb	WRHP	SC PSIG (Approx)	Superheat (Approx)
73.5	1200	-20	1180	231	133	550
67	1450	0	1450	258	132	540
62	1700	0	1700	282	133	540
57	1900	0	1900	289	133	525
52	1980	0	1980	275	132	510
46	2140	0	2140	263	132	505
42.6	2340	0	2340	266	132	505
36.6	2860	0	2860	279	134	510
31.5	3200	0	3200	269	137	515
27	3615	0	3615	260	141	515
22	4195	0	4195	245	148	515
16.8	4820	0	4820	216	153	510

At this stage of development the test reports omitted details of steam rate, making the outcome impossible to cross-check for specific steam consumption and other comparisons. The results of this low power test are nevertheless not without interest when plotted as below.

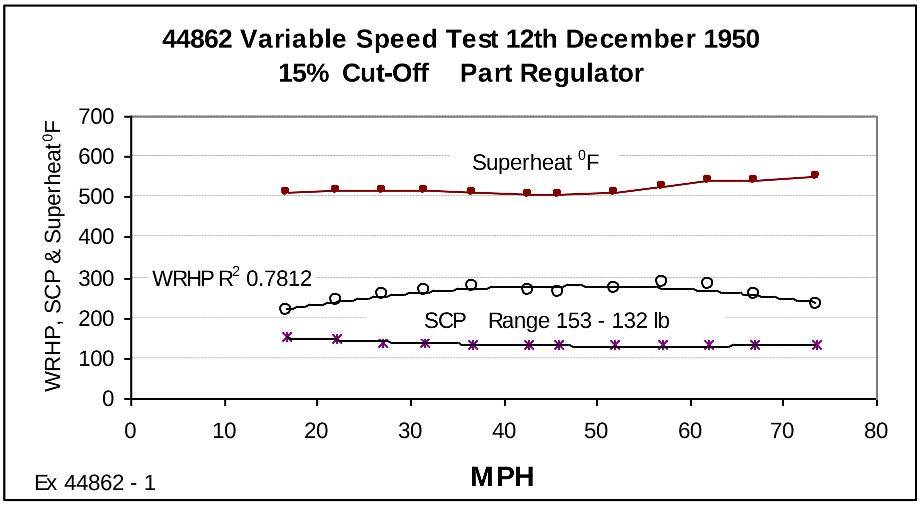

Figure 1 A power sensitivity to superheat appears apparent across the middle speed range. Note the sixth and seventh WRHP plots. The plot progression appears well behaved, free from any deviant changes.

Theoretical Critical Speed Calculations.

A calculation sheet dated 16th April 1951 examines the theoretical critical speeds for impending tests with the *Britannia*. The scope of damping considered ranged from no damping whatever, up to 10 pairs of Bellville Washers. It is evident that the critical speeds occur at the bottom end of the speed range, that speed decreasing as additional washers are brought into play. I have plotted the results in Figure 2 below. The Amsler dynamometer could function over 3 ranges of force; up to 12,000 lb, 36,000 lb and 96,000 lb. Only the two lower scales were considered for this exercise, and it seems likely the highest scale was seldom deployed. It emerges that critical speeds over the speed range encountered on the plant (to over 100 mph on the Duchess tests) was primarily a function of the uneven traction forces, most notably for 2 outside cylinders, and not as the result of dynamic imbalance at speed. The critical speed could be arranged to occur well below the planned test range and would be quickly passed as a locomotive got into its stride under low power at the start of a test. This contradicts John Knowles numerous suppositions and assertions as to how the damping must have malfunctioned, had not been adjusted to suit circumstances and so on. The dynamometer was not existing under constant risk of damage or even destruction, the damping arrangements did not screw up the test results (more on this below). Obviously commissioning and operating a complex test plant was to some degree beyond the experience of the engineers, and they would be treading a capricious learning curve along the way, but the problems were tackled with due diligence and they were not making the supposed oversights and basic mistakes that have been inferred. Please note I am not saying the plant and its operation achieved a state of perfection. How could it, given the inevitability of the metrological limitations, the extensive and varied instrumentation, and the mischief of small remainders.

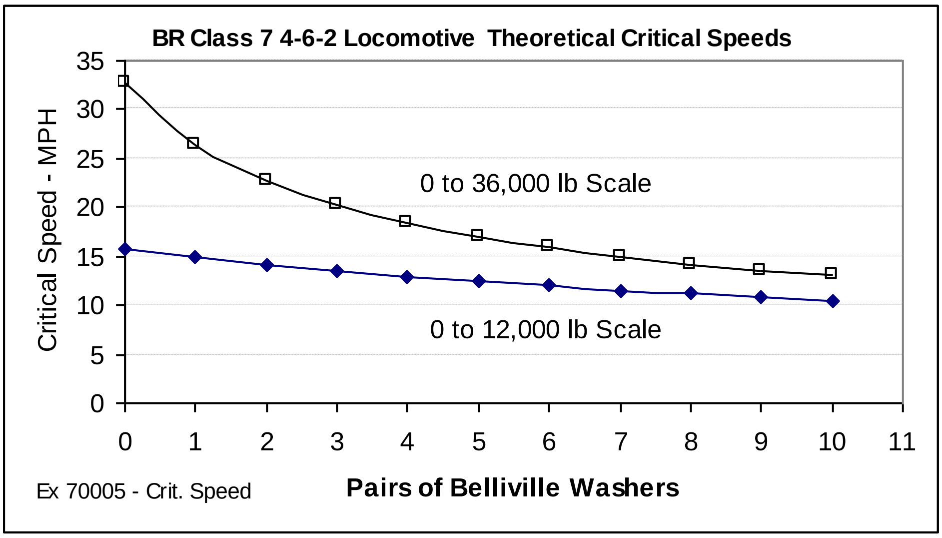

Figure 2 Plot of Rugby calculation sheet 16th April 1951.

Amsler Calibration Tests

Later that year on 28th November 1951; "The work done integrator was checked by pumping up a predetermined load on a National Physics Laboratory (NPL) standardising box and winding through a set distance on the recording table.

The recorded drawbar pull showed negative deviations at a pull of 2 or 3 tons and positive upwards of 8 tons, exceeding 1% positive over 20 tons, which was outside the tractive powers of any locomotive tested on the plant. It was noted that 1679 revolutions of the Amsler speedometer drive disc equalled 5277.37 feet travelled and 1680 equalled 5280.52 ft. In other words, over a mile (1680 revs) the distance error was 1 in 10,000. Below an abstracted data summary from the calibration test excluding data for pulls of over 20 tons (1.157% high at 40 tons). The work-done integrator was checked by pumping a pre-determined load and winding through a set distance on the recording table. This showed the recorded work done 1% high compared with the figures obtained from the standardising box.

This last observation passed without further comment, perhaps because 1% was within the Amsler guarantee. If systematic it would represent +10 HP per 1000 WRHP; 188 lb at 20 mph falling to 54 lb at 70.

Dead Weight Calibration of Amsler Dynamometer Table against NPL Standardisation Box
28 November 1951

Load Tons	2	3	4	5	6	8	10	15	20
Error %	-1.41%	-1.16%	0.0021%	-0.117%	-0.117%	0.021%	0.546%	0.205%	0.021%
Error Lb	-63	-78	0	-13	-16	4	122	69	9
MPH	70	60	50	45	35	30	20	20	15
HP Error	-12	-12	0	-2	-2	0	8	4	.0.4

Only the first two lines are as documented, I have added some notional speeds on the basis that the lower the drawbar pull the higher the speed, in order to give some inkling of the WRHP error magnitudes that would occur given the percentage errors indicated.

There were further calibration tests in 1953, 1955 and 1957. Remedial maintenance and refurbishment work to the Amsler integrator mechanism and mediating gear resulting from wear and tear was carried out from time to time.

1953 & 1955 Amsler Dynamometer Calibrations

	Work Done	Correction 1953	Correction 1955
12,000 lb Scale	6,000 lb	N/A	-0.1%
	12,000 lb	N/A	-0.75%
36,000 lb Scale	12,000 lb	N/A	-0.23%
	18,000 lb	N/A	-0.75%
	Scaled Pull	**Correction 1953**	**Correction 1955**
12,000 lb Scale	6,000 lb	+1.87%	-0.57%
	12,000 lb	+0.125%	-0.06%
36,000 lb Scale	12,000 lb	+0.71%	-0.4%
	18,000 lb	0	-0.1%

May-June 1967 Amsler Dynamometer Calibration

The report summary took a different form to the earlier reports. The calibration of the Dead Weight Tester indicated the actual pull was 285/286 of the calculated pull, a correction of - 0.35%.The Work Done integrator error was 361/360, a correction of +0.27%

Indicating Developments

The early commissioning phase gave little attention to cylinder indication, though ultimately of importance, such measurements were not integral with the functioning of the plant test bed and dynamometer. During the various interregna when the commissioning of the plant dynamometer was halted for one reason or another, the opportunity was taken indicate D49 62764 with Reidinger poppet valve gear and Capprotti Black 5 44752 in 1949. I have no experimental data for these tests. Perhaps, with an eye to the forthcoming BR Standards, it was done to discover if poppet valve gear potentially offered a better way forward. The first locomotive on the plant after the first commissioning phase was 45218, undergoing 137 test runs between 3rd January and 19th May 1950. This early post commissioning phase in the history of the test plant could be dubbed the “working up phase” which lasted about another two years. 45218 only appears to have been indicated during its last few days on the plant, notwith- standing that the tests were investigating the effects of changes in lead. Such determinations were evaluated by the changes in the recorded WRHP. As the official report notes: *“Unfortunately, no consistently reliable indicator cards were obtained either from the Farnboro indicator which is still in the process of adaption to work on a*

steam locomotive, or from a borrowed Crosby indicator, so that no assistance could be obtained in this way to explain the somewhat irregular sequence in the rates of consumption for the various leads. As all the above mentioned curves are *intended only for comparison with one another they have been left on a basis of horsepower at the wheel rim."*

The tests with 44765 comparing the efficacy of single and double chimneys and the steaming tests with B1 61353 have handed down WRHP and boiler performance only, though a note in the correspondence mentions that the B1 was indicated at the end of the final test series, recording very low or negative machinery friction (no data available). The data base boiler performance for 44765 and 61353 is poor in regard to specific evaporation rates (lb/steam per lb coal). It is concluded that the steam rates given in the data base are in fact the feed water rates only, and that the exhaust steam injector was in use. The steam temperatures reached support this view. This is known to be the case in regard to 61353; it says so in the test bulletin, but only in passing. The true steam rates were therefore about 6 to 6.5% higher than shown in the data base up to the ESI limit around 20,000 lb/hr.

Indicator shortcomings notwithstanding; 45218 was indicated for its last few days on the plant. The data base I am working from has no data on this, an internal report (20 May 1950) gives some details: "*In order to attempt to isolate the apparent error in the Farnboro attention focussed on the LH cylinder exclusively (to which the Crosby was fitted) and a number of diagrams taken with a Farnboro element while indicating by the Crosby."* The initial results with the Crosby showed a mechanical efficiency of 0.95, - with some lapses to 1.02." Some experiments concluded that the Crosby indicator was subject to a phasing error caused by the length of pipe between indicator and cylinder. Reducing the pipe length in stages. Eventually the Crosby MEP results were *"sensibly the same as the Farnbro element".* Both were *"less than the measured Amsler drawbar figures and therefore the latter also are in error to the extent of about 12%. The Rugby (Farnbro) indicator appears to be correct. <u>Action.</u> Indicate the Amsler cylinder as originally suggested many weeks ago."* The actual report the previous day put the probable error between 7 % and 10%.

It took over a year to organise such tests. A letter dated 8th August 1951 refers to "Dynamic Calibration Of Amsler Dynamometer" involving 61353, The last B1 test was a week earlier on 1st August. On what appears to have been an adaption of the Farnboro indicator, the peak and minimum hydraulic pressures of the dynamometer were monitored and compared to the recorded WRTE test value. There was no attempt to integrate the monitored readings into WRHP on a work done basis. More details of these tests on page 93 below.

Comparison of the WRHPs recorded at this stage with later periods, *when positive MFs were being routinely returned*, does not support the idea of WRHP errors as high as 12%, since the overlapping WRHP Willans Lines were closer or similar across time.

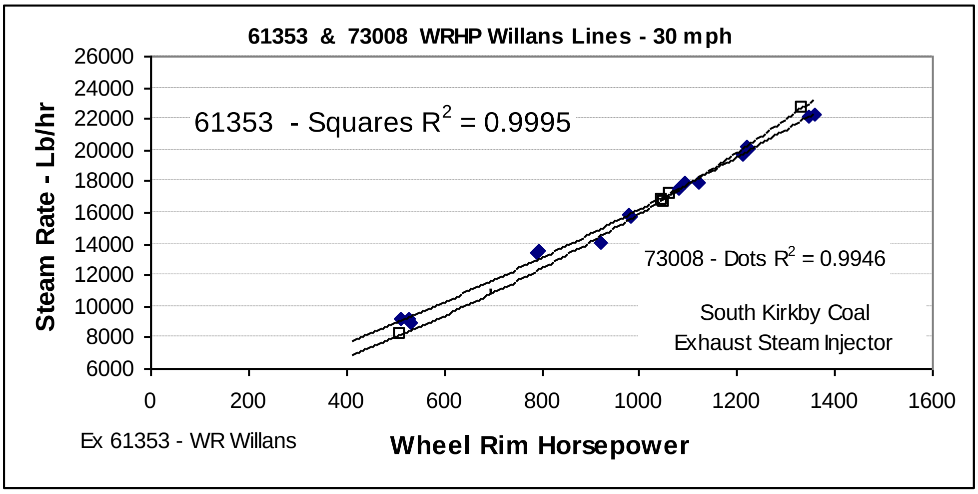

Figure 3. Diverging overlap with mid-range agreement. ESI contribution assumed at 6%.

Some lfurther comparative indicator tests with 70005 in December 1951 returned results for the Crosby (LH cylinder front only) averaging 2.8% below the Farnboro' (16 plots). Presumably the Crosby pipe set-up was along the lines developed for 45218. The conclusion in May 1950 that the Farnboro' indicator "appears to be correct" is put at odds to some extent by later IHP Willans Line outcomes for the Britannia which improved over time. In example the 40 mph IHP Willans Lines from the Rugby data and Test Bulletin at a steam rate of 20,000 lb/hr yield the following results.

	IHP	Index
70005 1951	1374	100
70025 1952/53	1420	103
Bulletin No.5 - April 1953	1445	105

It would be misleading however to conclude that this level of increase applied uniformly across the full speed and power range portrayed in the test bulletin. In contrast to John Knowles claim that the Rugby IHP data was "consistent", detail scrutiny of the IHP data for 70005 and 70025 reveals disparities at times verging on the chaotic, a situation applicable to some of the IHP data generally. The second test series for 9F 92050 showed a measurable decline in cylinder efficiency compared to the first; the WRHP reduced accordingly. In his case the change was real enough, attributable to steam leakage as traceable by exhaust steam temperature and pressure changes.

Correspondence from Ron Pocklington, the test engineer intimately involved with the operation and development of the Farnboro' "balanced pressure" indicating equipment reveals shortcomings in regard to reliability and performance in its first years of operation: "We used to get semi or complete snowstorms before an improved spark generator was obtained (1954). I endeavoured to sort it out to become reliable and precise, including an accurate assessment of the dead centre as a reference and the compilation of the stroke diagram and its IHP determination. If this is not carefully done then a direct fattening up, or down of the stroke based diagram appears." This level of reliability and performance was not the situation as he first found it when he started work at the plant at sometime in 1952.

The case made for correcting the Crosby result in 1950 was straightforward and persuasive. However; *"….the Farnboro' element had in effect been used as a stop watch to time the delay of the pipe line and as such had measured a delay of the time lag as about 4 milisecs.*" This effect fattened the Crosby indicator diagram. This assumes the Farnbro was accurately plotting stroke dead centre at the then stage of development. Commenting on the indicator diagram in the test plant brochure (70005
Test Run 665, 1.12.51), Ron Pocklington observed: *"If you look at the slide bar contact marks you will see some wobble due to slackness in the universal coupling to the indicator drum*." Written communication.

The Farnbro. "balanced pressure" indicator encounters some intrinsic "lag" in another way. It operates on the principle of those coloured tinplate clicking novelties popular in Christmas crackers. A shallow dish pressed into the tinplate makes a click when the dish is reversed by pressing on the convex side. The so called "balanced pressure" Farnbro indicator requires a finite pressure differential to operate. This is defined as the "lag", and ideally should be of very low magnitude. The contact with the diaphragm as originally set up at Rugby was spring loaded, this will have introduced a slight increase in the degree of "lag" when breaking contact. The final improvement of the Farnboro' indicator was achieved by the simple expedient of substituting a fixed electrical contact for a spring loaded one. *"One element was fitted with a new arrangement of centre contact and it was soon found this produced the standard of diagram so long sought after. No scatter was apparent even at the highest speeds."* This was early on in the Duchess tests starting at the end of January 1955. Quite late in the day, in the history of the plant. This outcome makes sense; a spring loaded contact would slightly delay circuit interruption and the spark generated pin holes that formed the diagram. The spring loaded contact was effectively minutely increasing the system lag by delaying contact separation and spark generation.

Progress achieving positive IHP-WRHP relationships is mapped out below in Figure 4.

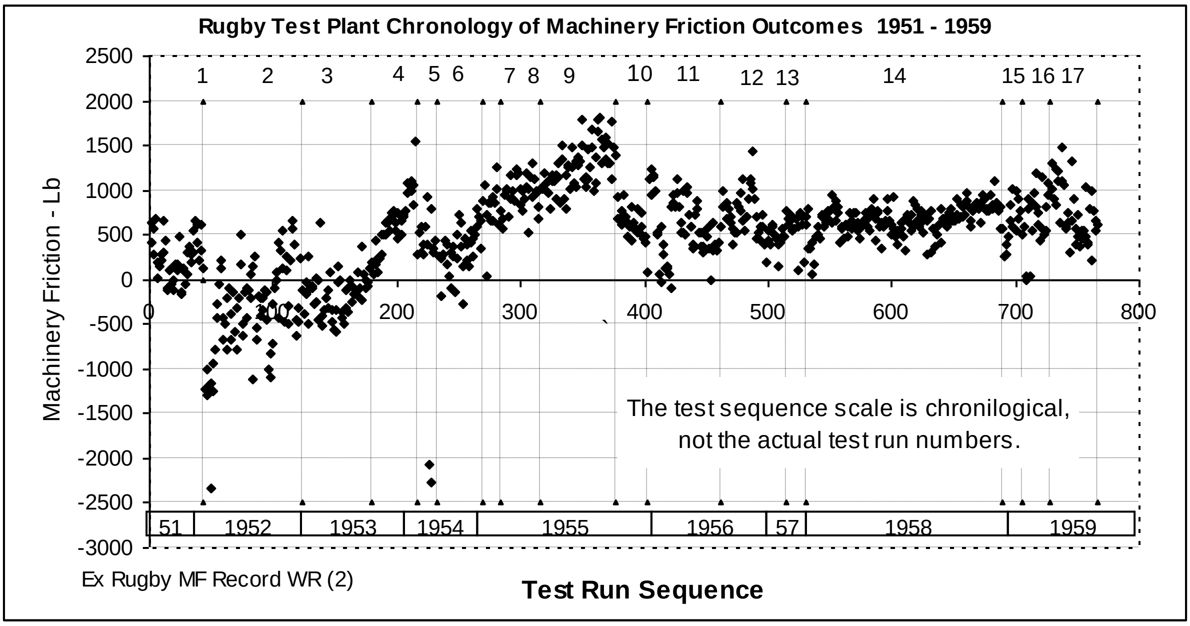

Figure 4 Earlier WRHP data available for 45218, 44765, 61353 and 70005 lacked any correspond- ing IHP data. The numbered data sets are indentified in the table below. 1953 was something of a watershed year since from that point, negative MF outcomes only rarely occurred, at a rate predicted by random number experiments. There were a number of developments and improvements in 1953 of which more later.

Absent through lack of data are further tests for 35022 with a single chimney following on from 70025 in March 53 (26 test runs), and again later that year after 73030, and 70025 (5 demonstration runs) for tests without thermic siphons (36 test runs), Also absent is data for two test series with Crab 42824 fitted with Reidinger poppet valve gear, following on from 70025 at the end of 1953, and later after 46165 in June 1956; 47 & 56 tests respectively. EE GT3 tests occupied much of 1957.

Key to Figure 3					
Ref	Locomotive	Ref	Locomotive	Ref	Locomotive
1	73008	7	42725	13	92050
2	35022	8	46225	14	73131
3	70025	9	92023	15	92166 Stoker
4	73030	10	92050	16	92250 D/C
5	42725	11	46165	17	92250 Giesel
6	92013	12	45722		

It seemed that the tests starting with 73008 in April 1951, imperfect though they were, with mixed MF outcomes, represented the dawning of some light. It was to be a brief victory of sorts, the tests that immediately followed with 70025 represented a serious relapse, which only became worse when with the turn of 35020, which proved to be something of a law unto itself. Somehow, when 73030 put in an appearance in July 1953, things seemed to be on track.

During this period the Farnboro' indicator equipment underwent many modifications as recorded in official correspondence and private communications from Ron Pocklington. This included several modifications to the spark generating circuitry, the diaphragm material, and the spring contact set-up prior the adoption of a fixed contact. The changes were driven by frequent failures of the spark circuit, cracked diaphragms and an ambition to reduce chronic scatter. In its final form the diaphragm could be operated “with a breath”. At operating temperatures this sensitivity may have been slightly reduced. Some of the changes along the way may have had a retrograde

outcome. This could explain some of the set-backs as evidenced by the see-saw nature of both the early MF outcomes and apparent IHP variations. Figure 5 below, though representing some progress, is not without its obvious imperfections.

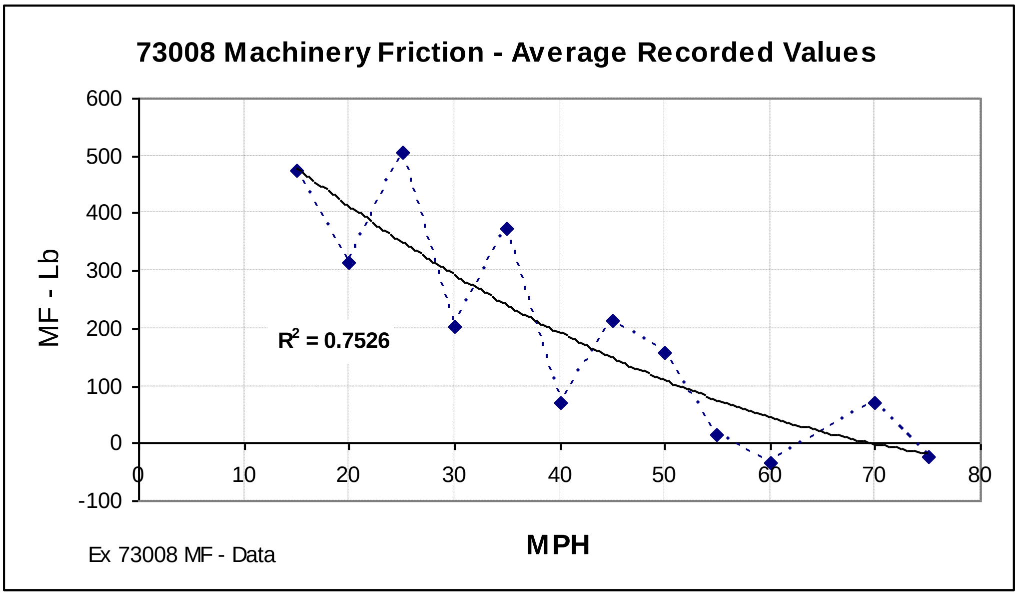

Figure 5 Here the scattered MF outcomes for the speed sets have been averaged and plotted against speed. The overall trend, clearly and illogically, is saying that MF is an inverse function of speed. However, when the plots were joined together, note how the resulting zig-zag trace follows the overall falling trend. As randomised number experiments have shown, speed data sets may cluster to form high and low biases as evidenced here.

Some degree of the scatter is ‘true’ in the sense that small variations in steam pressure and temperature will influence the result

When the 73008 MF outcomes are examined in order of sequence a different picture emerges. MF data was late to emerge in the test programme, since the Rugby test team had little confidence in mechanical indicators, and post commissioning, cylinder indication was largely absent from the early test programme as tabled below.

Rugby Test Plant Programme & Data Record 1951-53						
Engine	Test Runs	Dates	IHP	WRHP	MF	Notes

61353	449-508	15.1.51-30.3.51	-	25	-	
70005	509-543	17.4.51-28.5.51	37	-	-	1st Application Farnobro' Indicator
61353	544-589	7.6.51-1.8.51	-	26	-	
73008	590-657	13.8.51-5.11.51	-	50	-	
Amsler Calibration 28th November 1951						
70005	658-691	3.12.51-3.12.51	41	9	-	
73008	692-714	30.1.52-21.2,52	35	65 #	35	
35022	715-821	19.3.52-2.10.52	75	133	74	
70025	822-895	31.10.52-20.2.53	67	63	47	
35022	896- 923	10.3.53-7.5.53	-	-	-	Single Chimney Tests
73030	924-1022	22.7.53-3.11.53	35	94	35	$5^1/_8$", 5", & $4^7/_8$" Blast Pipe Caps
70025	1023-1027	25.11.53-27.11.53	-	-	-	Demonstration Runs
35022	1028-1063	5.12.53-25.1.54	-	-	-	Without Thermic Syphons Tests
		Total	290	465	191	
# A few test runs at miscellaneous speeds omitted						

It was not until April 1951 the Farnboro' indicator was available for testing with the initial trials of 70005. Following these tests, there was a 6 month interlude before indicating was tried again, presumably to deal with development problems that had emerged regarding the electrical circuitry and diaphragm durability. As a consequence the first test series with 73008 was not indicated. Cylinder indication for the second test series starting in January 1952, was confined to 35 test runs. When sequenced, the MF outcomes fell into two distinct groups: the 1st group comprising 21 test runs included 7 negative MF outcomes with an overall average of 95 lb; the 2nd series of

14 runs was free of negative outcomes, with an overall average of 411 lb. The specific IHP steam consumptions for the seven negative MF outcomes were all significantly high when plotted against the BR5 test bulletin IHP SSC Willans Lines as indicated in Figure 6. The implication being the IHP was under-recorded.

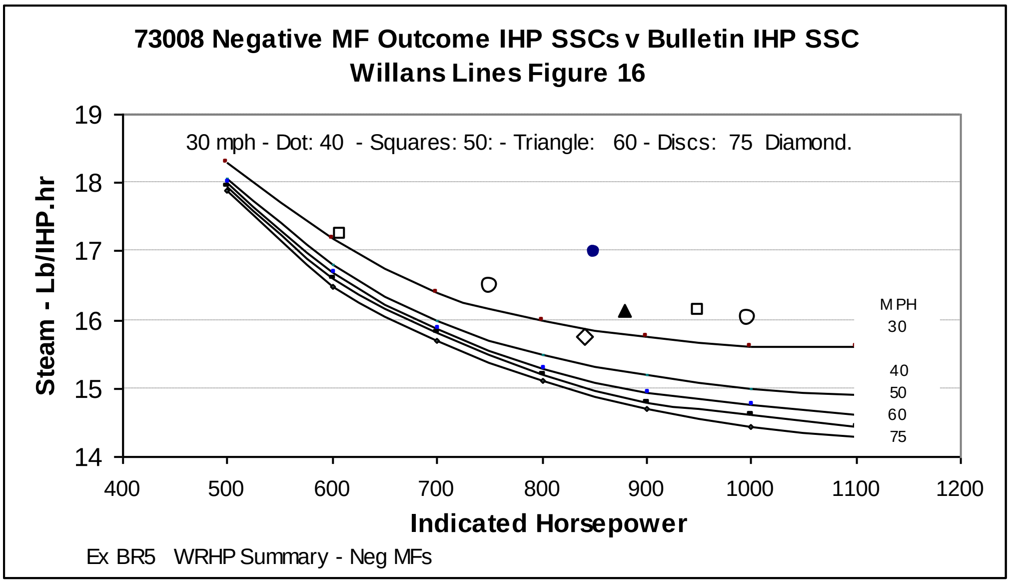

Figure 6. All the IHP SSC plots, as associated with negative MG outcomes, fall significantly above the related speed IHP SSC Willans Lines.

Overlapping test data for the 73008 and 73030 test series when both were fitted with 5.125" blast pipe caps is limited to WRHP data at 35 mph with 12 and 15 plots respectively, as plotted in Figure 7. The available overlapping IHP data is minimal.

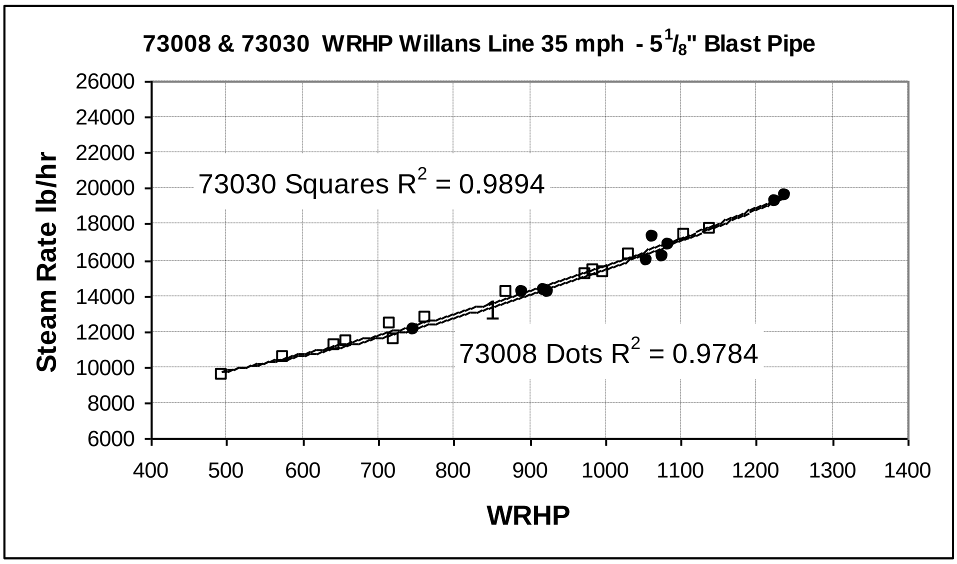

Figure 7 The 73008 plots include examples from the initial test series in the latter part of 1951 and the later tests early in 1953. The 73030 tests were in the second half of 1953. The slight Willans lines separation falls within the guaranteed dynamometer accuracy. Combining the plots returns an R^2 value of 0.9905.

In late July 1951, some 15 months after the 45218 tests, when it was proposed to
"Indicate the Amsler cylinder as originally suggested many weeks ago": the decision was enacted upon for the last few tests with B1 61353 (report dated 8th August 1951).

"*The discrepancies between the WRHP and the IHP obtained from the ER B1 Class Engine No.61353 has caused further investigation into the accuracy or* *otherwise of the Amsler measuring equipment. A differential pressure element has been made at Rugby, and after a very limited attempt to calibrate same inserted into the Amsler dynamometer cylinder".*

The report included a note of caution. "*As stated earlier, calibration of the element was found very difficult in view of the limited facilities available for pressure calibration at Rugby Testing Station. And the result obtained should be treated with the utmost* caution. since an error of 1 lb in the gauge used in the air side will cause a resulting error of 114 lb on the pull." A diagram of the apparatus has not been found.

61353 Amsler Indicator Calibration Test - 25% Cut-Off - August 1951					
MPH	Recorded Pull - lb	Indicated Maximum Pull		Indicated Minimum Pull	
		Maximum	Minimum	Maximum	Minimum
20	11,300	10,600	10,070	10,200	9,660
20.25	11,930	10,600	10,070	10,370	9,870
29.7	9,810	9,360	8,840		
40.5	8,850	8,420	7,910		
60.9	7,495	7,100	6,580		
60.9	7,505	7,100	6,580		

The "peak" calibration indications averaged only 95% of the recorded pull of the Amsler. The peak value should have been higher since the recorded pull was the average value. On an average of the maximum and minimum pulls, the indicated results were only 90% of the Amsler. No explanation is given for the absence of "Indicated Minimum" pulls above 20 mph. It may be that the differences were insignificant at the higher speeds. As Lomonossoff pointed out*, the flywheel effects of the coupled wheels and motion smooth out the fluctuations in turning moments such that they "*cannot perceptibly vary its speed*". It is therefore, difficult to model the drawbar pull profile per revolution directly from the simultaneous MEP pressure record of the four cylinder ends as recorded in these tests.

Obviously the results of these tests are problematical, at face value supporting the suspicion that the Amsler dynamometer was at fault. The problem remains, that later results, when positive MF outcomes were being returned, no change in the measured
WRHP obtaining when negative MF values were endemic is obvious: vide Figure 7.

It is perhaps not without interest that among the improvements listed in 1953, were improvements to the Farnboro' Indicator diagram converter. *"A new crank and connecting rod with ball bearings were fitted and the base board stiffened up. Following the successful improvised drive by a meccano electric motor, a permanent Hillman motor was obtained and a gearbox assembled at the plant." .*

Pocklington was not impressed with the situation as he found it when he arrived on the scene in 1952, citing among other things, the difficulty in establishing the true 'dead center' for the Farnboro' radial indicator diagrams. A situation further complicated since the dead centres for the cylinder front and rear power strokes occur at different, crank angles, having to accommodate for cylinder thickness.

Notwithstanding the apparent indications of dynamometer malfunction as manifest in the Crosby/Farnboro' tests with 45216 in 1950, and the calibration experiment with 61353 in 1951, the WRHP outcomes seem little changed over time, notwithstanding that MF outcomes had become positive in the interim, as exampled in Figure 7.

I have looked into the effects of dead centre error, converting a sample Rugby indicator diagram for one cylinder front end to a stroke base, then repeating the exercise, first with 'dead centre' moved $^{1}/_{32}$" to the left, then $^{1}/_{32}$" to the right ($^{1}/_{896}$ of the stroke).

70005 40% Cut-Off - 20.28 mph Potential IHP 'Dead Centre' Error Effects			
Item	As Diagram	1/32" 'Early' Admission	1/32" 'Late' Admission
MEP	144.9	146.84	142.0
MEP Index	100	101.4	98.0
IHP	1149	1165	1126

…………………………………………………………………………………………
…
* Introduction to Railway Mechanics , G Lomonossoff, Oxford University Press. 1933; page 105.

The calculated 1149 IHP assumes equal MEP for the four cylinder ends which is of course contrary to the actual case (1125 IHP). The tests at Rugby routinely followed a lwarming up period to stabilise any thermal effects on valve setting and dead centres.

The IHP test data from 1951 to early 1953 involving 70005,73008, 35022 and 70025 falls someway short on consistency, at times, things seem to have been going backwards. Starting with the BR7, the tests with 70005 and 70025 thread different paths when plotting Steam Rate v Speed & Cut-Off. In relative terms the two paths shown, Figure 8, are likely real enough, the difference are probably attributable to the subtleties of valve setting. Valve setting, long held as something of a black art, often with secretive ideas as how to best do it, provides scope for different outcomes. Some careful thought and experiments on thermal expansion allowances are said to have reduced Britannia water consumption on the Great Eastern section by about 12%.*

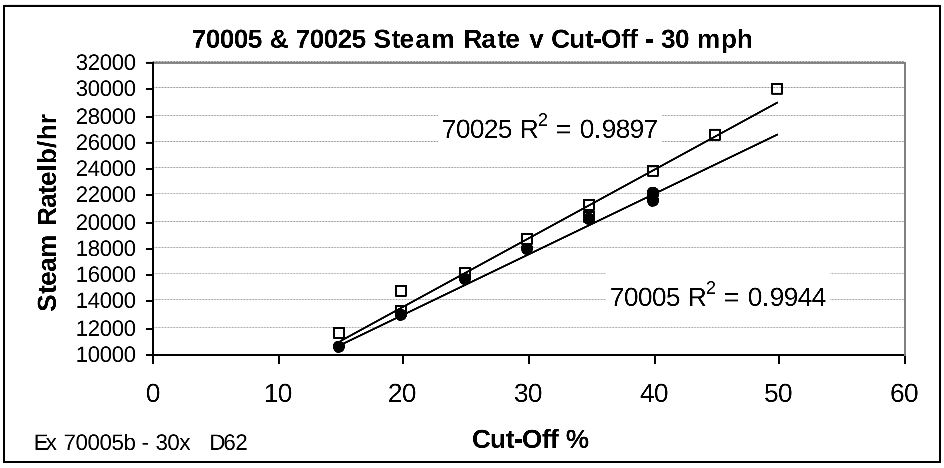

Figure 8 The trend for 70025 is the basis of the test bulletin cut-off curves; Figure15.

The recorded WRHP data for 70005 was not simultaneous with any IHP data, so there is no direct MF record. The comparative WRHP Willans Lines for 70005 & 70025 at 40 mph are plotted below. The 70005 XL extrapolation beyond 1400 WRHP is unreliable.

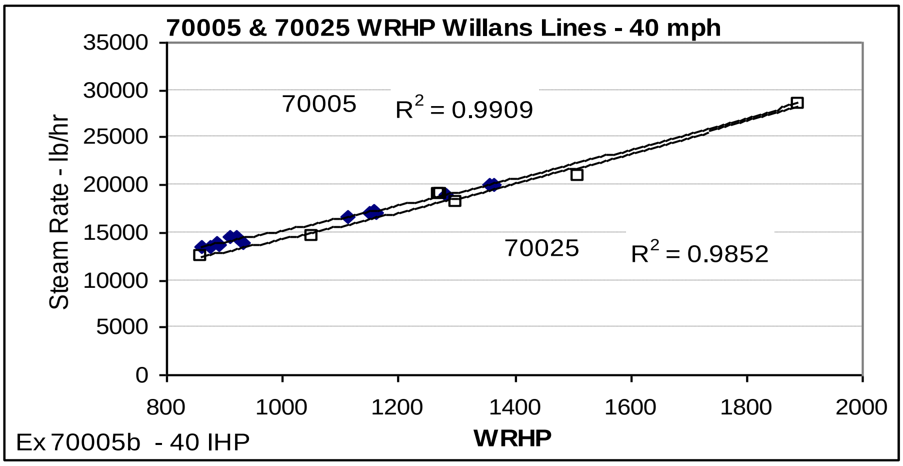

Figure 9 Unlike the WRHP data above, the 70025 IHP data features wide scatter when plotted on a specific steam consumption basis; R^2 0.2964. The data base at 40 mph lacks any coal rates and is endorsed "LSI assumed" (Live Steam Injector). In the absence of firing rates it's not possible to cross check this by calculating the specific evaporation rates Assuming the ESI was applicable to the outlying plots brings them into line. It is not possible to verify such changes

Merchant Navy 35020 treated the Rugby test team to a harvest of negative MF outcomes and one or two idiosyncrasies. One example was the dip in indicated horsepower at 24 mph as speed increased at cut-offs between 10 and 20%. A
similar eccentricity was evident when 35005 was road tested with a mechanical stoker in 1950. In this instance the dip was at 20 mph between 15 and 30% cut-off,
…………………………………………………………………………………………
……..
* Bill Harvey's 60 Years In Steam, D W Harvey, David & Charles, 1986; page 202.

The one uncertainty 35022 did avoid was the use of an exhaust steam injector, since none were fitted. In that regard, at least the data base steam rates are unequivocal. Some of the IHP data is clearly aberrant in character, with no potential explanation on the grounds of exhaust steam injector participation or lack of it. Said aberrations are best seen when the data is examined in enlarged form; that is IHP and WRHP specific steam consumption, as Figure 8.below. Following on is an orderly set of WRHP Willans lines for 15, 20, 30 & 40mph - Figure 10.

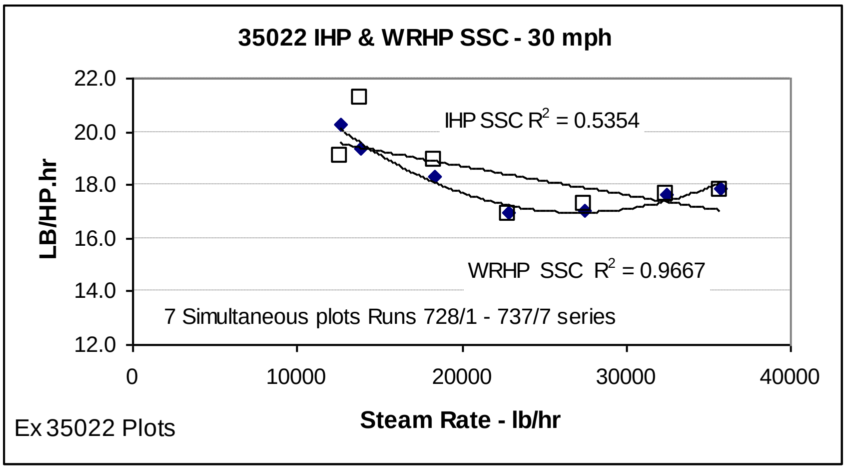

Figure 10 The IHP & WRHP plots are clearly in collision, as was endemic at this stage of development, but, unlike the IHP trend line, at least the WRHP curve is the right shape, and returns a respectable R^2 value. A similar exercise for 40 mph delivered a similar result. Removing the low LH IHP SSC plot, clearly an outlier, delivers a concave trend line,

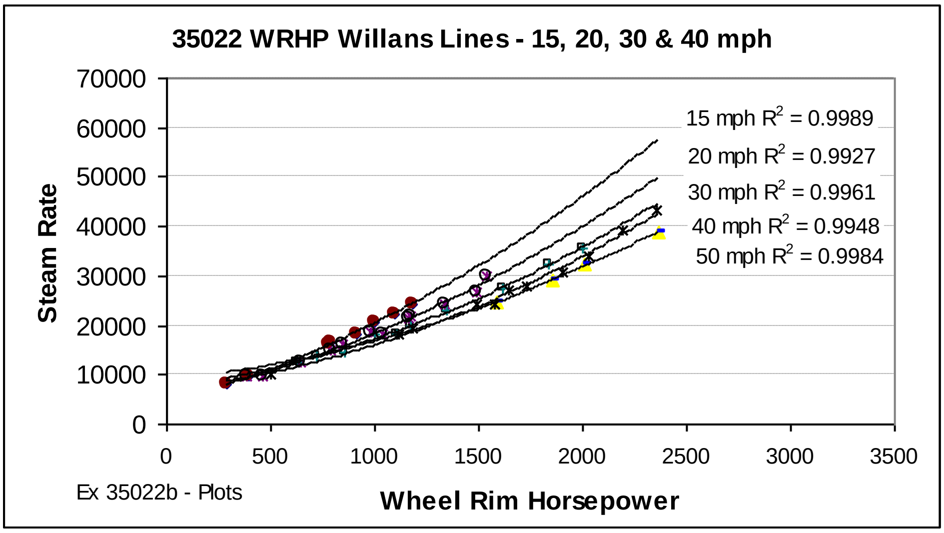

Figure 10 The orderly pattern as a function of speed and power follows the intrinsic characteristics of reciprocating steam. The equivalent diagram for the indicted horsepower is equally orderly at this level of magnification. The problem was the IHP/WRHP data at this stage of development was mostly in collision, with over 80% of the MF outcomes returning negative values. The recorded cylinder efficiency was about 12% low compared to a Duchess.

Mechanical Efficiency

Mechanical Efficiency is a simple relationship: $M\eta$ = WRHP/IHP or WRTE/ITE

Firstly, a look at the combigned raw MF data for stoker fitted 9F 92166 and 92250 in double chimney and Giesel ejector guise reveals wide scatter, a 'high' bias at 40 mph and a vestigial R^2 value, as evident in Figure 12 below. Some of said scatter is real in the sense that it reflects variations in effort. When re-plotted in mechanical efficiency form as Figure 13, the scatter is much attenuated, the 40 mph bias reverses, falling generally in line with the overall trend against speed, and the R^2 value, though remaining medioccre, is significantly improved.

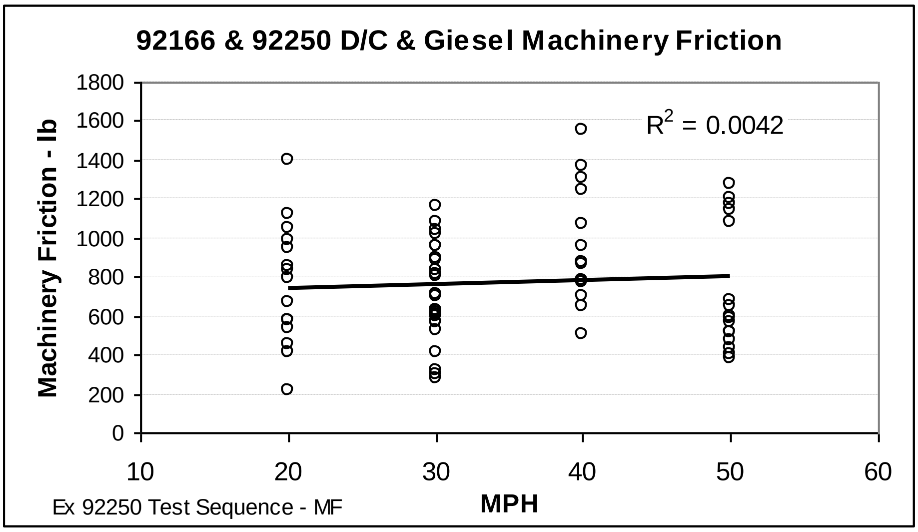

Figure 12. Wide scatter and some random bias as seen here is an inherent characteristic of small remainder data sets.

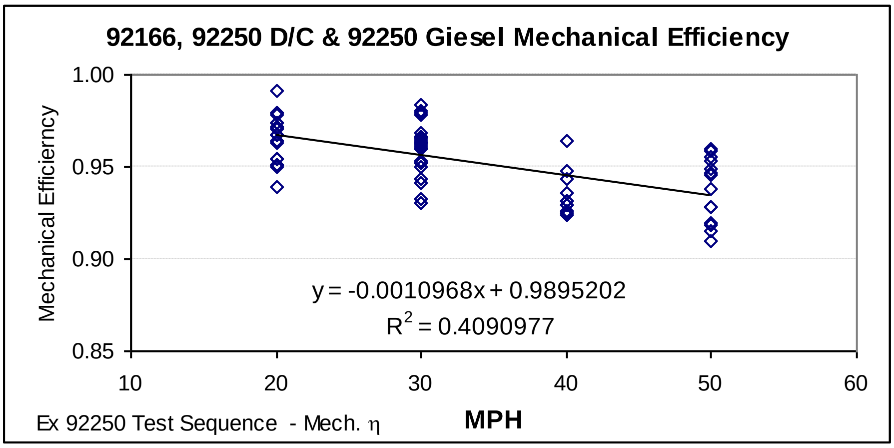

Figure 13. Expressed in Mech.η form, the Figure 12 data assumes a more orderly outcome with an unequivocal overall trend.

A similar exercise for the two 92050 test series produced a similar result – Figure 14

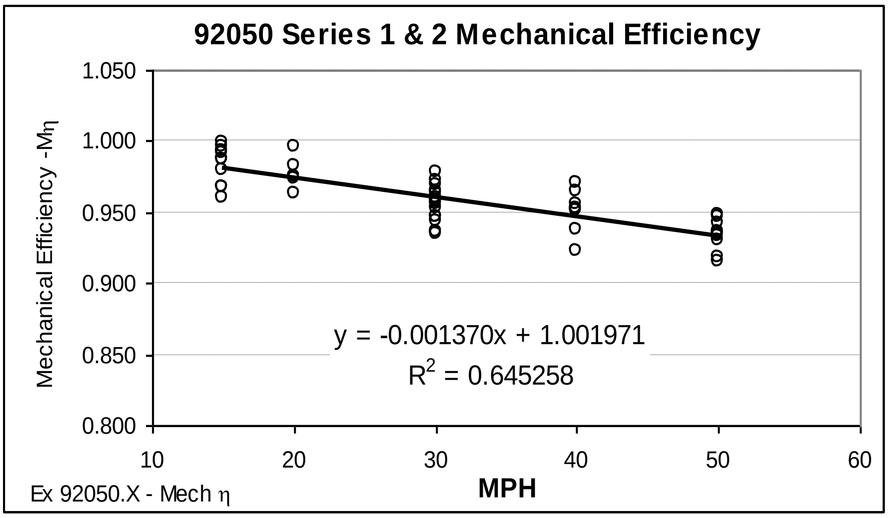

Figure 14. The overall trend and Mech.η values are similar to Figure 13.

The mechanical efficiencies for 92050 and 92166 & 92250 derived from Figs 13 & 14 are tabled below, they fall within +/-1%.

92050 & 92250 Mech. η			
92050 y = -0.00137x + 1.001971			R^2 = 0.6453
92250 y = -0.0010968x + 0.98952			R^2 = 0.4091
92050 & 92250 Mechanical Efficiency			
MPH	92050	92250 *	Δ Mech.η. 050 v 250
15	0.9814	0.9731	0.9%
20	0.9746	0.9676	0.7%
30	0.9609	0.9566	0.4%
40	0.9472	0.9456	0.2%
50	0.9335	0.9347	-0.1%
60	0.9198	0.9237	-0.4%
* Includes 92166 runs at 30 mph + 1 at 40.			

At face value the mechanical efficiency formulae as derived in Figures 13 and 14 provide a simple way of plotting WRHP across the speed range as a function of IHP, as exampled in Figure 15 below.

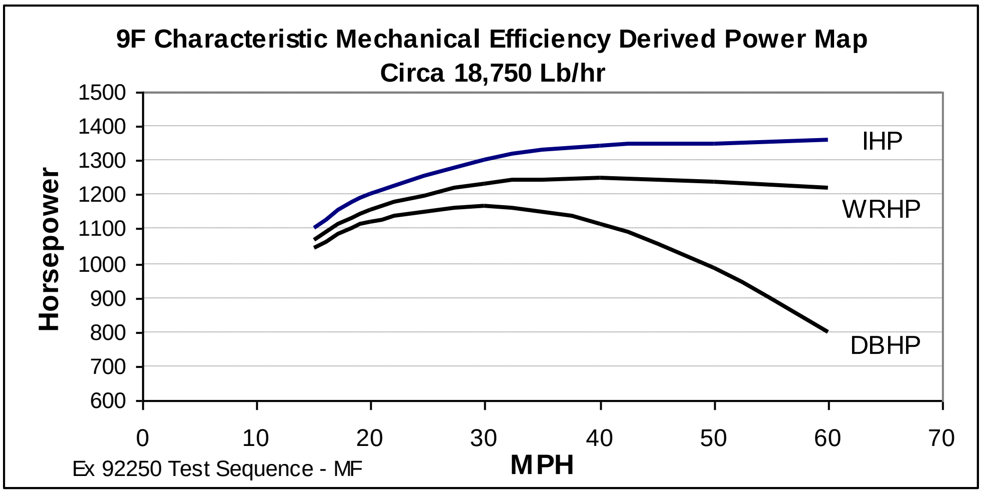

Figure 15. The average steam rates for Figures 13 & 14 data varied slightly for each speed set, the IHP values plotted here have been pitched to the mean rate. The DBHP curve assumes Report L116 Figure 3 locomotive resistance curve. Unfortunately, the Mech.η formulae are only a snapshot representative of the average steam rates obtaining for the available data sets, and cannot be used across the full working range, since the mechanical efficiency improves slightly with the level of effort - Figure 16.

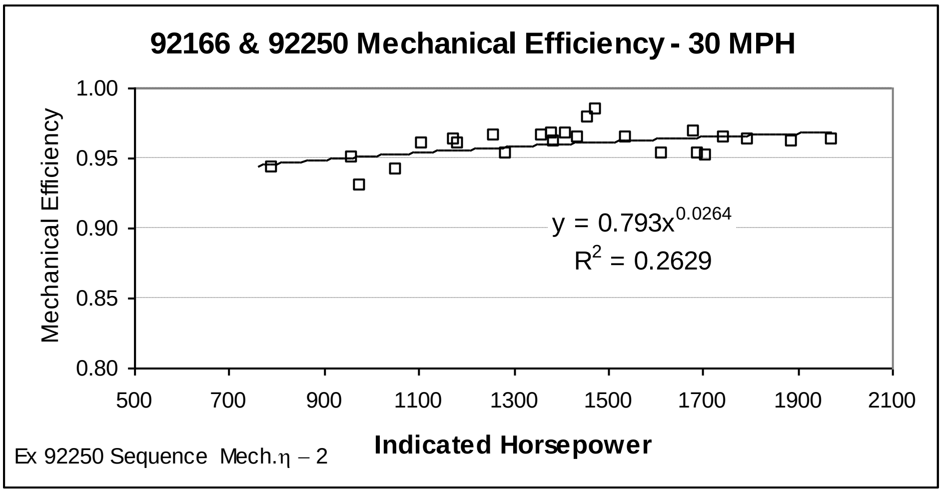

Figure 16 The somewhat scattered outcome and low R^2 value is characteristic of small differences and low rates of change. In this instance the spread is +/- 2.7%.

The small differences in mechanical efficiency for 92050 and 92250 tabled above notwithstanding, they are sufficient to generate significant differences in machinery friction outcomes at a given IHP power output, as tabled below.

92050 & 92250 MF Outcomes v IHP & Speed IHP

MPH	IHP	WRHP		MF LB		Δ MF HP 050 v 250
		92050	92250	92050	92250	
15	1275	1251	1241	592	858	-10
20	1400	1364	1355	668	851	-9
30	1510	1451	1444	739	819	-7
40	1560	1478	1475	773	795	-3
50	1590	1484	1486	793	779	2
60	1600	1472	1478	802	763	6

While in horsepower terms the discrepancies of up to 10 HP appear quite modest,
differences of over 250 lb at15 mph seems less impressive. So here we have equipment performing within the specified uncertainty, while the two WRHP sets at a given IHP and speed within 0.8% deliver measurably divergent MF outcomes.

Such differences fall within the expected range of experimental error, small wonder then, that Carling thought it difficult to confidently plot WRHP and likewise locomotive resistance. It is unlikely that such small differences are entirely down to experimental error alone. Given manufacturing limits and fits and such matters as machinery alignment and lubrication integrity, it does not seem remarkable to suggest that machinery friction for individual locomotives might vary by +/- half a percent, possibly more. Such small differences are more than enough to challenge the test engineer endeavouring to reconcile the divergent data of small differences. In WWII the performance of military aircraft as delivered was found to vary up to 2.5%. This was attributable to power unit variations and airframe quality, the latter having a long list of potential flaws. Obviously the scope for variation with a locomotive running indoors on a test plant is much reduced compared to aeroplanes, and anything serious will quickly manifest itself in the guise of hot boxes and so on. However, as already touched on, test outcomes will be sensitive to valve setting, other things being equal.

WRTE v ITE is Linear

That this relationship is linear is one of few certainties that emerges from the test data. Beyond that, when plotted, the outcome is not always reliable. For given types it appears unaffected by single or double chimneys, the Giesel ejector and blast pipe changes notwithstanding; ITE rules. The fundamental characteristic of the linear relationship is that as ITE increases WRTE increases at some slightly reducing overall rate (Figure 16). Such plots are confined to speeds sets, and if they provide only a few plots covering a limited range of power and steam rate, they sometimes deliver a trend line sloping the wrong way - falling from left to right. Such an outcome implies WRTE still available at zero steam rate. An outcome attributable to the vagaries of scatter.

The linear relationship is simple: $Y = fx – C$.

On occasion, notwithstanding a seemingly adequate number of plots and wide working range, the constant sign turns out to be positive. This again implies power at the wheel rim at zero ITE. This contradicts John Knowles assertion that more data axiomatically provides more accuracy. The reality is that some measurements are more accurate than others, and the sequence of delivery is entirely random. The nth plot might readily bring confusion where relative order otherwise prevailed. A good example is to be found in the data for 9F 92166 – Figure 17. In terms of WRTE v ITE, the outcome was in close accord with the data for 9F 92250, but the trend line constant for 14 tests at 30 mph delivered the wrong sign; WRTE cannot be positive when X is zero.

It took some weeding on a trial and error basis to eliminate the positive sign, the removed plots were randomly distributed – Figure 17B .

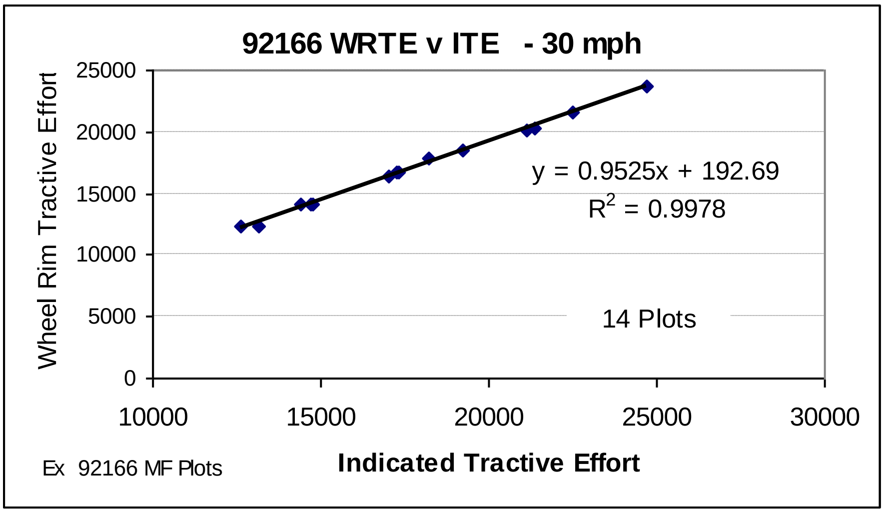

Figure 17 Visibly the scatter is low, as corroborated by the high R^2 value. However, delivering what would be 15 WRHP with the regulator closed is not to be countenanced (positive constant).

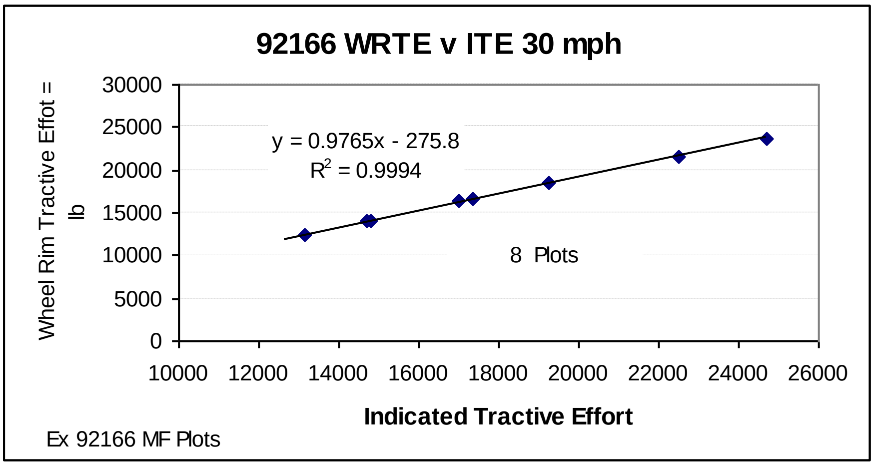

Figure 17B 40% fewer plots delivers a negative constant. Visible scatter reduced,
R^2 outcome improved.

Given sufficient range of output (more important than the amount of data), most WRTE v ITE plots are not troubling in the way of 92166 exampled above. An ‘untroubled’ example is shown below for 92250 – Figure 18

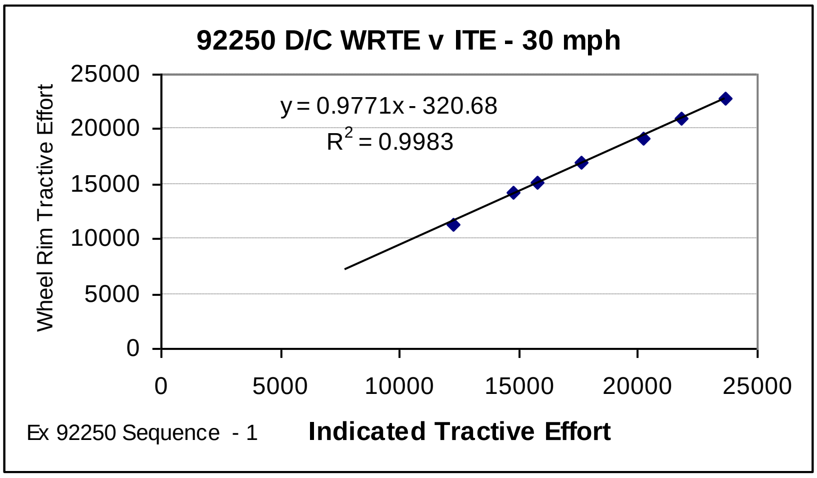

Figure 18 This straightforward relationship notwithstanding, note the slight differences in the x variable compared to Figure17B. This affects the slope of the trend line and thereby the derivation of the constant, which inevitably, will also differ. These small differences are the product of the random scatter, or may reflect slight differences resulting from manufacturing tolerances,. .

Looked at on an indices basis, the differences in the WRTE outcomes for 92166 and 92250 across the power range are negligible, under $^{1}/_{2}$%.

92166 v 92250 WRTE - 30 MPH				
ITE	WRTE		WRTE Index	
	92166	92250	92166	92250
10000	9489	9450	100	99.59
15000	14372	14336	100	99.75
20000	19254	19221	100	99.83
25000	24137	24107	100	99.88

However, when the small remainder problem raises its head, the MF outcomes are inevitably more tangible than a mere half a percent difference would seem to suggest.

92166 v 92250 Machinery Friction - 30 MPH				
ITE	Machinery Friction - LB		MF Index	
	92166	92250	92166	92250
10000	511	550	100	107.61
15000	628	664	100	105.71
20000	746	779	100	104.41
25000	863	893	100	103.46

It is all too apparent that small remainders (SRMs) can make mischief with trivial deviations in the cylinder ITE and WRTE data, *even within the supposed accuracy of measurement limitations.* Figure19 below plots the potential MF deviation ranges resulting from no more than 1.5% SRM compounded error.

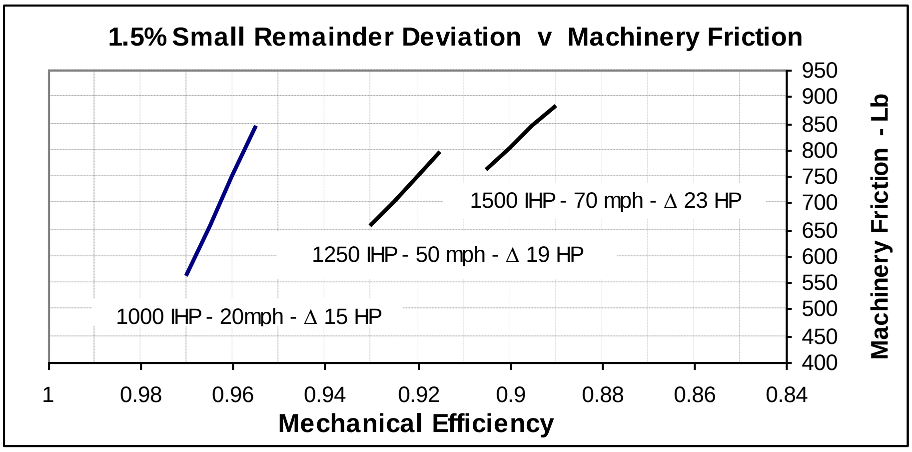

Figure 19 Given that Carling* put the accuracy of the Amsler dynamometer work
done measurement at 1½% and the Farnboro' indicator as "probably within 2% or less.", the scope for uncertainty is over 3%, and that's without things going wrong as they sometimes did. Carling* thought individual locomotives might vary by up to1%.

John Knowles call for around a dozen plots carries more weight in regard to small remainders. The random number experiments tabled below clearly support this point. The Rugby data sets are often limited to only a few plots at given speeds.

Randomised MF Outcomes @ 800 lb +/- 2% # 10 Data Sets of 10 Plots x 6 (20 to 70 mph)		
Average 600 Plots	782	98%
Set Minimum - 6 x 10 Plots	723	90%
Set Maximum - 6 x 10 Plots	847	106%
Average 10 x 5 Plot Sequences	682	85%
Minimum 5 Plot Sequence	379	47%
Maximum 5 Plot Sequence	1125	141%
# Randomised variation limit for ITE & WRTE entries		

……………………………………………………………………………………………

* Model Engineer 17 October and 7 November 1980

Uncoupled Locomotive Vehicle Resistance VRU – A Key Constant

Here we look at the "simple proof" alluded to earlier in this correspondence.

WRHP minus DBHP = VRU = a constant

The uncoupled vehicle resistance component of locomotive resistance, VRU, can be discovered by deducting the drawbar horsepower (DBHP) as derived from road tests, from the wheel rim horsepower (WRHP) as recorded on the test plant. If the test WRHP and DBHP data is accurate, this exercise *should return a constant VRU value for any given speed irrespective of power output and steam rate.* Such an outcome assumes the DBHP data has been regularised to a uniform situation in regard to wind and track conditions. The plausibility of this result, can be verified as within credible limits or otherwise by comparison with estimated values of VRU (VRUe) based on a body of empirical evidence in regard to the available experimental and technical data. The VRUe values calculated therefore represent a band of possibility within which the experimental VRUx values should fall. Where wind conditions pertaining for the road tests are known, as in the case to be exampled, the 'band of possibility' can be narrowed down to some extent. VRUx indicates as derived by experiment from the test plant WRHP in association with the road test data. For an examination of LR, MF and VRU, the following relationships obtain:

LRHP = IHP - DBHP (1)

WRHP = IHP – MFHP (2)

MFHP = IHP –WRHP (3)

VRU HP = LRHP – MFHP (4) & WRHP – DBHP (5)

LRHP = MFHP + VRU HP (6)

DBHP = IHP – LRHP (7) & WRHP – VRU HP (8)

These same relationships apply where using force, i.e.; ITE, WRTE, DBTE.

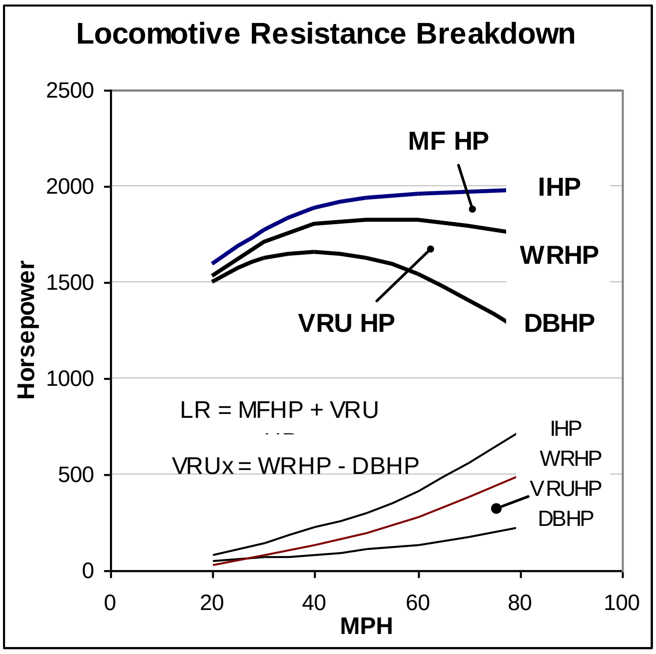

Figure 20 Plotted curves are notional values,

VRU Comprises 3 Elements

1, The rolling resistance of the locomotive and tender carrying wheels. This element is absent for tank locomotives without carrying wheels such as 0-6-0Ts etc.

2. Vehicle resistance is usually expressed in the form: $R = A + V/B + V^2/C$ Lb/ton, where the 1st term A represents rolling resistance as 1 above, and is assumed, as a convenience, to be a fixed value independent of speed. The

2nd term is attributed to the track and ride losses resulting from the behaviour of the vehicle and its interaction with the track. This term is usually derived as the remainder after the rolling resistance and aerodynamic drag (3rd term), has been deducted from the total resistance as established by experiment. The extent to which the 2nd term losses are replicated at the coupled wheels of a locomotive working on the test plant rollers is uncertain. These losses running on the spot will be reduced to some extent The absence of percussive rail joint losses on the rollers is estimated to save 0.015V pounds per ton.* Since the rollers are mounted on more solid foundations, further reductions are probable given the behaviour on the more flexible permanent way and track bed. In reality the 2nd term would also include an element of coupled wheel rolling resistance since this gradually increases with speed (ZN/P); this occurs on both plant and track.

3. The 3rd term, an intrinsically squared function, is exclusively ascribed as aerodynamic drag in regard to rolling stock. Where locomotive resistance as determined by experiment is concerned, the 3rd term will also include an element attributable to the dynamic losses of the motion and coupled wheel windage, which will occur as part of the power transmission losses (MF), and not as part of the uncoupled vehicle resistance losses, VRU, as considered here.

Aerodynamic drag is problematical since it is a variable subject to the moods and direction of wind, which potentially, may have a significant impact. Although aerodynamic drag can be estimated for an assumed set of conditions in regard to speed and direction, it will always remain an estimate of some uncertainty. Wind conditions tend to vary by the hour if not the minute, and are constantly affected by the shifting local topography. Some of the Swindon derived test bulletins declared wind conditions: a $7^1/_2$ mph, 45^0 headwind, and later 10 mph un-vectored; such specific information was absent from Rugby/Derby derived test bulletins and reports.

Test Bulletin Locomotive Resistance.

The test bulletins mostly return constant locomotive resistance at given speeds across the full working range. In some instances, including the Duchess, Report R13, deducting DBHP from IHP returns increasing LR with the level of effort; likewise the 9F bulletin. Assuming the data is regularised for a constant wind condition, then the VRU value at a given speed is a constant. This obtains whether it is VRUx as determined from deducting DBHP from the experimental WRHP, or using a VRUe estimate to crosscheck VRUx. Accurate WRHP data (assuming reliable DBHP values) theoretically returns constant VRUx values at a given speed across the working range. Such is the case for 46225 as below.

Scope of Experimental DBHP Data.

To determine cross checks on a VRUx based analysis it is necessary to have reliable DBHP data, so this potentially limits the types available for examination to the Duchess,. The Derby derived DBHP data for the Britannia, BR5, and the 9F is unreliable – Report L116. A Crosti locomotive resistance curve is included in L116, also for a standard 9F, and for the Duchess in Report R13.

----

* *How Long-Welded Track Aids the Rolling Stock Engineer*, J K Koffman, *Modern Railways* May 1965. *Traction Supplement*, D H Landau 1998.

The Duchess, 46225 (Report R13), incorporates DBHP data across the speed range, as determined by Report L109 and the L109 Supplement. The road tests for the 70005, 73008, 92050 and Crosti 9F 93023 were carried out under the "controlled road test procedure", as pioneered and developed by Sam Ell at Swindon in the early post war years, by the Derby road test team. The nub of this concept was maintaining a constant steam rate throughout the test period irrespective of changes in speed. It was claimed such control could be maintained by working at a constant blast pipe pressure. Given this assumption it was concluded by the Derby test department that this rendered indicating on road tests redundant, since, if the steam rate was so controlled at a known steam rate using the blast pipe pressure as a meter, backed-up by Sam Ell's 'summation of increments' procedure, the IHP data as determined at Rugby would be automatically replicated on the road tests. As things turned out this proved not to be the case. At a given steam rate, blast pipe steam temperature falls as speed increases. Since cylinder efficiency increases with rising speed, increasing the heat drop resulting in falling exhaust temperature and increased steam density, steam flow variations with speed at a given blast pipe pressure will occur. A problem was first suspected on the B1 road tests in 1951; action was long delayed.

Realisation of the problem eventually heralded the reinstatement of cylinder indication on road testing and periods of constant speed testing were also reintroduced, as applied for the Duchess road tests. As a consequence of this problem, the road test DBHP data for the B1, Britannia, BR5, 9F and Crosti 9F was compromised; the actual working steam rate tending to be lower than assumed at the lowest speeds and higher at the highest, and only coincident somewhere in the middle speed range. Consequently DBHP tended to be under recorded relative to what the supposed steam rate would have produced at the low end of the speed range and over recorded at the upper end. The resulting locomotive resistance curves were of strange form and improbably flat when extracted from the test bulletins. This problem gave fruit to Reports L109 (Duchess road tests), and L116 (9F & Crosti 9F), which investigated the roots of the problem and developed a procedure for correcting the road test data in line with the true steam rates obtaining. The report included before and after locomotive resistance curves for the Crosti 9F and an LR curve for the standard 9F. When the latter is plotted against the LR curve as derived from the test bulletin, these lines cross at about 39.5 mph; and likewise for the Crosti as first determined from the road tests, and as the corrected LR curve.

On the assumption the equivalent null point for the BR5 and BR7 would be at the same piston speeds as the 9F, it would occur at about 48 mph. The relative blast pipe areas differed however, on an index basis: BR7 = 100, 9F = 95 and BR5 = 91. This may have influenced the outcome beyond piston speed alone. Notwithstanding the many test runs conducted on the test plant, the data available for individual locomotives is sometimes quite limited in scope. In the case of the Duchess for example, adequate IHP and WRHP data is only available at 50 mph. Comprehensive IHP and DBHP data plus a locomotive resistance curve is available from report R13 based on report L109 and the "L109 Supplement". It is fortunate that at 50 mph the road test steam rates were in accord with the theoretical Rugby values throughout the working range, so the Rugby IHP determinations could reasonably be assumed as having been replicated. Report L109 investigated departures from steam rate over the working speed range, and determined the actual steam rates obtaining in regard to the recorded DBHP. "Corrected" DBHP curves were produced accordingly and these were incorporated in the final report. Oddly, the drawbar figures in the 9F report were as uncorrected, notwithstanding that report L116 was issued a year before the 9F test bulletin was published. Internal correspondence reveals E S Cox was unwilling to accept the idea of steam rate deviations; as being without a theoretical basis, and likely simply a case experimental error. At this point a departmental impasse is apparent. Exhaust steam temperature and specific volume at a given pressure falls with rising cylinder efficiency (density increases) as a function of speed and heat drop. Road test steam rates could deviate from the assumed value by over 1000 lb/hr.

46225 - A VRU Test Case

The available test plant ITE, WRTE and MF data at 50 mph for the Duchess, 22 plots,
is set out in Figure 21.

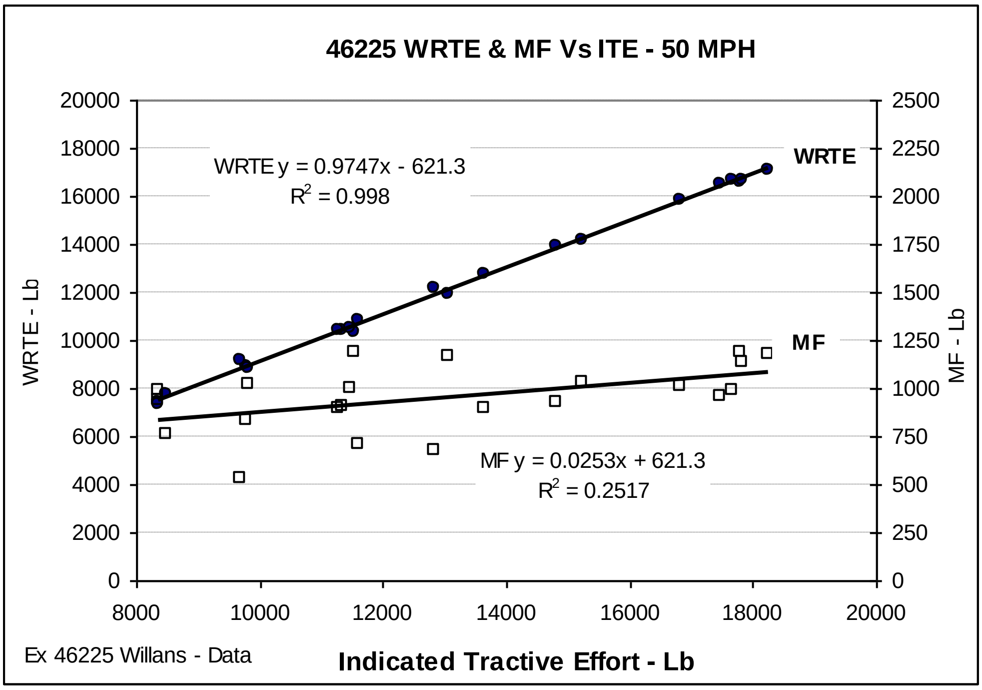

Figure 21 A similar chart using only15 of the available plots appeared in my letter 17 March 2017.This yielded the formula WRTE = 0.9708 – 545 lb.

The differences in the MF outcomes are slight. .

46225 MF Outcomes - 50 mph.				
IHP	1000	1500	2000	2500
15 Plots	764	874	983	1093
22 Plots	811	906	1001	1095
Δ MF Lb	47	32	18	3
Δ MF HP	6	4	2	0

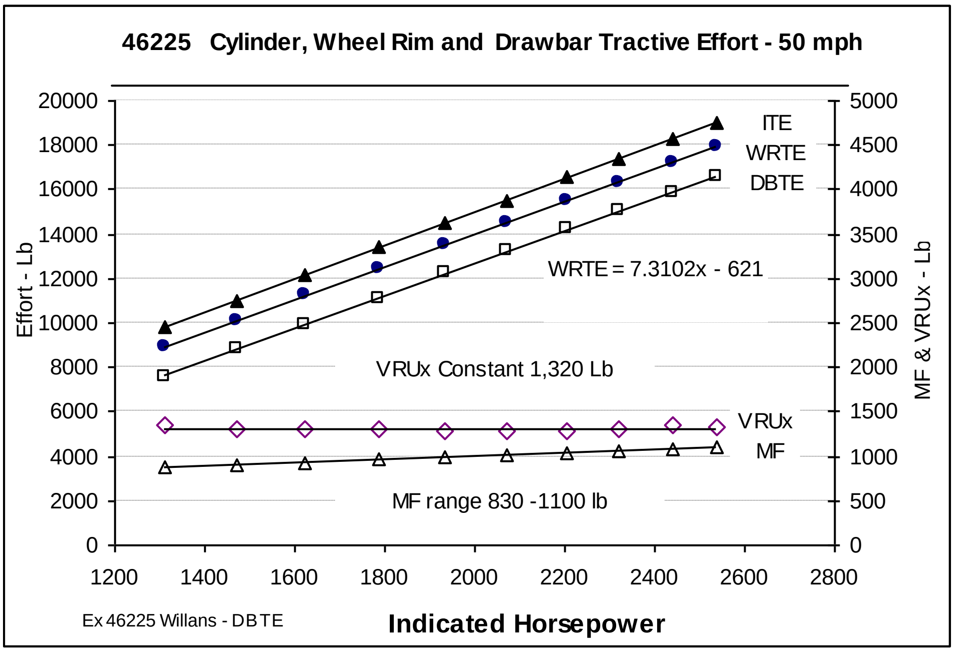

Figure 22 The WRTE & MF plots are 'smoothed' as derived from Figure 21. The VRUx scatter is within the range + 21 - 9 lb. The bulletin graphs are not drawn with tool room accuracy, likewise recovering said data by scaling off is short of high precision. The DBHP Willans Lines so derived from L109 return high R^2 values, sometimes achieving unity, but this is no guarantee of spot-on determinations.

The Report R13 locomotive resistance curve is in lb/ton (Figure 18). At 50 mph the LR is given as 14.4lb/ ton; 2327 Lb in total. This is coincident with a steam rate of 30,000 lb/hr, a coal rate of 4,110 lb/hr, IHP 2072. The smoothed experimental data for ITE, WRTE, MF and Report R13/L109 DBTE, and the derived VRUx values are plotted above in Figure 22. Since LR = MF + VRU (5), then:

ITE @ 2,072 IHP = 15,540 Lb; WRTE 14,526 Lb; MF 1,014 Lb + VRUx 1,320 Lb
= LR 2,335 lb. Report LR at 2,327 lb is effectively identical..

Tabled below a VRUe estimate for the Duchess. It is assumed the 2nd term losses for the coupled wheels will be reduced to some extent when running on the test plant relative to the losses that occur working out on the line. This reduction occurs on two counts. Firstly the percussive losses at rail joints will be absent, and secondly, given the more solid foundations of the plant, the degree to which the adhesion weight LR 2nd term ride and track losses are encountered on the test plant. It seems likely that these losses will be reduced running on of the test plant. In this example the plant losses appear reduced to around 60% relative to what is normally encountered on the more flexible track and track bed of the permanent way. Obviously, given the estimated make-up of VRUe, this determination is tentative.

Most of the limited WRHP data available for 46225 is at 50 mph, this was coincident with the speed at which *the assumed steam rate was accurately replicated on the road tests.* The Derby Farnboro' indicator was deployed throughout the road tests. The comparative Rugby plant and Derby road test indicated horsepower results were in agreement at 50 mph: no revision of road test IHP and DBHP data applicable.

46225 Estimated VRUe 50 mph *			
Uncoupled Wheels 1st Term			R Lb
Bogie	2 x10.75 tons	4.45 lb/t	96
Truck	1 x 16.8 tons	3.75 lb/t	63
Tender	3 x 18.8 tons	2.8 lb/t	188
Uncoupled 2nd Term 94.65 tons		3.125 lb/t	296
Aero $3^1/_2$ mph 45^0 Headwind			645
Coupled Wheel Percussion Losses		0.53 lb/t	50
Coupled Wheel Track & Ride Losses **		0.5 lb/t	34
Total VRUe (= VRUx + 4% = 52 lb, 7 HP)			**1372**

The wind conditions for the road tests over the S & C are on record and were atypically moderate. The VRUx and VRUe outcomes in this instance are tolerably close. On the basis of these figures about 40% of the 2nd term coupled wheel LR losses are avoided when running on the test plant. The remaining 60% will primarily relate to the journal ZN/P losses and the coupled wheel windage as part of the overall machinery friction. The modest track ride losses are based on a relatively recent paper on train performance hailing from the USA. **

----

* 1. The 1st term as tabulated is based on bearing loadings, mechanical advantage, and friction coefficients derived from Ell's wagon resistance data in his 1958 I. Loc. E paper; *The Mechanics of the Train in the Service of Railway Operation.* It's purely a mathematical fit to the data, effectively a rolling resistance constant, excluding the ZN/P frictional speed increment.

2. The 2nd term assessment assumes some of the normal coupled wheel adhesion weight track and ride losses will be absent when running on the test plant. Namely the percussive losses at the rail joints and some of the losses involving the ride interaction with the track and track bed. The rail joint losses were determined some years ago from an article by J L Koffman: *How Long-Welded Rail aids the Rolling Stock Engineer*, Modern Railways, May 1965. $R_p = 0.015V$ lb/ton.

3.. The aero term assumes a drag coefficient of 0.77 as LMS wind tunnel tests, a net frontal area 101.5 sq.ft and a $3^1/_2$ mph headwind. The latter value is the average of the road test wind record.

** *Train Performance: AREMA Manual for Railway Engineering*- American Railway Engineering and Maintenance-of-Way Association,1999. It elegantly described these losses as attributable to the "wave action of the rail".

Drawbar Horsepower Derived Locomotive Resistance

Back in 2013 I investigated the veracity of the Duchess resistance curve included in the Report R13. The resistance curve was regarded by many as being too low. The examination subjected the data to four tests which were satisfied (DHL R13 Audit). The 4th test was the derivation of locomotive resistance from the DBHP data.

This method of approximating LR is derived from the zero root point of DBHP Vs Steam rate linear trend lines at given speeds, the root point (negative value**)** being representative of LR (Figure 23). The proximity of these results to the R13 LR HP curve is striking – (Figure 24). The underlying theoretical point is that no horsepower appears at the drawbar until the locomotive resistance has been overcome. The linear projections represent the tangential mean of the recorded data. Having explored this method extensively, the outcomes are very sensitive, notably at low speeds, to the steam rate range selected to find a tenable data set. There is some scope for geometric mean solutions; in the case of the R13 data, this proved unnecessary, no weeding required.

This method was inspired by reading Stanley Hooker's autobiography *Not Much Of An Engineer*, Hooker was an engineer at Rolls Royce, initially specialising in super chargers. Backwards projection was used to determine aero engine frictional losses.

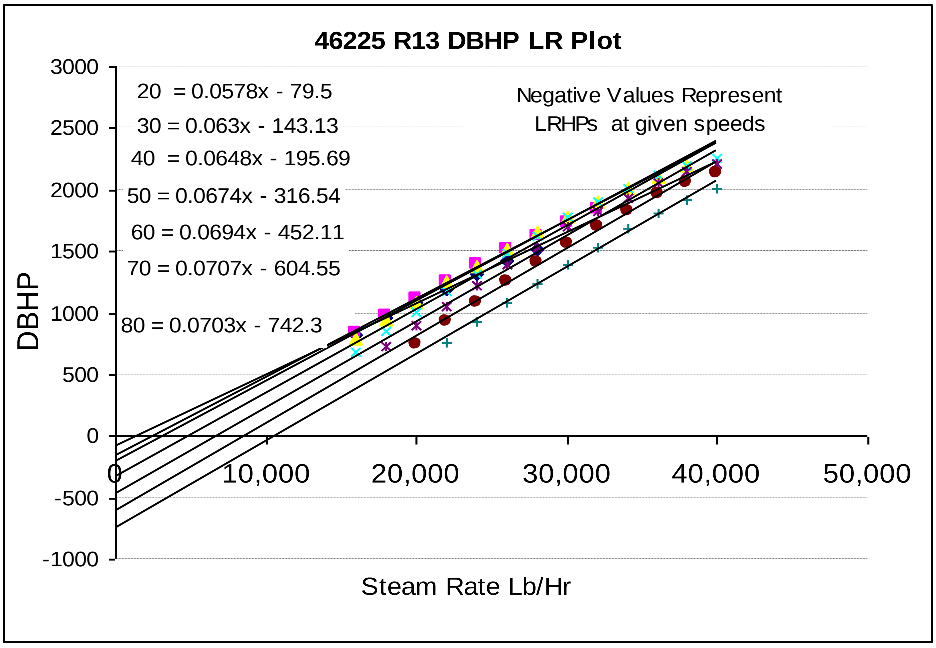

Figure 23 The plotted data covers the full test bulletin power envelope. The outcomes theoretically approximate to mean steam rate LR.

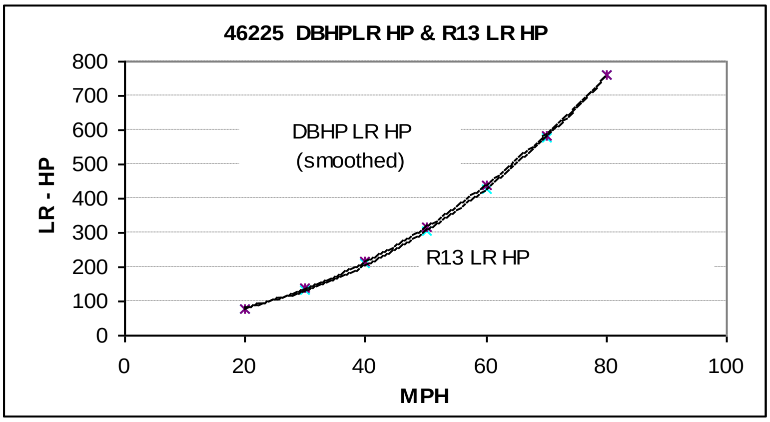

Figure 24 The smoothed DBHP derived LR HP is barely distinguishable from the Report R13 Figure 18 derived LR HP.

Road Test Steam Rate Anomalies

Report L116 treating the steam rate anomalies in regard to the Crosti and Standard 9Fs showed, as with 46225 (Report L109), the same trait of deviation in steam rate at given speeds across most of the working range. The machinery friction for Crosti 9F 92023 as tested at Rugby was significantly higher than as recorded for the standard 9Fs tested on the plant. This difference was confirmed in road tests as below.

LMR No.3 Dynamometer Car and Mobile Test Unit *
Steam Rate 16,000 lb/hr

Speed MPH	Drawbar Horsepower (DBHP)	
	Crosti 92023	Standard 92050
20	862	917
30	900	960
40	875	939
50	827	903
Average	866	930

The Crosti drawbar deficiency was 55, 60, 64 and 76 HP for the speeds shown. This was attributable to reduced indicated horsepower of the Crosti resulting from higher back pressure (offset to some extent by higher superheat), and increased machinery friction as evidenced on the test plant. Subsequently, 92050 underwent further tests at Rugby eighteen months later to “resolve perceived differences between results obtained on the stationary test plant and the road tests.” No indicating was carried out on the standard 9F and Crosti road tests.

The nominal road test steam rates were not held constant across the speed range, tending to increase with speed, the test plant indicated horsepower/steam rate only being replicated on the road tests at about 39.5 mph. The steam rate deviations as determined in report L116 were significant.

Post the road tests, some satisfactory comparative tests between the Rugby and Derby versions of the Farnboro indicators were conducted at Rugby in 1957: 92050 Series 2 tests . These tests post-dated the significant improvements to this equipment reported by Ron Pocklington.

92050 Comparative Indicator tests IHP Indices 1957						
Steam Rate	IHP - Rugby-Derby Mean Value Indices					
	15 MPH		30 MPH		50 MPH	
	Rugby	Derby	Rugby	Derby	Rugby	Derby
12,300			100.6	99.4		
13,100	99.9	100.1				
14,900			99.8	100.3		
15,500			100.4	99.6	99.1	100.9
16,150	99.3	100.7				
17,400					98.4	101.6
18,500			98.8	101.2		
18,900	98.4	101.6				
19,100			99.3	100.7		
19,500					101.1	99.0
19,750	99.8	100.2				
21,400					100.1	99.9
22,400	100.4	99.6				
23,400			100.4	99.6	100.4	99.6
Averages	99.6	100.5	99.9	100.1	99.8	100.2
Averages	All Rugby		99.75	All Derby		100.26

----

*A Detailed History of British Railways Standard Locomotives, Vol. 4: The 9F 2-10-0 Class, page 217. RCTS, 2008

The 92050 Series 2 tests at Rugby in 1957 returned reduced IHP and WRHP outcomes relative to the 1955 Series 1 tests. The Series 2 tests recorded higher exhaust steam temperatures for given steam rates at 30 and 50 mph. (Comparative data at other speeds unavailable). Such an outcome is symptomatic of steam leakage, The Series 2 tests also showed an increased steam consumption of around 2 percent at a given cut-off. 92050 was in traffic for 18 months between the Series 1 and Series 2 tests 92050 and will have clocked up around 35,000 miles in the interim. The BR Standards with the 3 bar crosshead slidebar arrangement were notorious for high piston valve ring and piston ring wear.

92050 Test Series 1 & 2 IHP & WRHP Comparison - 50 mph						
Steam Rate	IHP Willans 50 mph			WRHP Willans 50 mph		
	16,000	20,000	24,000	16,000	20,000	24,000
Series 1	1,170	1,500	1,770	1,090	1,415	1,680
Series 2	1,100	1,415	1,670	1,010	1,315	1,562
S2 Δ HP	-70	-85	-100	-80	-100	-118
S2 Δ HP %	-6.0%	-5.7%	-5.6%	-7.3%	-7.1%	-7.0%
The Series 1 tests 1955, and the Series 2 1957 tests post dated the final improvements to the Farnboro Indicator early in 1955.						

The comparative exhaust temperatures are consistent with increased leakage for the Series 2 tests – Figure 25. Curiously the 9F test bulletin IHP appears to have combigned and thereby averaged the Series 1 and 2 IHP data. Possibly this was a deliberate decision to reflect typical operating conditions.

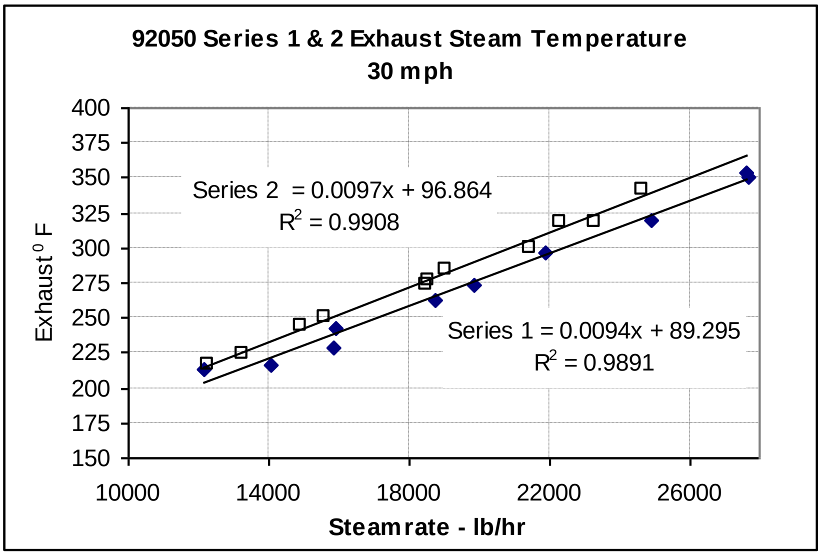

Figure 25 The higher exhaust temperatures of the Series 2 tests are indicative of increased steam leakage. This may occur as both a constant loss to atmosphere from the steam chest, and a cyclic loss via the cylinder during compression, admission and expansion.

The apparent and eccentric road test locomotive resistances of Crosti 9F 92023 and 9F 92050 were subject to correction in Report L116, after adjustment for significant steam rate departures from the assumed constant rates. These deviations from the nominal test rate could be over 1000 lb/hr, positive and negative, crossing over from negative at some point roughly two thirds through the speed range.

Report L116 gives 'before and after' LR curves for the Crosti, and an LR curve for the standard 9F. The degree of adjustment for the Crosti was striking (Figure 26). The standard 9F Report L116 LR curve was of similar form and crossover point relative to the
9F LR curve as derived from the test bulletin.

The outcome of the steam rate deviations, aside from the crossover point, was that the recorded DBHP related to other than the supposed steam rate and related Rugby IHP data, hence the eccentric L116 LR curves as initially derived from the road tests.

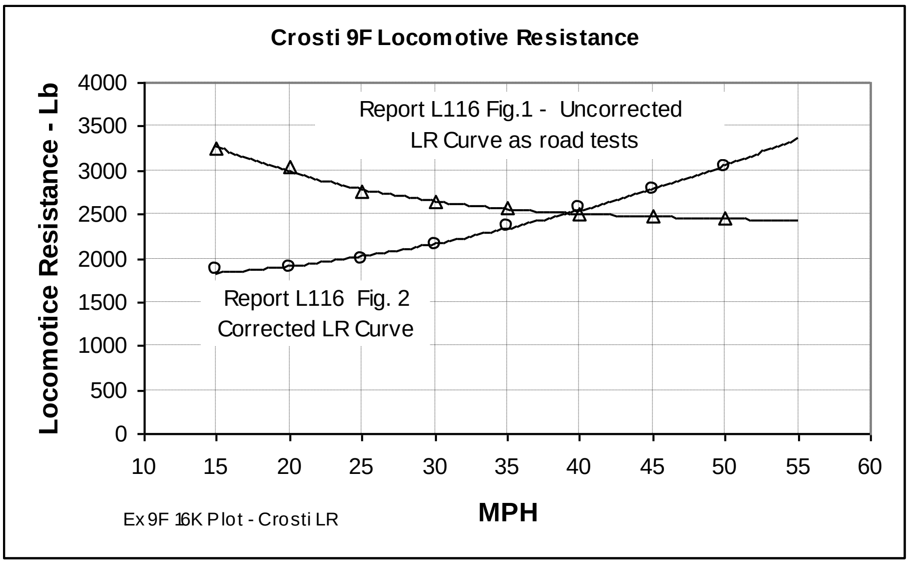

Figure 26 The uncorrected curve reflects a trend for the steam rate fall below the nominal test rate as an inverse function of speed, an inishing to zero at the crossover with the corrected curve, and function of speed thereafter. A similar pattern is apparent for the L116 Fig. 3 LR curve when plotted against the LR curve derived from bulletin. Both the Crosti and standard 9F share a common crossover 39.5 mph. The steam rate anomalies for Duchess 46225 as evaluated L109 follow a similar pattern; crossover point 50 mph. The BR5 relative to the estimated LR (dashed lines) is less distinct.

initially to
error dim-
increasing as a
standard 9F
the test
point of
in Report
crossover

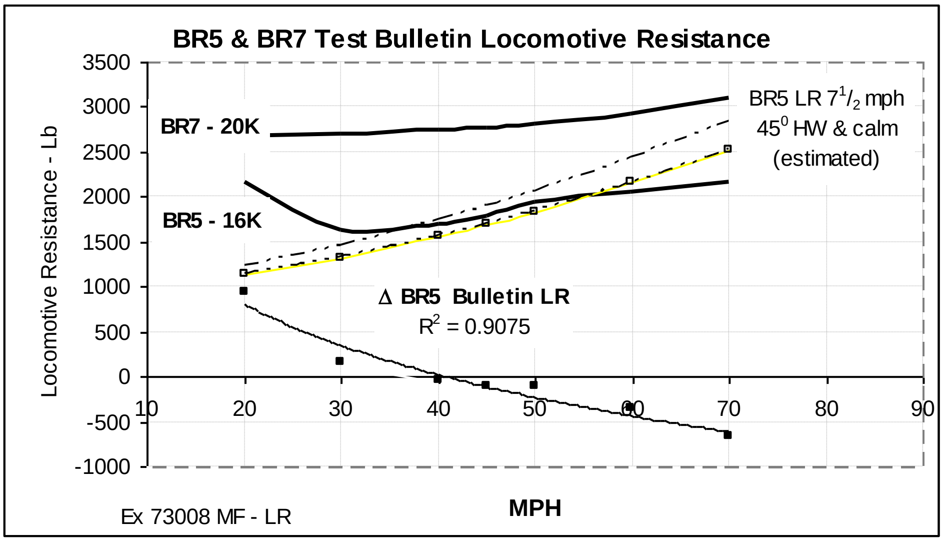

Figure 27. The high flat lining LR curve for the BR7 is an extreme example of how things could go wrong. The BR5 appears somewhat undecided, with a plausible outcome somewhere in the middle steam range. The falling error curve shown is for the test bulletin derived curve difference relative to the estimated LR curve for $7^1/_2$ mph headwind.

The key change increasing steam rate with speed at a given blast pipe pressure is the fall in exhaust steam temperature and density that accompanies increasing cylinder efficiency and heat drop as exampled below for the BR5. An characteristic example of along the lines of Report L116 Figure 11 is portrayed in Figure 28.

On the basis of piston speed relative to the 9F, it has been calculated that the point of zero steam rate error on the road tests would occur at 48.7mph, this is considered sufficiently close for the test bulletin DBHP curves for 50 mph to be suitable for the analysis, as set out in Figure 29, as derived from the procedure set out for Figure 22.

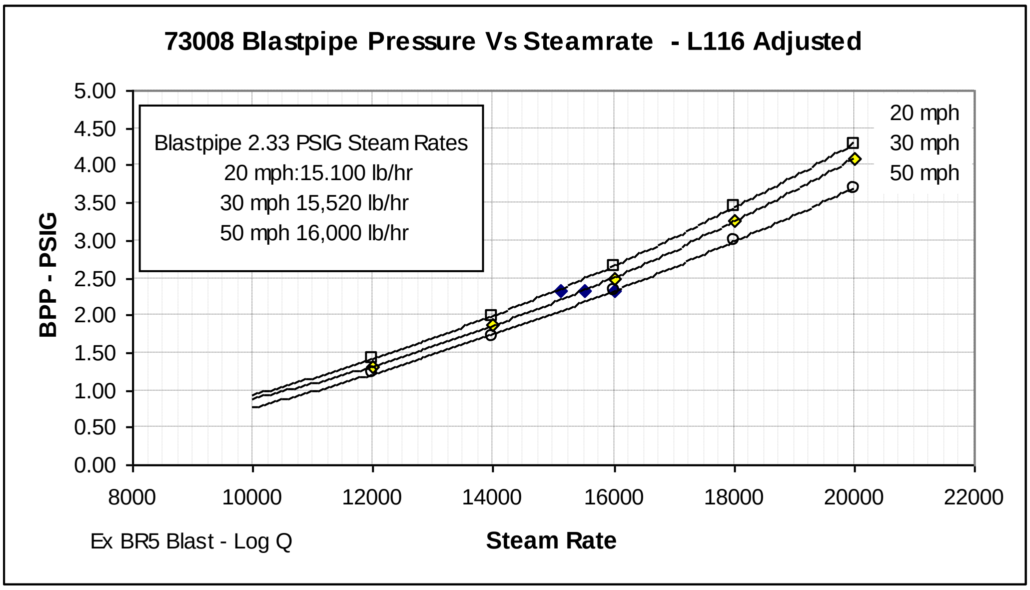

Figure 28 This is of equivalent form to Figure 11 for 92050 in Report L116, as determined from Rugby test plant experimental data using the Log Q = Log C + n Log P relationship.

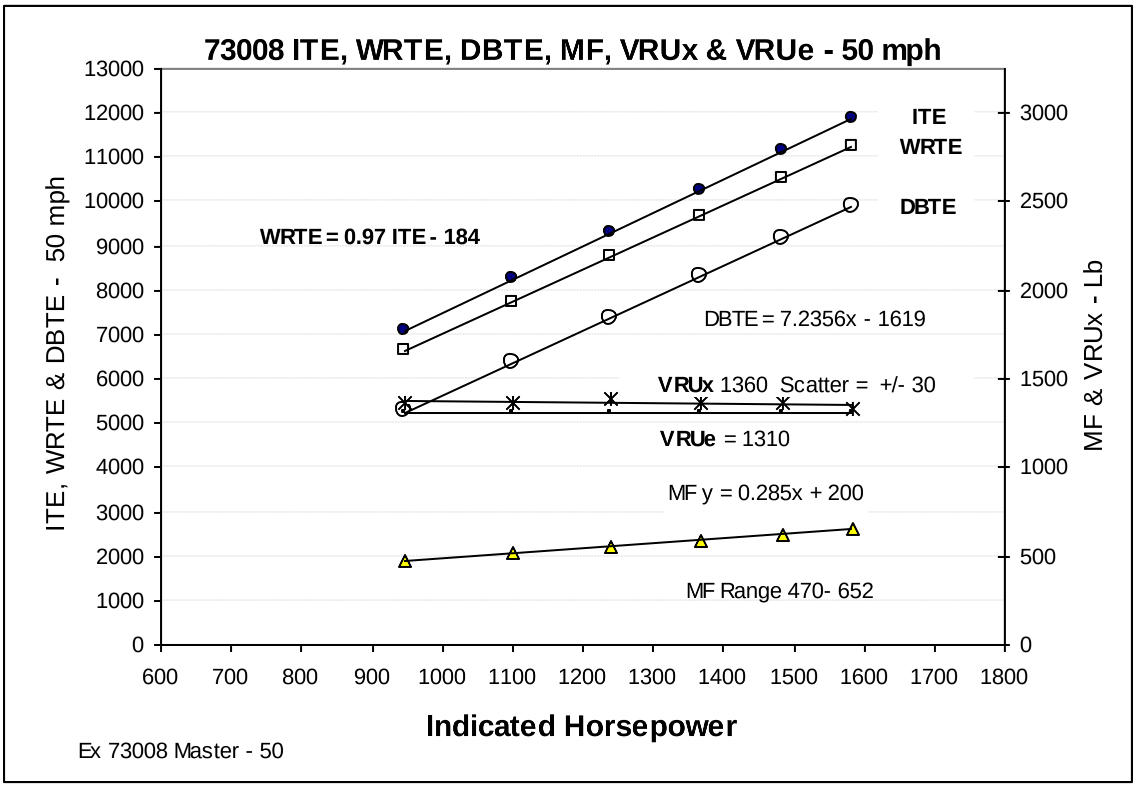

Figure 29 The IHP/ITE data used is as test bulletin, WRTE as Rugby Willans Lines; 7/2 mph 45^0 headwind assumed. In the event, the BR5 road tests were subject to unusually high wind speeds averaging 14 mph south westerly – 270^0; as derived from Beaufort Scale median values. Line headings Carlisle – Appleby SE (135^0); Appleby – Settle Jcn SE (170^0).

BR5 73008 Figure 29 LR Derivations 50 mph			**73008 Estimated VRUe - 50 mph** *	
Steam Rate	18,000 lb/hr	24,000 lb/hr	Uncoupled Wheels 1st Term	R Lb

IHP	1238	1580	Bogie	2 x 8.95 t	5.27 lb/t	94
ITE	9,285	11,850	Tender	3 x 16.4 t	3.94 lb/t	194
DBTE	7,353	9,813	Uncoupled 2nd Term 67.1 t		3.125 lb/t	210
LR	**1,932**	**2,037**	Aero $7^1/_2$ mph 45^0 Headwind			739
MF	553	650	Coupled Wheel Percussion Losses		0.75 lb/t	44
VRUx	1360	1360	Coupled Track & Ride Losses **		0.5 lb/t	29
LR	**1,913**	**2,010**	Total VRUe			**1310**
Figure 27 Estimated LR 50 mph - 2054 lb			∆ VRUx v VRUe = 50 lb, 7 HP			

A "Simple Proof" along the lines of the Duchess procedure Figures 21 & 22 has also returned constant VRUx of 1190 lb for the 9F at 40 mph. The speed was selected on the grounds that there was minimal departure from the supposed steam rate, corrections unnecessary, the bulletin DBHP curves at 40 mph were assumed satisfactory. At 1190 lb the VRUx plotted scatter was +/- 35 lb, +/- 4 HP.

92050 16,000 lb/hr - 40 mph	
IHP Bulletin Figure 11	1115
DBHP Bulletin Figure 2	899
Fig. 11 - Fig. 2 = LR - Lb	2025
MF - Lb	796
VRU = LR - MF Lb	1229
VRUx (∆ VRUx v VRU = - 4 HP)	1190
L116 Figure 3 LR - Lb	2062
∆ Fig. 3 LR v Fig. 11 - Fig.2 LR	37 Lb, 4 HP

A Simple Proof?

While the simple proof described appears satisfied within tolerable limits, SRMs are not a simple case for verification, as compound errors they are beyond simple calibration, and therefore best avoided where alternatives exist.. Many of the measurements on a locomotive testing station involve complex instrumentation subject to finite degrees of potential error, which though small, is sufficient to play havoc in the small remainder situation. Such outcomes are the inevitable result of randomised scatter, a problem considered further in the addendum. Absolute proof is elusive. As far as is practicable, the constant VRUx outcome "simple proof" has been demonstrated for 46225, the BR5 and the 9F. Given all this, some prerequisites must be satisfied:

1. Repeatability.

Though combigned WRHP Willans Lines for locomotives of the same type have returned high R^2 values and generally low scatter with few 'strays', this is not proof in itself. Systematic errors may occur. Willans lines do however return relative order whereas the small remainder MF outcomes deliver confusion; hence the low R^2 values. Repeatability nevertheless remains a prerequisite of proof, but SRMs are unlikely to be of any use in this regard. Plots of WRTE against ITE are generally even better behaved than Willans Lines, but even when returning visually near identical trend lines as plotted immediately below, the curve fitting formulae may return little agreement regarding the coefficients and constants involved as exampled in Figure 30.

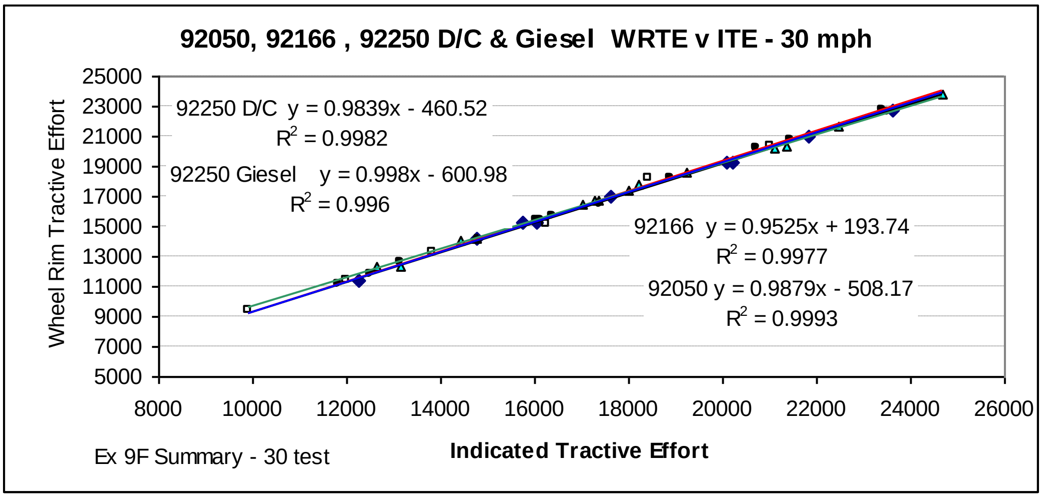

Figure 30 The four trend lines bundled together here are indistinguishable over the middle range. Of the four constants, three are of the same sign and general order of magnitude. Perversely, such are the joys of random scatter, 92166 contrives to change both sign and magnitude. (This was corrected above - Figure 18).

An assumption that for a given indicated tractive effort and speed, machinery friction will be the same, irrespective of the back pressure and superheat obtaining resulting from changes in blast pipe area, appears to be bourn out by the pooled data, as for the 9Fs plotted in Figure 30 The 92250 Giesel data, comprising only 6 MF plots, has been combigned with the 11 plots available in double chimney guise yielding outcomes, along with those for 92050 and 92166, as tabulated below.

9F Collective WRTE v ITE Machinery Friction Outcomes @ 1600 IHP, 20,000 lb ITE - 30 mph					
Engine	Plots	R^2	Formula	20K ITE MF	20K ITE MF HP
All	44	0.9978	y = 0.9779x - 308.16	710	57
92050	12	0.9993	y = 0.9879x - 508.17	750	60
92166	15	0.9977	y = 0.9525x + 193.74	756	60.5
92250	17	0.9974	y = 0.9865x - 476.3	746	60
Averages		0.9981	y = 0.9820x - 390	740	59

The MF returns, representative of an effort of around 24,000 lb/hr steam rate, fall within +/- 2 HP, 25 lb of the mean value. While not proof of accuracy in itself, it does satisfy the repeatability criteria, and even then, only up to a point. As will be seen the various formulae fitted show differences in the x coefficient, representing the work sensitive friction coefficient (1-Function x), and more markedly for the constants, including the anomalous positive constant for 92166 (as examined above- page 16). The x term outcome is very sensitive to the tilt generated by the random scatter of the data set. It is noted that 92166 returns the highest implied frictional coefficient, approaching 5%, and that a false compensating positive constant is returned in order to fit the recorded values.

The 92166 IHP and WRHP SSC curves return mediocre R^2 values, 92166 involved a mechanical stoker, and allowing for the steam consumption involved may on occasion have led to some miscalculation of the steam reaching the cylinders. Given this possible potential for error, or for whatever reason, the ringed SSC plots below possibly relate to steam rates other than shown. The R^2 values are accordingly compromised.

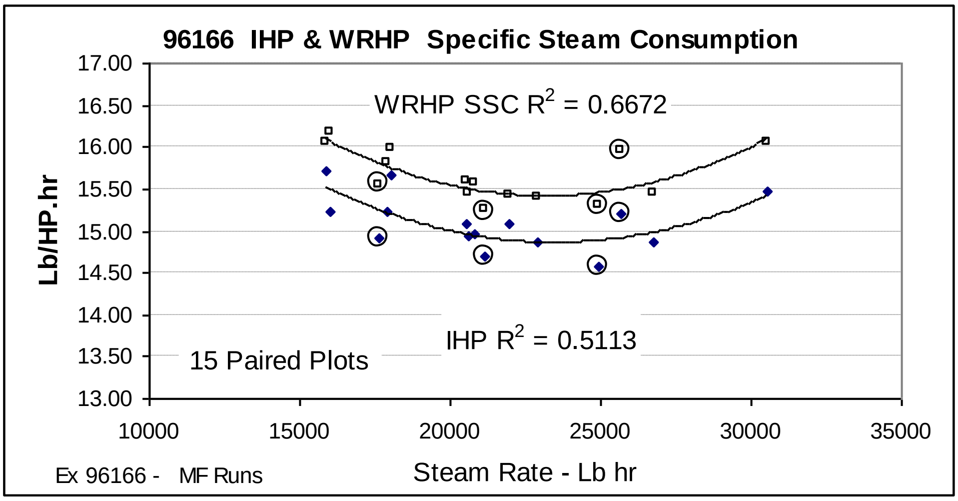

Figure 31a The master/slave relationship of the IHP./WRHP vertical paired coupling displacements are clearly in evidence here. I have ringed four pairings, and have likened this in the past to a dog following on a lead, with the slack or tension in the lead being analogous to the potential small remainder experimental error when determining the distance between man and dog.

John Knowles has disputed the existence of this relationship in his letter 12 July 2017 and elsewhere, Like it or not, WRHP is ever the child of IHP. Given the matching vertical shifts of the IHP-WRHP pairings shown here, it is apparent the IHP deviations from trend are in most cases are the outcome of real shifts rather than measurement errors. The usual 'elasticity' of small differences of course remains.

.

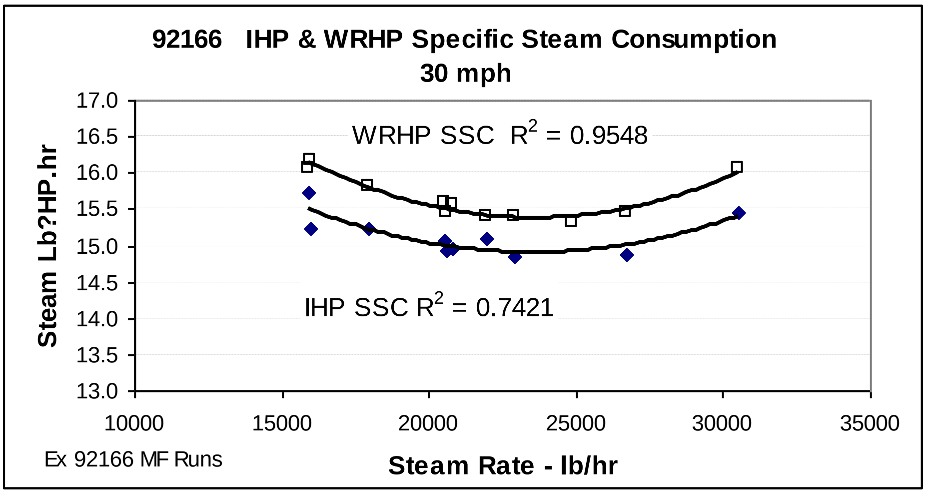

Figure 31b Removing the 4 vertically displaced pairings improves the SSC curves R^2 values;

The data set for 92166 includes 49 WRHP readings against steam rate. The associated Willans Line gives an R^2 value of 0.9946. Reducing the data set to 42 by removing randomly distributed plots not in contact with the trend line marginally increases R^2 to 0.9974. Another example that more data does not necessarily lead to more accurate outcomes. A poor plot or plots can occur at any point in the testing cycle. Simultaneous IHP and WRHP plots are limited to 15 for 92166, and as explained (page16), the positive remainder it returns for the WRTE v ITE formula is unsatisfactory. Such an outcome can only be eliminated by reducing the data set to 8 pairings, as determined by experiment. The revised outcomes, along with 92050 and 92250 are tabled below.

9F Modified* Collective Machinery Friction Outcomes @ 1600 IHP, 20.000 lb ITE - 30 mph					
Engine	Plots	R^2	Formula	20K ITE MF	20K MF HP
All	37	0.9984	y = 0.98530x - 449.48	743	59
92050	12	0.9993	y = 0.9879x - 508.17	750	60
92166	8	0.9994	y = 0.9765x – 275.8	746	60
92250	17	0.9974	y = 0.9865x - 476.3	746	60
Averages		0.9986	y = 0.9840x – 427.4	747	60

At 10,000 ITE, the MF outcomes average 588 lb, 47 HP, spread 40 – 50; at the highest output, ITE 24.000, MF averages 812 lb, 65 HP. Spread 64 – 67.

2. Sensitivity.

This is observable in the linkage of IHP - WRHP master-slave coupled plots. In the main, the IHP/WRHP scatter pattern pairings move in the same direction, up or down in elastic harness. It is that elasticity of small errors born of large numbers that generates the small remainder scatter. Outliers exceeding +/- 100% of the mean experimental value and the occasional negative outcomes may occur, as demonstrated in random number experiments,

While the above describes the responsiveness of the dynamometer to changes in drawbar pull, the collective sensitivity of WRTE v ITE data sets is very sensitive in regard to the tilt of the simple $Y = C_f x – R$ relationship as generated by the random scatter pattern of the data sets as exampled for the 9F in Figure 30 and the associated tabulations above. Since the trend line constant notionally represents the resistance of the of the power transmission machinery (including of course the coupled wheels) *when not under power*, some relationship of the contsant as a function of speed is to be expected. In practice the random scatter is often sufficient to frustrate clear outcomes in this regard. As demonstrated for 92166, the constant outcome was not even the right sign. Other examples can be found in the Rugby data generally. The hostage to scatter is heightened when the ITE – WRTE relationship only covers a limited range of steam rate and power. The tilt outcomes do not necessarily improve as a function of the plot numbers available, a trend wrecking plot or plots can occur at any point in a test series.

Some plots are obviously more accurate than others, and in some instances so wayward as to be beyond the definition of 'outliers'. In this situation, something has obviously gone wrong

3 Veracity**.**

This is something of a judgement call: does it all make sense? The determination of VRU, an idea of fundamental logic, has satisfied the theoretical outcome of returning constant values, and perhaps is the nearest thing to a "simple proof". Said VRUx values however must be considered close approximations at best. In reality, that caveat applies to the test bulletin data generally, whether it originates from Rugby/Derby or Swindon. It was sometimes more wanting from both camps. Understandably high cylinder efficiency will be welcome, but if accompanied by unusually high locomotive resistance should it be believed? The ultimate comparator of locomotive performance at a given steam rate and speed is the DBHP, but even that measure has sometimes proved unreliable due to assumed steam rate errors. This applies to both the Rugby/Derby and Swindon bulletins.

4 Uncertainty

Even if the test plant performed perfectly to the design specification in all respects throughout its operating life, the small remainder problem would not disappear. The delivery of empirical data that falls into place with the precision of a perfect jig-saw is inevitably beyond reach given the metrological limitations. While Chapelon opined that the Rugby data was the most accurate he had seen, this was against the notably chequered history of locomotive testing generally. I think Carling was right to be equally circumspect about the determination of both locomotive resistance and machinery friction. This he attributed as intrinsic to the small remainder problem. If anything, locomotive resistance is more problematical since it is determined in uncontrolled, and typically, unstable atmospheric conditions. One certainty is that WRTE will fall somewhere between ITE and DBTE, the problem is exactly where? It can tentatively be approximated by adding VRUe to DBHP where the latter is thought reliable. At best such estimates can only produce a plausible band within which the WTRE, and the MF thus implied, could fall.. Unfortunately most of the DBHP data in the Rugby/Derby derived test bulletins is wrong (Report L116). Report R13 for the Duchess is the only example where the DBHP data was fully reconciled with the Rugby IHP data (Report L109 and L109 Supplement). The available WRHP data for 46225 is only sufficient at 50 mph. The WRHP data for the BR7, BR5 and 9F is more comprehensive; but the DBHP data is deficient. The bulletin derived LR for the BR7 even appears to elude a 'no error' crossover point - Figure 27. Locomotive resistance determinations, given the small remainder problem can be no better than as for WRHP, and are additionally subject to climatic variation. At least WRHP, along with IHP and DBHP can be measured and scrutinised as a quantity; MF and LR and are forever a small remainders.

Addendum

First and foremost, the data base drawn upon must be credited to an XL spread-sheet put together by David Pawson in 2009, following an epic stint of research at the NRM. Comprising over 2,200 rows with up to 50 data entries per row chronicling boiler, cylinder and dynamometer performance, temperatures, pressures and gas analysis, it must comprise between 50 and 60,000 entries . It is a truly monumental piece of research. Additional to the Rugby data, there is some Swindon plant and road test data for 6001 and 71000. The Rugby data covers 10 locomotive types and 22 allowing for sub types. Additional to this, various reports and correspondence came to light.

As alluded to earlier in this correspondence, Dennis Carling is on record as thinking the determination of locomotive resistance and machinery friction as troublesome. Having been privy to what at first sight is a vast body test data, my impression is that putting together a test bulletin was not exactly easy either; it was inevitably something of a

black art. It was akin to working with a shoddily manufactured jig saw with a large number of missing pieces, both randomly distributed and whole missing sections. When the data is broken down for particular speeds, it is often sketchy or absent altogether. A significant amount of interpolation, extrapolation and tweaking will have been unavoidable.

"*When a sufficient number of values of indicated pull or power had been obtained over the necessary range of speeds and rates of steaming, the values of each speed were plotted to obtain the relevant Willans Line: these are compared to those of adjacent speeds and slight adjustments are made to obtain a regular family of curves fitting as nearly as possible to all the points. No two draughtsman will draw exactly the same curve through the points as to what fits best, and indeed, they may be influenced to some extent by the set of French curves available in the drawing office!" **

This may sound unscientific, but it is very much the practical reality, moreover, the XL curve fitting programme is not necessarily better at it, and can be notably poor at extrapolating much beyond the maximum and minimum recorded values. The randomness of the experimental data sets and the formula thus generated is nothing less than a lottery. Wide variations of coefficients and constants are evident as demonstrated. The most reliable first steps for analysis is plotting Willans Lines, steam rate against IHP. WRHP and DBHP, or ITE, WRTE and DBTE. The drawbar data is only available by scaling off the test bulletins. Steam rate, particularly when working with the live steam injector, was thought the most accurate determination of the Rugby test data, with experimental error "*probably well under 1%"* *

*"Amsler of Switzerland, guaranteed an accuracy of 1% of the scale (dynamometer pull) used, and $1^1/_2$% for the work done. ** "A calibrating device, itself checked at the National Physical Laboratory, showed this value was in fact substantially improved upon, tending to fall from close to 1% at quarter scale to 0.75-0.5% at three quarters scale, in which range most of the work would be done."* See page 91 for an NPL test record.

While IHP and WRHP Willans lines at particular speeds uniformly returned R^2 values approaching unity (not in itself is not proof of veracity), they do not

extrapolate reliably much beyond the minimum and maximum plotted values, and are influenced by the particular random scatter pattern obtaining in a data set. Plots of WRHP v IHP or
WRTE v ITE provide a direct relationship where scatter is typically low as a percentage of the quantities measured, but as already demonstrate , the linear trend lines are sensitive to the scatter in regard to 'tilt'. Some of the data base steam rates are unclear in regard to the use or otherwise of the exhaust steam injector. These uncertainties can be sometimes be resolved by examining specific evaporation rates (if coal rates available) and the steam rate v cut-off relationship. Adjustments can then be made accordingly where necessary.

Below, demonstrating the sensitivity to scatter, 3 doctored outcomes of an 8 plot MF data set, as derived from WRTE v ITE for 92050 at 40 mph when a single WRTE plot is removed. Note the varied outcome of the constant. The MF outcome at a steam rate of 20,000 lb/hr, roughly midway of the range examined; ranges from 608 to 671 lb: +/- 5% of mean. The range of uncertainty, maximum v minimum, is +/- 0.46% of ITE

92050 WRTE v ITE MF Plot Variation Outcomes 40 mph				
Plots	R^2	Formula ITE v WRTE	20K MF*****	MF Index
8	0.988	Y = 0.9889x-465.5	618	98
- Minimum	0.9972	Y = 0.9798x-330.3	608	97
- Maximum	0.9974	Y = 0.9679x-229.68	671	107
- Middle	0.998	Y = 0.9892x-463.3	612	98
* Q - Willans IHP 1465		Average	627	100

----

* Dennis Carling: *An Outline of Locomotive Testing on British Railways*, * Model Engineer, 7 November 1980. Page1331. ** Ibid 17 October 1980, Page 1253.

Work done was the basis for calculating the WRHP, and for the most part it probably achieved the +/-1.5% standard. At 15 HP per 1000, up to 1.5% seems to be a realistic assessment regarding the range of uncertainty that accompanies the Willans lines. There are occasional plots where this standard of accuracy was obviously not achieved. The scatter problem is further complicated beyond experimental error in that some of the scatter is real, given the small variations in steam chest pressure and superheat. The Willans lines for IHP & WRHP routinely deliver R^2 values approaching unity, which accords with low measurement deviations from trend in percentage terms. When the difference between theses two large numbers is examined, the MF, then the data set R^2 values approach zero due to compounded error; the randomised "high" or "low" bias of speed related data sets relative to the overall trend of all the MF data independent of speed are frequently in evidence. Random number experiments have shown that such MF data set biases may not imply a real shift in measurement accuracy since exactly the same ITE & WRTE values are always entered. The resulting experimental outcomes showing clear "off-trend" bias are entirely the result of random variation within the set measurement accuracy parameters. High R^2 squared values are not axiomatically an indication of accuracy. Consistent error would also score high.

The limited scope of the experimental data, routinely fails to cover the full range of power and speed portrayed in the test bulletins. The published data for the lowest and highest working rates is evidently often based on extrapolations, and as such is sensitive to the French Curve syndrome described by Carling. As explained above, extrapolations using the XL curve fitting formulae cannot be relied upon either. This problem was apparent when looking at the VRUx determinations, when it was found constant values did not obtain over the full working range, though they did for the bulk of it. The outcome for BR5 73008 in Figure 29 for example; covered a range of 12,000 to 24,000 lb/hr as against 8,000 to a little over 26,000 in the test bulletin. This degree of cover, around 70% of the working range, was typical.

Finally, returning to the constant steam rate deviations encountered on the Derby road tests, it should not be thought the Swindon road tests were immune from this problem. The locomotive resistances evident from the Swindon derived test bulletins, though at least satisfactory in regard to the general shape of the LR curves, are far from anomaly free. Below the LR curves as derived from Test Bulletins Nos. 3 & 4.

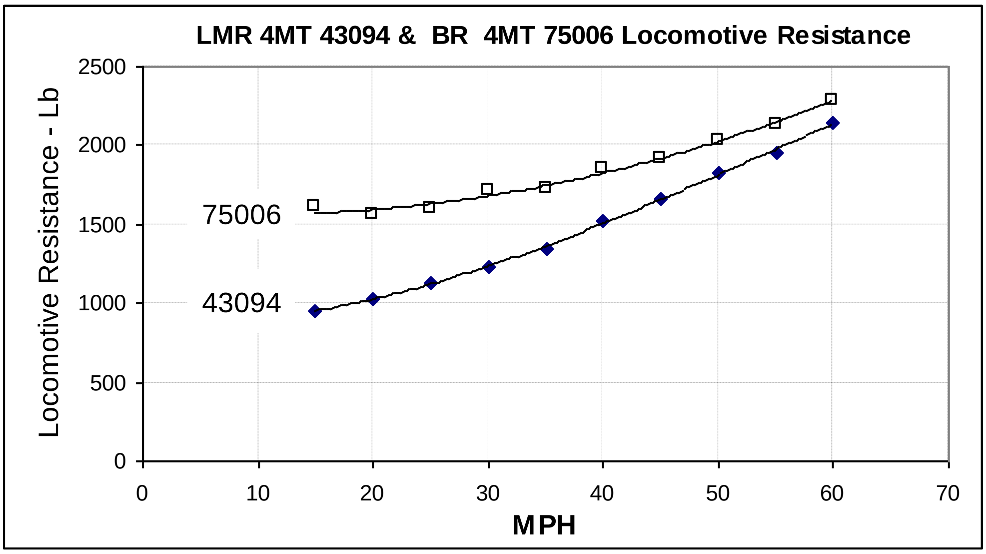

Figure 32 Note the marked LR separation at low speed

The LM4 weighs in at 99.4 tons and the BR4 at 110.05 tons. At 20 mph the respective resistances are 55 and 83 HP, a difference of 50%.

The Swindon test team had the advantage of a test route featuring fewer and less severe gradient changes, enabling longer periods of relatively steady pace. This will likely have simplified controlling the steam rate, though nevertheless, the diversity in LR outcomes as shown above, and in other cases, was at least in part, contributed to by steam rate uncertainties.

On the evidence of the Swindon road test data for 75006 and 71000, significant steam rate deviation tended to occur at the lower end of the speed range when speed was changing more rapidly, acceleration forces, and steam rate increments potentially rising quickly.

The mean steam rate of 23 spot readings based on speed and cut-off for 75006 works out at 15,214 lb.* This is not representative of the overall average for the test, since it is based on instantaneous values rather than a summation of all 48 cut-off changes of varying duration shown in a series of steps, and the associated speed changes. The overall test average was probably closer to the nominal rate. The point that emerges here is that significant departures from the nominal test steam rate could pass undetected; the summation of increments procedure with a metered water supply notwithstanding. Unseen short term boiler water level changes and shifting gradients and inertia effects provided a cushion of uncertainty. From MPs 103 -106, for example, on a constant gradient, cut-off is *shown* held at 24% for approximately 2.8 minutes as speed rose from 60 to 68 mph. Steam rate will have increased about 12% over this section. The bulletin of course, working with the visible metering summations, showed only minimal drifts from the nominal steam rate at any point, as published in the bulletin.

It was perhaps inevitable that cut-off adjustment of steam rate and the available instrumentation had its limitations as a means of controlling Q. The increasing heat drop and reducing exhaust steam specific volume with rising speed and cylinder efficiency for given steam rates was challenging on road tests, even when the density effect was understood. It maybe, the cut-off changes were more gradual than shown. This pretty well concludes my investigations for now, at least I think it can be agreed that the determination of locomotives resistance and machinery friction was no easy matter, or for that matter, the production of test bulletins more generally.

John Knowles Submissions 4 July 2017 and 2 April 2018

As previously, points raised will not necessarily be taken in chronological order, words in quotation marks and emboldened for clarity are his own. The underlined subheadings
are mine. Quotations by others are in italics. There may be some repetition here and there involving points raised above or in the earlier correspondence. This occurs because the same points keep re-emerging, often in mutated form, calling for further comment.

Some General Points.

"Doug seems to believe the data are sacrosanct, apparently perfect, or if not perfect (a real world situation?) they are as good as can be obtained in the real world, and are not to be questioned."

This is far from the case, contradicting my many writings on the subject down the years, of which he is aware. Were it so, I would not have spent years tying to make sense of locomotive experimental test data generally and the Rugby and Swindon record in particular. I have posed many questions and identified numerous anomalies over the years and extensive correspondence since 1970 testify. Even within the contractual measurement limits, the randomised scatter in the small remainder situation is fundamentally problematical. Some disparity is a statistical inevitability. Obviously a satisfactory standard within the understood limitations was not always achieved, some highly aberrant outcomes affecting various aspects of the data is evident; systems can malfunction. A key point here is 'measurements' as opposed to the lottery of small remainders. On a direct measurement basis the WRHP data (Willans Lines) returns higher consistency over time than the IHP data in the early years. Overall, the latter was more erratic in this regard (higher scatter- lower R^2) and inconsistent with later outcomes. More on this below.

- Test Bulletin No.4. Road Test No.1   14,200 Lb/hr steam rate.. Cut-offs shown as a series of steps. Steam rates calculated from steam Rate v cut-off and speed – Figure 15.

My very first writings on this topic in 1970 began:*

"The steam locomotive is not an animal the test engineer would fondly regard, for as the discrepancies in the BR Test Bulletins bear witness, it does not readily give an accurate result. And later -*.These results (LRs) can thus be taken to show constant losses. We thus have nine sets of results, seven of which suggest that locomotive resistance at any given speed is a constant independent of power output, and this has been taken to be the case. In stating the above however, it should be noted that this runs contrary to engineering experience and logic, and some rise in losses with effort should occur."*

"Doug uses Carling's belief that because the ITE results for the same test circumstances fall in a narrow band, the ITE data are acceptable, even accurate."

I don't know where this idea comes from. On the contrary, the opposite is true of IHP and ITE over the history of the plant. Perhaps he meant to say WRTE. The performance of Farnboro'' indicator took some years to reach a satisfactory level of performance and was not free from some setbacks along the way. It is the WRTE Willans lines that I have generally found consistent for different test series of the same locomotive type. In contrast to the claim of "consistent" IHP data early in this correspondence, it is often poor. This emerges most clearly when the IHP data is examined in the basis of specific steam consumption. The outcomes often verge on the erratic, with evident 'strays' and poor R sq'd values.

It took some years for the indicating equipment and process to reach a satisfactory standard of performance, and progress was not without some setbacks along the way.
Even then, the occasional episode of wayward performance was not unknown in later years such occurred as late as 1959 with 92250 in Giesel ejector guise. The IHP SSC data for 50 mph produced a medley of strays: Figure 34.

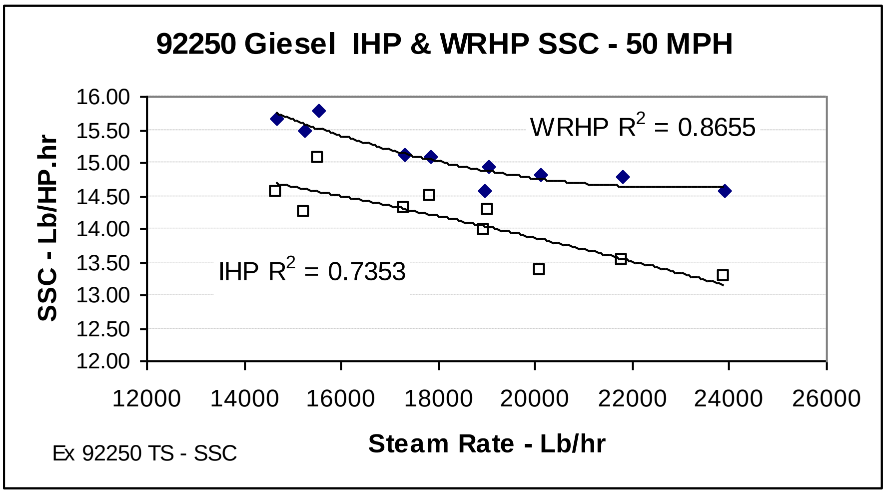

Figure 34 Most of the IHP 'strays' from trend evident here are likely of spurious value since for the most part, the corresponding WRHP plots remain un-persuaded and stick close to trend. The IHP's slightly convex IHP trend line is the wrong shape.

Indicator Calibration Tests

There were three episodes of comparative indicator tests. The first series compared the Rugby Farnboro' in dicator with Maihak and Dobbie mechanical indicators supplied and operated by visiting Swindon engineers in January 1953, The Rugby v Derby Farnbro indicators were matched later that year, and again in March/April 1957. Only this last test series achieved, for the most part, close agreement, with average results within +/- 0.5%.

………………………………………………………………………………………………

* Test Result Anomalies – An Interim Study; D. H. Landau; Stephenson Locomotive Society Journal, December 1970.

Initially the 1953 mechanical indicator MEPs were up to 10% higher than the Rugby Farnboro' outcomes. Subsequent calibration checks reduced the discrepancies to +2% for the Maihak, with the D & M still 7% high at low steam rates, then falling to about ½% at 23,300 lb/hr. On this showing the D & M indicator was an unsatisfactory piece of kit. The Maihak indicator re-calibrated results were consistently 2% higher than the Farnboro'. The differences here perhaps represent a margin of uncertainty.

The intermediate 1953 tests deemed the Derby Farnboro' to be indicator erratic, with mixed results overall. The Derby variance with Rugby was up to +13% - 3.4%. Full data sets are available for Rugby tests 872 to 882 immediately preceding these tests. Each test involved averaging up to 10 indicator diagrams. Maximum scatter was +/- 2.9%, averaging +/- 1.5%. Speeds covered 30, 50 and 70 mph. The final Rugby/Derby Farnboro' indicator results were as tabled for the 92050 Series 2 tests - page 24.

"**It would be wrong to regress DP against Q. Q has already influenced ITE, at a rate varying with Q per se and V, and as seen in the Specific Steam Consumption**."

This objection is without any rational basis. The relationship rejected is as would be derived from WRHP Willans lines. It removes the obvious way to compare WRHP outcomes of other test series with the same type at given speeds. Steam rate (Q), is the most accurate baseline of available from the Rugby data, (perhaps not quite so secure when the exhaust injector was (rarely) in use). The WRHP relationship with Q is unaffected by whatever the IHP *measurements* turn out to be. The determination of WRHP is an independent function. There were several episodes where cylinder indicating was omitted and the measurement of WRHP continued. Presumably the indicating equipment was undergoing repair or modification. The WRHP Willans lines were then the adopted basis of comparison, as for example the 92015 regulator experiments. The plotting of WRTE against ITE gives a direct measure of mechanical efficiency. Such plots for given speed sets have established one of the few certainties to emerge from within the Rugby data: WRTE v ITE at a given speed is a linear relationship.

"**Doug should not be concerned about a proper regression line (rather than an EXCEL trend line) 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**." (JK letter 25 October 2016)

"A best fit not passing through the actual data" sounds like a mathematical aberration rather than a revelation of a supposed statistical reality. Something akin to walking on water or flotation without getting wet. It is absurd. A good example emerges in his letter 4 July 19 2017 (page 37) where he cites a Graph that I gave him some years ago that has not appeared in this correspondence – Figure 35a.

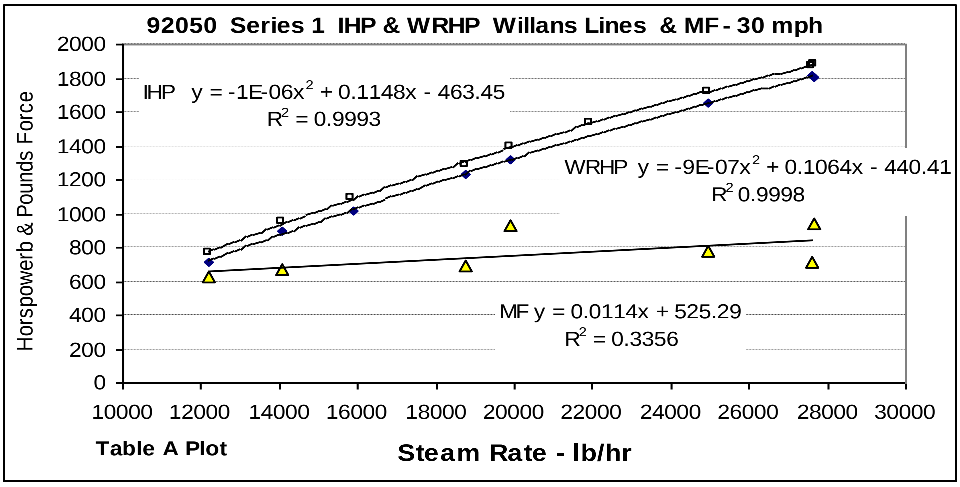

Figure 35a The 9F returns a positive ITE – WRTE separation. The MF values average 763 lb, the smoothed outcome ranges from 706 to 840 lb.

He comments;

"This exercise was supposed to show that TSR was constant at 30 mph (like a dog following its master on a lead he claimed – see *Backtrack*, April 2014, p 253). It does the exact opposite. It shows TSR supposedly varying with Q, but not as fast, and at a declining rate, to high levels."

It was most certainly not originally presented to show "constant TSR", from a long correspondence John should know that is not a view I hold. What he actually said at the time was that seven plots was too few, rendering the positive MF outcomes worthless

John goes on to calculate the smoothed MF outcomes derived from the formulae shown in Figure 35a. While this exercise is mathematically correct, the outcome from the smoothed results significantly raises the MF from an average of 763 to 1270 lb. A comparison of the "before and after" IHP and WRHP Willans Line proved revealing as Figures 35b & 35c below.

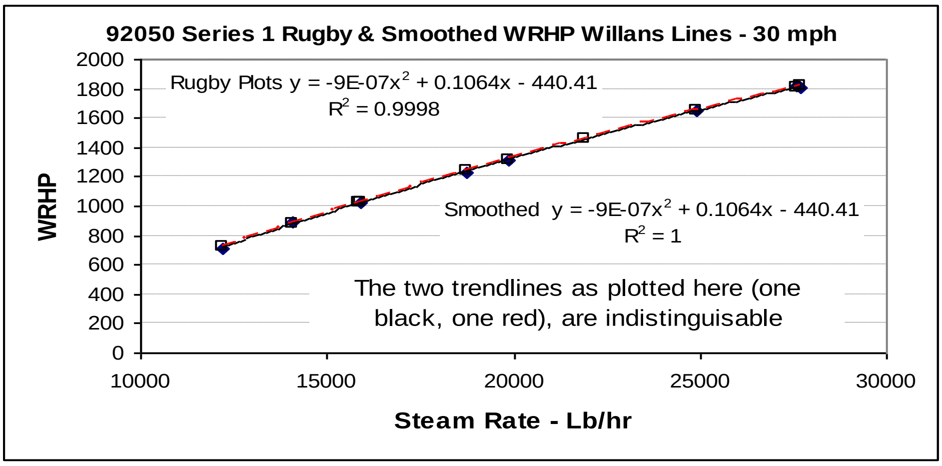

Figure 35b There was little adjustment to the Rugby WRHP plots. They fell within 0.6% to – 1.7% of the smoothed values; the average deviation was 0.7%.

The smoothed IHP plot, Figure 35c is unsatisfactory, inflating the IHP outcomes.

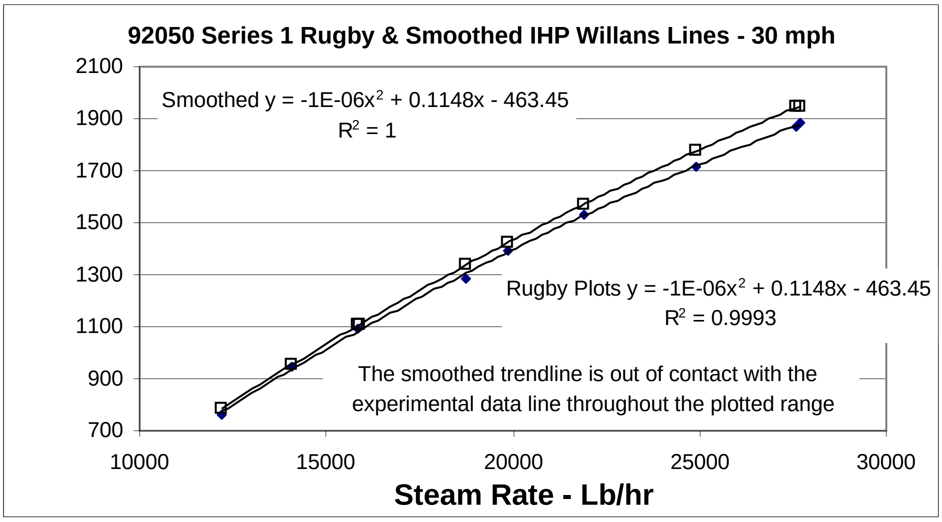

Figure 35c The upper "smoothed" IHP trend line makes no contact at any point

with the Rugby plotted data. This is clearly a mathematical aberration, hence the erroneous uplifting of the MF outcomes in which the smoothing of the WRHP trend line plays no part.

The smoothed IHP values are clearly an aberration and are seriously in error. The answer has proved quite simple; the XL curve fitting programme *defaults to four decimal places.* An override option increasing the decimal places is available: RH click on the trend line equation, and then choose ‘Format Trend line label’, select ‘number’ then choose ‘decimal places’. In this instance 9 was selected, the aberration disappeared, refer Figure 35d.

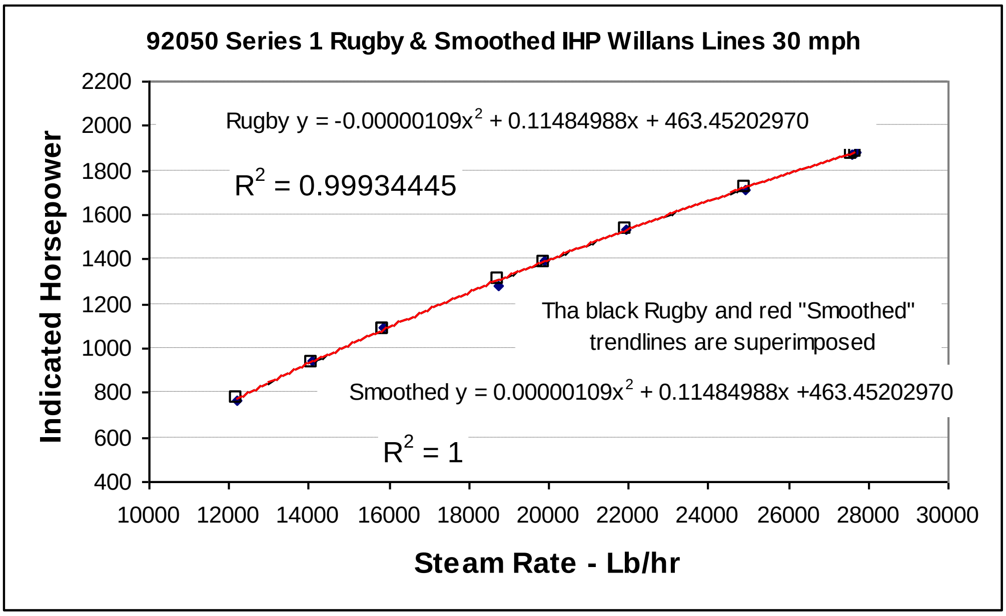

Figure 35d The enhanced decimal place formula and Rugby trend lines are indistinguishable. The average “smoothed” IHP correction was 0.1%

“The Rugby indicator results are highly consistent for a given engine when regressed against Q and V.” “In addition he calls on repeatability as a criterion for acceptability or accuracy of data, when all the repeated data can all be wrong.”

We don’t disagree on this basic point. While repeatability is a prerequisite, it not in itself an axiomatic proof of accuracy, as I have written elsewhere. The same limitations apply to high R^2 values as also pointed out, obviously fixed calibration or systematic errors might be in play. I note that early in this correspondence John was content to cite the indicated horsepower data as “consistent” in an attempt to infer WRHP data shortcomings implied by negative MF outcomes fell entirely on to the shoulders of the Amsler Dynamometer. This supposed “consistency” was inaccurate; the said data appears to have been taken on trust without due scrutiny. The chequered history of indicator development described in the Ron Pocklington correspondence receives no mention. The recorded IHP for the BR7 increased with time, as I have shown. Indicator performance was not deemed satisfactory from both the reliability and diagram quality standpoints until early 1955. The differences between the 92050 test Series I & 2 IHP results were overlooked. (The difference in this case proved to be steam leakage, not IHP measurement,)

“Only late in the testing was it discovered by simple consideration of the data, that for LR in this case, that such was not correct.”

The Rugby/Derby test staff certainly seem to have been slow to take action; this was likely down to the test plant work-load, but they could easily have re-introduced indicating for the road tests at an earlier stage. However, contrary to the above assertion, Report L116 indicates the LR problem was recognised early on, as indicated in its opening sentence: “*In all cases where locomotive trials at Rugby have been followed by road tests carried out with the LMR Mobile Test Plant there has been a lack of reconciliation of the results to the extent that values of locomotive resistance obtained by subtracting road T.E. from Rugby cylinder T.E. have not been acceptable.”*

It later continued: *“It was first observed with the E.R. B.1 Class 4-6-0 Engine No. 61353 during the course of a day’s running from Carlisle to Skipton and return, the steam rate produced by a particular setting of the blast pipe pressure during the outward run could not be accurately be repeated on the return. The only difference of any significance between the two test runs was that the overall average speed was lower on the return, owing to the nature of the test route.”* The road tests were in 1951.

“Perform”

“…the Perform program gives results a little higher than those from Rugby. Perform is by far the best way of approximating cylinder outputs.”

This is an optimistic view of the Perform programme. For those unfamiliar with the late Professor Hall’s “Perform” programme, herewith some brief notes. Hall, a nuclear power engineer, did some ground breaking research using a live steam model, demonstrating that even with superheat, under some circumstances condensation could occur in the course of a power cycle. In summary he developed a programme embracing the many complexities of thermodynamics, fluid dynamics, valve events and the various dimensionless coefficients involved to compute IHP. He then compared his theoretical results against the published data.

He was not privy to the actual experimental Rugby and Swindon test data that has later become available. His matrixes for comparison were confined to the data available in the *Britannia* Test Bulletin (N0.5) and S Ell’s 1953 I.Loc.E paper *Developments in Locomotive Testing*; essentially a test report for high superheat *King* 6001.

Hall was unaware of the notoriety that surrounded the test data for 6001, distinguished by high LR with a distinctly high sensitivity to the level of effort, when he commented ; *‘However it has been possible to infer enough information for a start (comparison) to be made using an excellent paper by Ell which describes controlled road tests made in 1953 on the former G.W.R 4-6-0 4-cylinder “King” class locomotive No. 6001’.*

As things turned out the computed results for IHP v speed at constant cut-off traced a similar parallel path to the report data but were over 10% higher at 40 and 50% cut-off. Hall was unaware of the disparate outside/inside cylinder performance of the King; the inside delivering only around 70% HP relative to the outside, and the high pressure drop from boiler to steam chest; about 10PSIG more than a Duchess at the same steam rate, and more still compared to the Scot. Had Hall had access to this data he would likely have been less encouraged. The IHP Willans Line R^2 returns for 6001 covering 14 road tests were mediocre, averaging 0.7933; the range 0.6451 to 0.9002.

The later comparison by Hall for the *Britannia* was generally close to the bulletin values at given speeds and cut-offs. There was however some difference in regard to the actual steam rate at 15% cut-off, and to a lesser extent at 25% up to 40 mph. Hall also converted a few bulletin indicator diagrams in radial form to the conventional stroke base, with an overall trend for the computed admission PSIG values to be a little higher than the actual. Of the indicator diagram conversion for 25% cut-off at 40 mph, Hall concludes that the *'result appears to somewhat out of line with the others, and leads me to wonder whether the location of top dead centre has been correctly defined on the indicator record'*. Shades here of Ron Pocklington's concerns when he forst arrive at Rugby in 1952.

David Pawson, is an expert in using 'Perform'. His recent (MP 38) *How Powerful are UK Steam Locomotives?,* with its *Perform* computed IHP results are tabled below.

Perform Power & Steam Rate Estimates at 25% Cut-Off, 60 mph v Test Bulletin record

Loco	Perform Estimate		Test Record			Perform indices v Test Record	
	Q Lb/hr	IHP	Source	Q Lb/hr	IHP	Q lb/hr	IHP
Duchess	33,600	2440	R13	31,500	2195	107	111
Reb Scot	23,400	1720	Rugby	27,930	1945	84	88
BR5	18,700	1410	Bulletin 2	17,750	1230	105	115
BR7	22,000	1740	Bulletin 5	21,500	1610	102	108
BR9	23,600	1880	Bulletin 13	24,500	1770	96	106
V2	20,300	1610	Bulletin 8	24,180	1665	84	97
King 4RS	28,700	1730	S O Ell	27,800	1910	103	91
Mod Hall	17,500	1230	Bulletin 1	24,250	1630	72	75

The test record data shown is as interpolated by measurement.

It is apparent, that Perform is unable to replicate the test record steam rates at a given cut-off with both under and over estimates returned. The test plant derived water rates are the most accurate data available, Carling reckoned steam rate experimental error to be "well under 1%". Given the nuances of valve setting, cut-off introduces some uncertainty, but the deviations from nominal values inherent from the crank angularity effect tend to cancel out front and back, and seem insufficient to explain the differences tabled. The valve settings were checked by the Rugby test staff and the practice at Swindon was probably the same. In exception, quite what the true cut-offs were for the V2 middle cylinder is difficult to determine from the bulletin indicator diagrams. That the *Perform* estimated steam rates fall both above and below the test plant values suggests that uniform assumptions for steam port friction coefficients and other design details affecting steam flow are more nuanced than supposed. The measured test plant IHP data is also of course subject to uncertainty, notably the early Rugby data and the Swindon data generally. Had life given Bill Hall more time, and he'd had more access to the experimental record, his ground breaking work may well have acquired a few more tweaks.

.At an estimated 5% accuracy, Perform may well have outperformed many mechanical indicators, but with uncertainty up to 50 HP per 1000; it would play havoc in small remainder situations.

All of the above on the *Perform* programme is a bit of a diversion, and not really relevant to the discussion in hand; but John Knowles having referred to it, it seemed an outline of would be helpful to those unfamiliar with Hall's work.

"It is therefore extraordinary that Doug Landau, after all these years, claims to be able to judge the Rugby data better than Carling, and to want to do so without explaining how. That is the same as setting his face against regression results – nothing declaring against the Rugby regression results, specially by me, is to be tolerated I suspect too, that he believes that scatter is evenly distributed and that the true answer lies in some sort of average of all the data. I fear not. The testing and consideration of the data requires consideration of the scatter, its extent and an examination for biases. Simply declaring that the Rugby data are fit for providing TSR values avoids crucial steps in showing that it is fit. Declarations are empty if the steps have not been taken. Doug Landau has never shown that he has considered the data, so it follows his declarations are empty. "

These imaginative assertions are travesty of my thinking and methodology. *Pure rubbish* would be a fair description. Not content with putting words and thoughts into the mouths of the dead, he now seeks to do the same for the living; desperate stuff. I have not challenged the powers of regression. What is being challenged is flawed thinking and misapplication, reducing the exercise to the status of reading tea leaves. What I am supposed to explain? Essentially, all I have done is present the recorded test data in clear unequivocal form. What could be more straightforward for example, than the linear WRTE v ITE relationship?; a simple representation of the recorded data; likewise Willans lines. In that form scatter is generally of low magnitude as a percentage of the values directly measured. Estimates have been avoided as far as possible, are few in number, and when deployed, their basis is explained and open for challenge if thought at fault. If my experiments removing one or two plots from data sets is deemed 'playing with the data' so be it; I am simply doing so to demonstrate the random uncertainties and sensitivities of the data sets exampled. Some plots are inevitably more accurate than others

"I suspect too, that he believes that scatter is evenly distributed and that the true answer lies in some sort of average of all the data."

Why would anyone think anything so silly? My randomised small remainder experiments show the complete opposite.

The actuality is that the scatter falls into two camps. Though random, scatter is small when referenced to direct relationships such as ITE and WRTE Willans Lines or WRTE v ITE, where the scatter generally falls within the understood metrological limitations. The second category is the chaotic statistical joint venture of small remainders where scatter can readily exceed +/-100% and random clusters of bias and the occasional negative outcome may occur.

As to **"doing better than Carling**", I agree with Carling that it was not possible to determine internal friction *within fine limits free of some uncertainty*. He attributed this to the small remainder problem and thought the same in regard to locomotive resistance notwithstanding a larger remainder. In regard to the direct measurement of WRHP, he said *"We got the results right".*

The advantages I have had over Carling is considerably more time, a comprehensive overview of perhaps 80% or so of the Rugby test programmes data, and the time saving powers of the Excel programme when it comes to plotting graphs, fitting trend lines and calculations. A considerable degree of the mental labouring aspect is eliminated. That is not to say that Excel is free of limitations and potential pitfalls.

I feel compelled to repeat and elaborate: the last thing I think is that "scatter is evenly distributed". Indeed the random distribution of speed specific data set groupings on occasion show clear signs of positive or negative bias relative to the overall trend for locomotives data sets. The idea that more plots axiomatically deliver sounder outcomes is not bourn out by examination. The last plot in a data set may well be a wild card disturbing what would otherwise have been a plausible relationship. "Unbalanced" outliers may occur. The best way to minimise this sensitivity is to plot WRTE against ITE. This relationship follows a straight line law in the form WRTE = Ax – B, where x is a coefficient sensitive to A, the ITE, and B a negative constant notionally representing the resistance of the power transmission machinery including the coupled wheels when coasting without any application of power. Such outcomes should deliver a negative constant. In other words as long as the locomotive is moving the power transmission machinery including the coupled wheels will encounter some machinery friction with steam shut off. Compression effects in the cylinders when coasting may of course add to the friction losses, but theoretically this should not effect the constant as derived under power. Some experimental error, will however be attached to said constant, given the sensitivity of the linear trend line tilt sensitivity to the distribution of the scatter.

The 'constant' outcomes as tabled for four 9Fs on pages 124-125 above examples these uncertainties. As things turn out, the constant may sometimes be falsely positive as cited for 92166; an unequivocal example of random scatter mischief..

A reproduction of my chart plotting the recorded MF data for Jubilee 45722 is criticised as below.

"These trendlines are not regressions. As immediately above, there is no discipline to them – Doug Landau has used them here to obtain relationships which do not exist in physics or mechanics. They can be done without any of the tests possible with regressions." (Reference to 45722 chart of Machinery Friction v Speed – plotted Rugby data.)

The chart is simply a plot of the recorded test data using the Excel curve fitting programme.. Contrary to his assertion that the relationships shown "do not exist in physics or mechanics", there are very sound theoretical reasons why the MF v speed relationship *may* take the dished form as represented by the trend line. At low speed the traction force piston thrusts are at their highest, initially falling rapidly with speed; in parallel, the rotational and sliding friction is increasing as a function of speed, and
the dynamic forces are increasing as a square of the speed. In such circumstances a dished MF trend line is entirely possible from the theoretical standpoint.

I accept that the outcome shown for 45722 might equally be simply down to the randomised bias of error within the scatter pattern of the overall data set . In contrast some of the Rugby data sets seem to flat line across the speed range. Such outcomes, based on the small remainder data could equally be the product of randomisation. The iterations of force, friction, dynamics and inertia within the span of each revolution are complex. While the shape of the MF v speed relationship may remain an open question, a flat-lining outcome is theoretically difficult, but cannot be ruled out. It is no wonder Dennis Carling thought the determination of MF (and likewise LR) to be problematical. A situation he attributed to the small remainder problem.

"Doug Landau appears to be unaware of the convention applying to the term static axle or bearing load. He thinks it means without the wheels turning. It applies to both circumstances. There are plenty of examples of the term static in the sense in which I have used it – see for example the paper by Cox on locomotive axleboxes, which he quoted, with the flavour that Cox's paper proves I am wrong in some way. If this still offends him, he can ignore the word static."

I don't know where John gets this idea of my objection to 'static' load comes from, I think nothing of the kind. Obviously the 'static' load is a constant that never goes away, whether stationary or in motion. In motion, dynamic effects, track behaviour, and imperfect balance will augment said vertical load both positively and negatively within the course of a revolution. In citing Cox's diagram of the forces acting on the coupled wheel axleboxes when under power, I was making a point he seems unable to understand. (my letter 7th March 2017). (He has also not revealed the **"other analysts"** that, apparently, do not consider the resultant (journal) loading part of. MR.) The point being that the sum of piston thrusts, dynamic forces and the vertical (static) load on the coupled axlebox journals is *less* than the mathematical sum of these forces. In other words, there is a degree of opposing forces and vectors cancelling out. It is a shared mitigation.

His idea of dismissing coupled journal friction and V squared losses as part of MF, in order to determine the notional values of 'Pure Machinery Friction

(PMF), overlooks this mitigation. (His 9F statistical analyses pages 45 - 48). Deducting a questionable friction estimate for the coupled wheel journals when notionally behaving as a passive unpowered vehicle, in order to discover the delusion of PMF, is an exercise without any conceivably useful purpose. This corruption of the measured evidence by interference is further compromised by subtracting a doubtless dodgy estimate of losses attributable to dynamic effects. The PMF idea as an analytical approach can only be described as utterly clueless. Whose "playing with the data" now.

I put his idea to Adrian Tester, he replied: "*As you correctly point out, WRTE and the pull recorded on the Amsler dynamometer were one and the same. Also, WRTE has to be net of all the machine friction inherent in driving the locomotive. Axlebox friction forms part of MR. it does not appear in WRTE; it represents part of the difference between indicated power and WRHP. It cannot somehow escape to be part of the WRTE, only to be absorbed later. I don't see the logic of that.*"

Note also, that this exercise creates a smaller remainder to be tested against the previously existing levels of scatter and uncertainty. Such an exercise is implicitly inferring that the recorded IHP data was perfect - blameless. The dynamometer was entirely at fault; strident confirmation bias on the march.

Such unnecessary meddling is wholly avoidable by simply treating the coupled wheels as part of the power transmission system which is exactly how they function. That is what is actually measured, it constitutes the overall *mechanical efficiency*; as referred to as such by the Rugby test staff. It's interesting to imagine, how, in the absence of any adhesion weight, power would be transmitted. The statistical analyses on pages 45 – 46 of his letter 7 July 2017 are worthless: Pure guff.

The estimates of journal resistance (CWBR) are, according to his earlier citation, based on a misunderstanding of what Ell's paper on rolling stock resistances reveals. The paper was of much interest since it concerned 520 ton freight trains of varying length and vehicle type in both the empty and loaded condition. The related resistance formulae fitting the data in all eleven cases took the form $R = A + BV + V^2/C$ lb/ton. While it is true that notional frictional rolling resistance relationships as a function of axlebox loadings were determined from the constant A term values fitted to the experimental data, they cannot be construed as actually representing the journal friction *across the speed range*. It has been shown (DHL R13 Audit) that the individual values of the ABC resistance formulae will accommodate some permutation of the coefficient values while still delivering a satisfactory fit to a given curve. In other words any seemingly causal relationships of A, B & C are tenuous.

Even the simplified form, $R = A + V^2/B$ can sometimes do the job. In summary, the three elements of the classic resistance formula, may at best only approximate to some causal functional relationship; obviously the squared function will have lot to do with aerodynamic drag, but close representations of the causal realities cannot be assumed. This does not matter of course if overall, the curve fitted is considered sound and the formula fits the purpose of estimating *total* vehicle resistance.

It is another matter when trying to determine the true journal friction across the speed range; it is not a constant. JK's Tables 1 to 4 for 92250 data for erroneously show constant a CWBR value of 228 lb across the speed range; given established bearing theory, this is wrong.

Coefficient of friction $\mu = ZN/P$ where Z = Viscosity, N = RPM, P = Bearing pressure

It is apparent that μ is a function of speed and an inverse function of P. The rising ZN/P relationship only obtains for values upwards of around 25 once *hydrodynamic lubrication* has been established. Starting from rest the *boundary film lubrication* zone (otherwise known as stiction) is encountered, then falling rapidly, the intermediate *mixed film lubrication* zone being reached from about ZN/P = 5, then falling to a minimum on reaching the hydrodynamic state; from this point μ increases with speed.
These uncertainties are of course wholly avoidable; PMF = Pure Mechanical Fallacy..

"He gives no reference for the claimed confirmation of TSR by road tests for the Crosti and standard 9Fs, nor explained how he reconciled what are essentially different measurements – TSR given on the test plant and LR on the road. Given the lack of repeatability in the Rugby data, he does not say which 9F data among the non-repeating 9F data he picked for his own use as the resistance of the 9Fs".

A curious statement; obviously, if two locomotives of similar vehicle architecture, size weight and shape display significantly disparate machinery friction, the locomotive resistances will be inevitably be similarly disparate. The Crosti 9F 92023 returned notably higher MF on the plant than 92050 and all the other 9Fs tested. The confirming details of these road tests were covered on page 117 above. The other 9Fs were consistent in regard to MF when examined as WTRE v ITE.

The more significant comment here is **"which 9F data among the non-repeating 9F data he picked for his own use as the resistance of the 9Fs".**

This supposed "non repeatability" is based on small remainder data sets, when such disparate outcomes are highly probable. It's about as meaningful as comparing the results on a Bingo night. His claim of **"non-repeating"** is thus entirely erroneous.

His statistical analysis of truncated small remainder data (PMFs) for 92250, vide Figures 1 to 4 for 92250, pages 43-44 is flawed at every level from conceptual to execution. Small remainder outcomes are the results of joint enterprise, not direct

measurements, and are no place to start with statistical dissection in the first place. Other, more direct relationships are available. His first step is to corrupt what is already inherently troublesome data (SRMs) by deducting highly questionable and wholly avoidable estimates. Thus the SRM gets even smaller whilst retaining the same degree of scatter. All apparently a the outcome of single handed of dynamometer malfunction. This approach can justly be deemed clueless. The scatter originally displayed, generally falls within the SRM potential scatter given the known accuracy limitations of the indicating gear and dynamometer, as has been demonstrated by the randomised small reminder experiments.

Notwithstanding that the same notionally perfect measurements were entered on dozens of test runs, it was all too apparent that individual speed data sets and whole data sets could sometimes display upward and downward bias relative to the perfect fixed remainder value entered. Off target clusters may occur. The spread of individual remainders could exceed +/-100% of the actual fixed true value adopted for the exercise, with the occasional negative result. Notionally the averaged data sets should loosely approximate to something approaching the true mean value across the working range tested, but discrepancies for individual data sets may average significant diffrences in small remainder form, or worse with only limited SRM plots available. That's about all such data sets are good for at best, a rough approximation. Some outcomes may also be hostage to the average work rate of the individual test series, which may differ sufficiently in magnitude to skew average outcomes.

When, as in Fig. 4, page 43, July 2017, 3 plots are cited **"as good as it gets"** but a fourth as demonstrating the **"lack of consistency or repeatability.",** one can only ask **;** Whatever ever happened to the call for plots in double figures as essential to providing meaningful samples for analysis? Back to reading tea leaves it seems.

"Even knowing these ranges (measurement limitations), the effects of the small difference between two large numbers problem could well prevent satisfactory data and analyses emerging." *(My italics).*

Exactly: A moment of sanity? The moral here would seem to be, where possible, leave small remainders alone.

In forming his **"consistent IHP"** conclusion, this was presumably by plotting the IHP Willans Lines at given speeds The relationship with speed and cut-off perhaps providing scope for secondary analysis. High R^2 values alone are not proof of accuracy, merely consistency as mutually agreed. The WRHP data Willans Lines return equally high, and often superior R^2 values. It can sometimes be more revealing to examine the IHP and WRHP data in specific steam consumption form (SSC). This is a form of amplification, and aberrations sometimes emerge. Some examples are below.

"but scatter is lack of repeatability," Some experimental instrumentation error is inevitable , normally falling within known limitations for direct measurements. In the small remainder situation, the margin of potential experimental error is intrinsically magnified, and are a troublesome basis for statistical examination since two uncertainties of unknown deviation contribute to every outcome. The line seems to have been taken here that poor statistical outcomes are solely indicative of dynamometer malfunction; an unlikely scenario. The WRTE and work done was recorded and summated mechanically over the course of test period. The IHP and ITE was determined on a sampling basis during the test period, the average of around half a dozen readings being taken as the test value. The indicator diagram determination was literally a case of "joining the dots", not that easy when faced with joining a “snowstorm” of dots in the early years of the test plant, as attested by Ron Pocklington. It was not until sometime in 1952 that RP took up the reins at Rugby, when indicator diagram “snowstorms” were evidently a problem.

The IHP data was less than “**consistent”** in the early years of the plant. The tests with Merchant Navy pacific 35022 were notorious for delivering negative MF values. Onwards from test run 744/1 to the last of the 1952 test series, the recording of IHP discontinued; WRHP recording continued. These tests involved variable speed at constant steam rate and cut-off, a test scenario inevitably involving part regulator working, with steam chest pressure reduced as speed increased. A procedure possibly adopted to replicate the way the Bullied pacifics were often worked in traffic. The WRHP curves recorded in these tests, as plotted in Figure 36 were of consistent form collectively for the four steam rates shown. Yes, of course such consistency is not in itself proof of accuracy, but it is a long way from the ‘tea leaf’ chaos delivered by the small remainder data, and is therefore the proper subject for regression analysis or any other means suited to testing the data’s veracity. It eliminates the problem of apportioning the random dual contributions to joint error as delivered by small remainders.

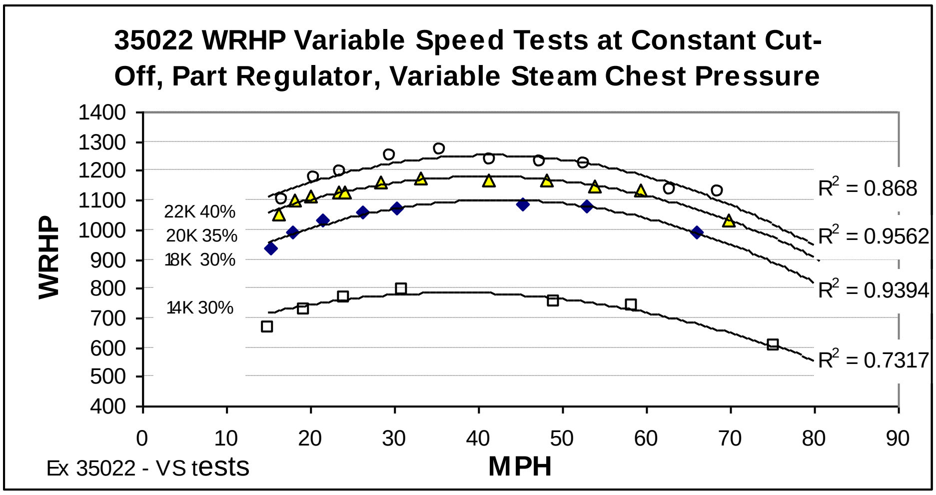

Figure 36. An orderly distribution of plots. The speed steps were initiated at 5 minute intervals.

This discovery prompted a comparison of the simultaneous IHP and WRHP SSC data where available. On this basis the IHP R^2 values were poor relative to the WRHP data, as exampled in Figure 37 below. Similarly erratic results obtained at 30 mph.

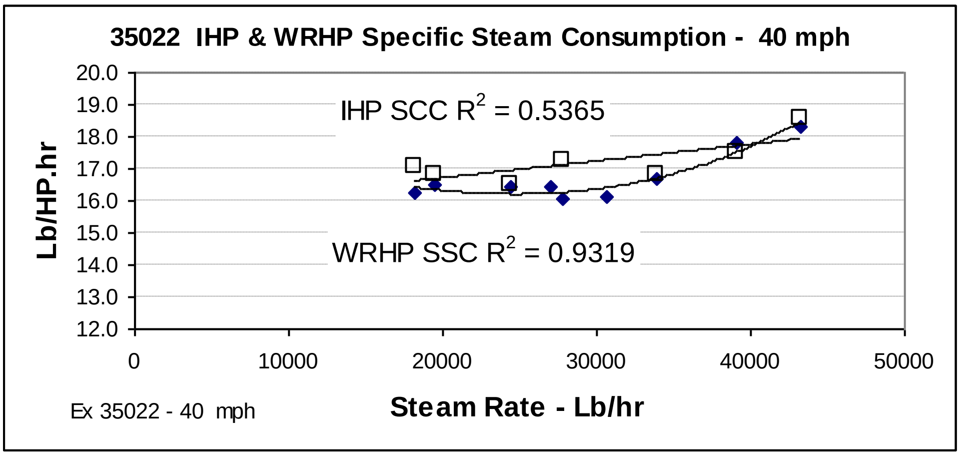

Figure 37. Both trend lines are polynomials, the IHP plots were unable return the characteristic shallow dished SSC curve as returned for WRHP. All the IHP plots should of course fall below the WRHP plots. A similar outcome was found at 30 mph with the IHP SSC poly trend line flat-lining; R^2 0.534; WRHP; R^2 0.9667 (Figure 10 page 12). The recorded WRHP SSC values are unexceptional in regard to implied thermal efficiency. These were full regulator tests, boiler pressure averaged 272lb and steam chest pressure 261 lb.

Noteworthy here that is that the nine WRHP SSC plots do not all slavishly follow the usually observable 'dog on a lead' response to the linked IHP pairings, but stick close to the trend line. The inference here is that the IHP plot at circa 28,000 lbs/hr is erroneous.

As possibly, in a different way, are all the other IHP plots. The WRHP SSCs at over 16 lb per WRHP hour are unremarkable.

"Fig.4. "…….. the three observations in the far top left of Fig 4 are as good as could be expected, but the fourth observation at 16,800 lbs demonstrates the lack of consistency, or repeatability.*"*

I could not see the "far top left" plots (I think top right was intended). The alleged rogue fourth 16800lb plot lower down falls within normal small remainder scatter. As far as I can see, all the plots shown fall within the predictable scatter range. Potentially, the trend for such small samples of small remainders over a short abscissa range could point anywhere. The approach portrayed is about as meaningful as reading tea leaves. Applying regression to random small remainders rather than the direct measured relationships generally returning high R^2 values is beyond logic.

"Heat from any effect (the Belleville washers and dashpot) will be lost from measurement, so that measured DP will have been too low and measured TSR too high. "

Any heat generated by the Belleville washers was minimal, resulting from the slight hysteresis effects. The force at the drawbar and Amsler dynamometer were exactly the same, simultaneous, equal and opposite. The dashpot, being in parallel, rather than in series was another matter. As Carling pointed out;* "*Being wise after the event he considered that, had the whole of the system been suspended on the drawbar, not fixed to the foundations, and acted as an* *inertia damper, there could have been no falsification of mean pull. It would have involved a major engineering modification and was not justified."* The dashpot falsification was plain to see; under steady state running conditions the recorded drawbar pull steadily increased. However, as now established, by the end of 1950 the dashpot had been decommissioned and is irrelevant. .

"Adrian Tester has informed me (personal communication) that Carling, superintendent of the plant, noted that the Amsler could record to +/- 1% for pull, and provided data within a +/- 1½% range for work done and +/- 2½% range for power (these are presumably at its own recording table, as might be expected from what these terms represent and the accuracy of the components. Only the pull, however, was recorded."

Did Adrian Tester realy write the the last sentence? Writing in Backtrack* he explains that *"speed was recorded in miles per hour via the Selsyn drive thereby enabling work done in ft lb to be integrated by means of an Amsler spherical integrator to give rail power."* If only pull was recorded, how would the "work done" (HPHRS), as clearly referred to, be determined? John has been given a sample Rugby test sheet: Drawbar Pull, Work Done, Speed,. Distance (miles), and the Mediating Gear Inch Seconds are among the items recorded. The mediating gear inch seconds recorded the net deviation of the coupled wheels from top dead centre on the rollers over the course of the test. If the recorded value was the same at the start and finish of the test no deviation had occurred. There was provision on the test sheet to record corrections as necessary. The rollers were manually rotated during calibration tests to determine the accuracy of the work done function.

<u>Pure Machinery Friction</u>

Some further points. It was about 16 years ago John Knowles conceived the notion of *Pure Machinery Friction* (PMF). The idea was to describe the machinery friction of the locomotive pistons and motion, free from the friction arising from the locomotive's vehicular aspects – the coupled wheel journals machinery *friction, and windage losses.* What was to be gained from such a concept remains a mystery. It thus might

---

- Locomotive Testing Stations (Part II), D R Carling. Proceedings of the Newcomen Society Volume 45 .1972, p. 173.
- *Stationary Locomotive Testing Part 3 – Adrian Tester; Backtrack; October 2013*

be construed as a corollary, that the vehicle resistance was somehow. *Impure Machinery Friction.* Did this involve different mechanical laws? If the idea of PMF seemed to be simplifying any analytical approach it could hardly do so.

Machinery friction is a complex iteration of ever shifting simple, dynamic and inertia force vectors in the course of each revolution. It was the resolution of all these forces *that was measured on the Rugby test plant. The PMF idea inevitably interferes with the recorded evidence to no conceivable purpose.* The small remainders involved are trouble enough without making them smaller and inevitably subject to flawed estimates in order to extract some supposed item of purity. How is the missing quantity to be apportioned between the PMF and the subtracted VR element? Since the manifold forces and resultant outcome (MF = ITE – WTRE) is less than the mathematical sum of the forces involved, how is this mitigation to be determined and divided when breaking down the measured outcome into two separate quantities? 'Pure' MF and 'impure' MF. Even if the notional VR element of the coupled wheel journal friction estimate was accurate it would, lacking a mitigation allowance, deduct too much, rendering the PMF element dubiously low as the default outcome. It is also noted, that any apparent improbabilities resulting from the supposed scientific analyses of these truncated remainders are axiomatically presented as proof of the Amsler Dynamometer deficiency: the Farnbro indicating equipment being assumed satisfactory. A travesty of supposed objective analysis..

46165 Tests Analysis

These tests are examined on an SRM basis by John Knowles in detail at 40 and 50 mph, tests at other speeds having insufficient plots. The conclusion that the 40 mph data is sufficient for close analysis is ill judged, 13 observations notwithstanding (actually only 12 returning an MF plot). The range of power and steam rates covered is very narrow, both increasing only 5% from the lowest to highest values:1220 IHP for test run 1492 to 1287 IHP for test run 1504. Under these circumstances the hazards of small remainder random scatter outcomes can potentially tilt the overall trend in numerous directions with both positive or negative constants of wide extremities possible. This is exactly what happens in this instance. The "so poor" data falls within the understood metrology limitations when examined over such a narrow range.

Some of the IHP data for 46165 is erratic when amplified to the SSC format, vide Figure 38 below. Whilst the WRHP plots the characteristic shallow dish shape, the IHP poly trend line is linear. Shades here of the problems with the 35022 IHP SSC data. There is an intriguing note in a list of modifications to the Farnboro' Indicator set-up dated 31st December 1955: "Improvements in 1955".

"3. Further developments were made in the mid-stroke devices, including one for the inside slidebar of Engine No. 46165.which could be adjusted from the outside whilst the engine is running." Had a problem come to light?

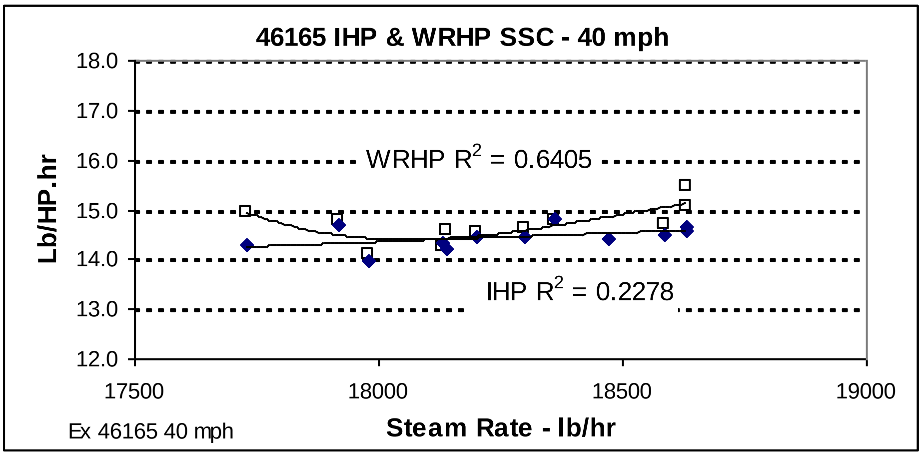

Figure 38. The WRHP plots (squares) generates a characteristic dished trend

Line. The R^2 value is mediocre. The IHP trend (dots) flat-lines, R^2 poor.

As already referred to, the determination of dead centres was critical to the accurate determination of indicator diagrams. Some of indicator diagrams for 35022's middle cylinder in the test bulletin feature compression loops and rats tails, which given the 9.8% clearance volume seems unlikely if dead centre had been properly established.

The other cylinders were not exactly anomaly free. The left cylinder front delivering a very skinny outline at 15% cut-off and low speed, and the right hand only achieving about half boiler pressure on admission back and front. All this was on full regulator, the diagrams only achieving a degree of even work back and front for all three cylinders at speeds of 45 mph and over. The Bulleid gear was clearly a law unto itself.

Returning now to the analysis of 46165 plots given in John Knowles Fig. 6, page 52, It is difficult to see where the numbers come from.

46165 Rugby Power & Tractive Effort Test Range at 50 mph						
Status	Test Run	JK Fig.6 PTTE	!HP	WRHP	ITE	WRTE
Minimum	1564	C,13,750	1130	1076	8,475	8,070
Maximum	1544	C.16,400	1957	1909	14,678	14,318

The mysterious PTTE on the Figure 6 x axis is described in the glossary of abbreviations as the Piston Thrust Tractive Effort, it being defined on page 58 as the net sum of the PTTES and the PTTEVsq'd; these being defined as "Piston Thrust Tractive Effort propulsive and compressive.", and "Piston Tractive Effort forces from unbalanced reciprocating masses dependent on speed squared". Note that the outcomes shown and tabled above exceed the minimum and maximum recorded ITE outcomes for 46155 at 50 mph. The point at which force PTTE impinges itself on 46165's anatomy is not explained, no force diagrams, sample calculations etc.

The outcomes are hard to follow. The ITE and WRTE working range recorded at Rugby increases by over 70%, in contrast the PTTE increases by only 11%, and at the lowest output contrives to exceed ITE by over 60%. What do the numerical values given for PTTE actually represent? On what parts of the Scot's anatomy is PTTE supposed to impinge? This is quite aside from the fact that the whole exercise is a conceptual misadventure.

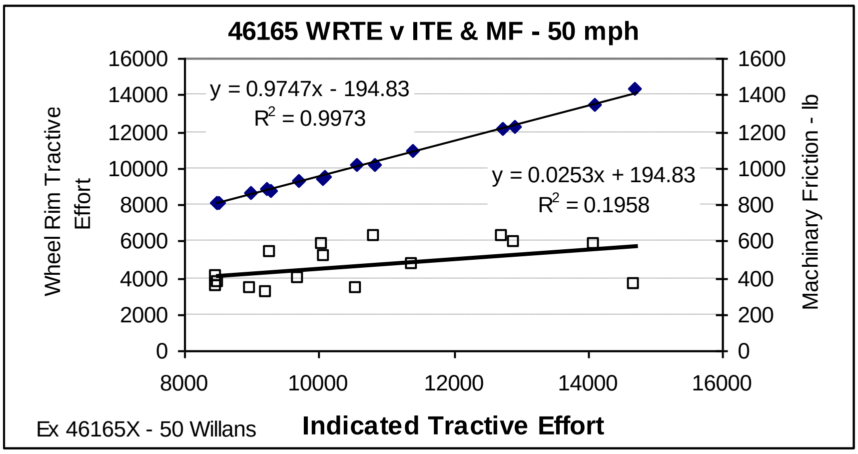

Figure 39. Aside from 1" smaller cylinders, the architecture of the Scot's and Jubilee's power transmissions are essentially identical. Combining the 50 mph test data for 46165 & 45722 (33 plots) returns y = 0.976 – 250.79, R^2 0.9943.
The average mechanical efficiency for the combigned outcome is 95.4%
In both instances the variable is around 2.5% of ITE. Both constants look low.

At one point John suggests a peer review. Confused thinking aside, his presentations fall a long way short of adequate explanation and clarity. Such things as force diagrams, assumed friction coefficients and basis for same, shifting force iterations, sample calculations, explanation of statistical dissection method and theory, etc are notably absent. The prime weakness is the lack of any convincing argument as to why the measured machinery friction, an intrinsically troublesome small remainder, is unnecessarily corrupted in pursuit of notional imaginary quantity – Pure Machinery Friction.

Among a long period of correspondence with John, I recall the following. "**I make no apologies for treating the coupled wheels as part of vehicle resistance, it is after all a vehicle."**

The locomotive is an active traction unit, not a passive vehicle.

I'm reminded by this of the civil servant at the Ministry of Agriculture and Fisheries, who wanted the welfare conditions of captive live crayfish to be the same as for aquatic vertebrates on the grounds it was called a fish. Shakespeare's *Merchant of Venice* also comes to mind, when, paraphrasing a little, Portia says; *You can have your pound of flesh, but do spill one drop of blood.*

This concludes my comments on John Knowles' July 2017 letter at this point; more will follow in my final summary. I now turn to his letter 2nd April 2018:

"A defective approach in UK to UK Loco testing."

This is largely focussed on Report L116 and its implications regarding locomotive testing in the UK generally. While it broadly covers the scope and substance of the report,
there are one or two critical omissions that would undermine the arguments he develops. Before dealing with this however, I will first make a few general points of clarification regarding Report L116 and the related report L109.

Scientists

My mention of "scientists" was with the Amsler design and commissioning staff in mind, not the Rugby staff. As manufacturers of international renown in the field of scientific instruments, the Amsler team may have included one or two scientists; but perhaps they were all engineers. Any distinction between the two professions in the context to the tasks in hand will be of little significance. Engineers such as Dennis Carling and Jim Jarvis will have shared a common understanding in the fields of applied mechanics and mathematics.

Report L116

Report L116 was focussed on the road test results for 9F 92050 and Crosti 9F 92023. Both locomotives had been tested at Rugby prior to the road tests. These locomotive were only indicated on the test plant The anomalous road test results prompted a second series of tests at Rugby with 92050. This second series included comparative tests between the Rugby and Derby versions of the Fanboro' indicators. These tests proved satisfactory (page 25 above). The fundamental problem was that when the recorded road test DBHP data was subtracted from the Rugby IHP data at given speed speeds and steam rates, the locomotive resistance curve was the wrong shape. The resistance curves for the BR5 and BR7 as derived from the test bulletins were similarly anomalous, but the LR curves were of varying form. See Figures 25 & 26 on page 25, and figure 37 below.

Report L109 and the "Supplement to Report L109" concerned the road test anomalies with Duchess 46225. Report R13 essentially took the form of the BR Test Bulletins, and incorporated the corrections in report L109, namely corrected DBHP curves (Drawing DTG .976). Unlike the 9Fs, 46225 was indicated on both plant and road tests. These tests too were anomalous, only coincident with the road tests at 50 mph.. The 9F test bulletin as published retained the anomalous DBHP data. Some unresolved departmental politics were perhaps in play here. E S Cox was reluctant to accept that in practice, the *Controlled Road Test* procedure (constant steam rate), was flawed in principle; the theory of constant blast pipe pressure for a given steam rate independent of speed having proved not quite so straightforward as originally thought.

Below, Figure 40 illustrates the extent to which the locomotive resistance curve as initially derived from the road tests for 92050, was "the wrong shape".

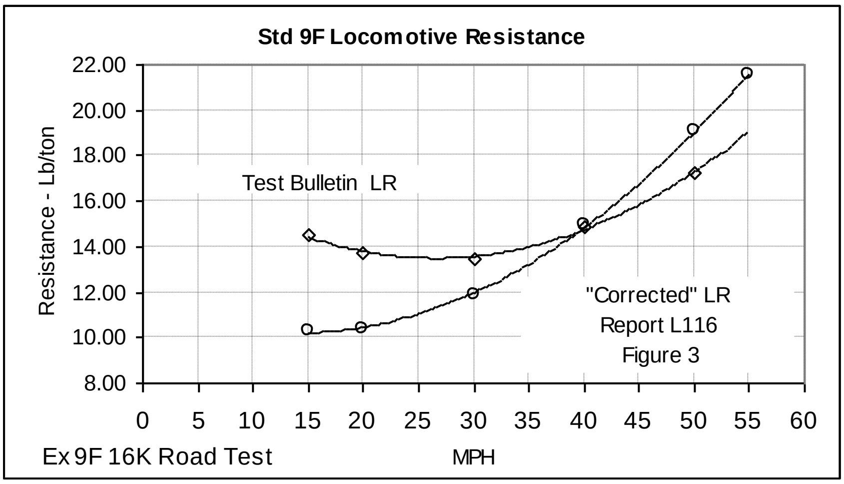

Figure 40 The bulletin curves takes on a slightly different form to Figure 26 page 26 above for Crosti 9F 92023. while having a similar crossover point. The bulletin locomotive resistance curve derives from Figure 11 - Figure 2 as for 16,000 lb/hr steam rate.

<u>"They could not find any thermodynamic reason,</u> which probably meant there was none, and picked, in speed effect, something which did not exist, as I show below. It is true that among the road test data, they had examples of tests where the result differed with the speed, eg by direction. These tests drop out as a basis because they were not

comparable with the principle of the testing, constant Q, V and BPP. One wonders if such non constancy by direction in a test was not the reason for the error." (my underlining)

This is with reference to road test anomalies involving steam rate variations under constant blast pipe pressure irrespective of speed. It is an inaccurate representation of what report L116 actually says. (The idea that direction may have changed the thermodynamics is most amusing.)

Report L116 Page 2 *"It is possible to correct the steam rate resulting from constant blast pipe pressure testing by two alterative methods, i.e.*

(a) Variable heat drop in exhaust steam according to temperature.
(b) Variable Density of Exhaust steam (Swindon Method).

Neither of these methods will entirely eliminate the discrepancy between Derby and Rugby."

Note the word "entirely", this is in deference to experimental error uncertainties (of which there are several mentions in the report), to which all aspects of measurement are subject. Note the reference to the *Swindon Method* regarding variable steam density. On L116 page 7 it states:

"It has been stated elsewhere that the steam rate variation which occurs when at fixed blast pipe pressure and variable speed is familiar at Swindon. A condition due to uncompensated change in density. Assuming that the flow rate is proportional to the product of the square roots of the differential pressure and density, it was suggested that the constancy of the steaming could be maintained by suitably varying the nominal blast pipe pressure to compensate for any observed change in density."

This was considered impractical for variable speed road tests where speed was frequently changing, and the exhaust temperature responses lagged. It was seen however as a suitable basis to amend the test data.

"………**and picked, in speed effect, something which did not exist."**
"Their analysis of the data was defective and biased the results of their thinking towards the idea that there was a speed effect."

At no point does John Knowles mention report L116 Figs. 5 and 6 showing variation in steam temperatures with speed. He appears to be unfamiliar with Charles Law con-
erning the temperature/volume relationship of gases, or to have ever looked at a Molliere Diagram. He describes these variations as **"peculiar effects"**.

Unfortunately the blast pipe pressure data is missing from the 92050 Series 2 Rugby tests data base. It does however include exhaust steam temperatures against steam rate. When plotted as T against Q in speed sets, the temperature separation, and by implication density variation that emerges, is plain to see.

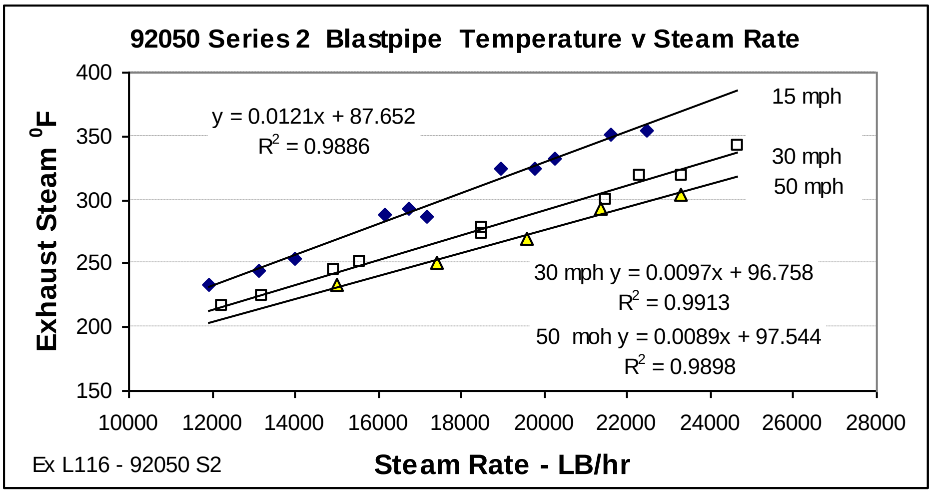

Figure 41 The reducing separation with speed accords with the trend indicated in L116 Figure 6 and is co-incident with the characteristic cylinder efficiency curve as a function of speed.

Examples of the temperature effect from test plant data are given in the internal "Comments on Test Report L116" document (Rugby June 1958) to which he has access.

Blast pipe pressure is a difficult measurement on account of the changing pressure during the exhaust cycle between exhaust release and compression. Some experiments comparing steady and pulsating gas flow through an orifice found that while the recorded manometer pressure in the pulsating situation was the mean of the maximum and minimum pressure per cycle, the quantity differed to that obtaining for steady flow at the same pressure. The effect varied with the frequency of pulsations, up to 200 per minute. The experiments were not entirely free of some uncertainty. *"…..the result did not indicate any improvement in the scatter of the final results, suggesting that the complexity of the problem is more fundamental than has been thought up to now."* *****
Also, close control of inlet steam temperatures was not possible.

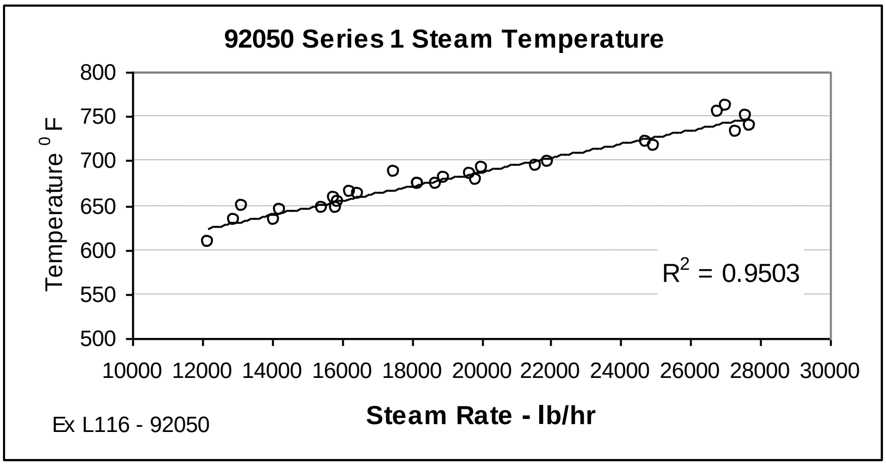

Figure 42 Departures from trend fall within the range +2.8%/-2.5%. Another potential source of scatter is variations in steam chest pressure. This ranged from 234 to 241 PSIG against the average value 239.1: +0.75/-2%.

- *Metering Pulsating Flow – Coefficients For Sharp-Edge Orifices;* J M Zarek, The Engineer, January 7 1955.

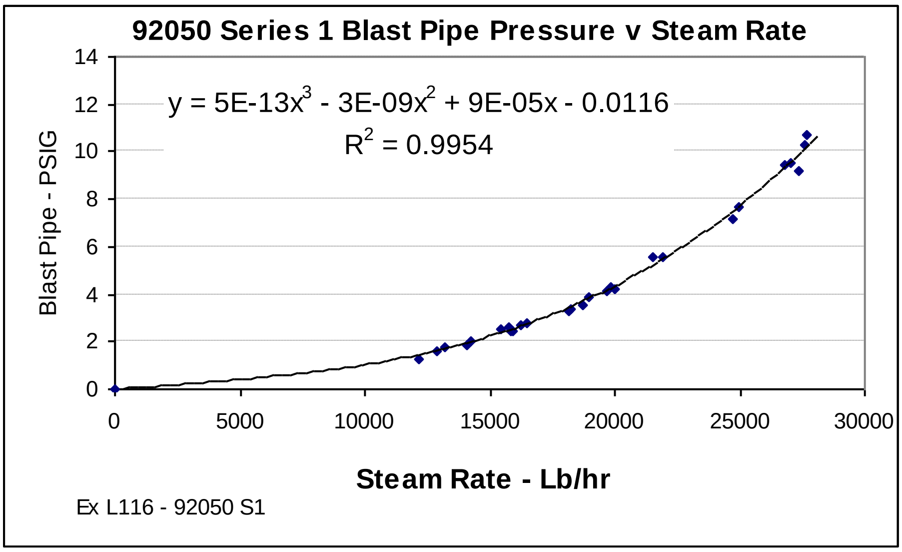

Figure 43 In the absence of any blast pipe pressure data for the Series 2 tests with 92050, the Series 1 tests must suffice. The low scatter here with only one or two visible strays from trend, and the high R^2 value, is typical of such data generally. The plots shown cover four speeds at 20, 30. 40 and 50 mph. An additional anchor point has been added to the plotted data, that being that when at rest, steam rate and blast pipe pressure will be zero: a simple matter. The constant shown should of course be zero, not -0.0116 lb.

At face value, Figure 43 supports the impression that blast pipe pressure is constant at any given steam rate independent of speed. Analysis of Q v BPP in separate speed sets reveals otherwise, as Figure 44 below.

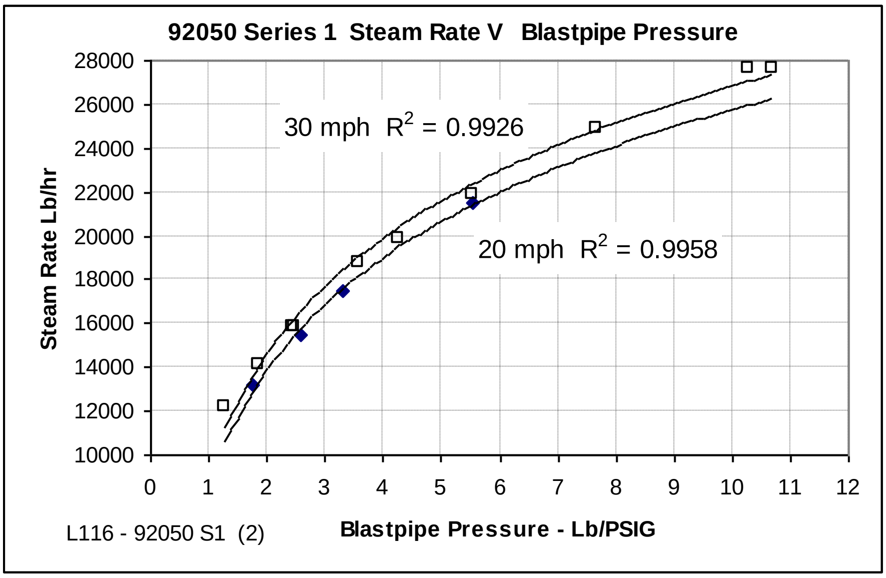

Figure 44, A clear increase in steam rate with speed is evident.

“They could not find any thermodynamic reason which probably meant there was none, and picked, in speed effect, something that did not exist,”

Really? The *Comments On Test Report L116* states: “*Variation in steam density is accepted by L116 as a condition which properly requires compensation.”*

Page 8 of the “Comments” cites the conclusions of looking at other test series when they were carried out *“on the assumption the effect did not exist.”* Some of the earlier test series were handicapped by the manometers then in use. Nevertheless some evidence was found for 45218, 44765, the BR7, 35022 and 46165.

The mean steam rate was 20,467 Lb/hr at a mean speed of 40.4 mph. On these figures the potential drift from the assumed Rugby steam rate on a road test at 20 mph would be about -700 lb/hr increasing the apparent LR based on the *supposed*

replication of the Rugby plant IHP data by about 42 HP, 790lb. It is apparent from the disparate locomotive resistances resulting from the Swindon controlled road tests that similar problems sometimes obtained. Hence the low speed LR anomaly identified for the two 4MT locomotives in Figure 32, page 33 above.

46165 Steam Rate Variation at 4Lb Blast Pipe Pressure.

MPH	20	35	50	65	80
Steam Lb/hr	19,772	20,325	20,588	20.936	21,142
% Mean	96.6%	99.3%	100.06%	102.2%	103,3%

The effects of changing temperature on steam density, and thereby the discharge rate through an orifice at a given pressure had been well understood long before the Rugby Test Plant was up und running. An undated booklet, probably dating from the 1930s, gives the following formula;*

Q = C Sq.rt P x W Lb/hr

Where C is the orifice constant as from tables, P is the pressure head across the orifice, and W is the steam density in Lb/cu.ft.

“There are three important defects in this work. First BPP is measured in atmospheric pressure or gauge pressure, whereas it should be in pressure absolute, as even an apprentice scientist knows.”

This is incorrect. As Kent’s formula shows, the discharge from an orifice is a function of the pressure head, steam density and the orifice discharge co-efficient. For “pressure head” read pressure differential, so if you adopt absolute pressure you have to set it against atmospheric pressure. So what differential do you end up with? Gauge Pressure!

A Swindon road test diagram with King 6013 in 1955 traces steaming rate as

.a function of sq.root P, defined as “Orifice Differential Pressure in PSIG.

“Second, the three curves in Fig.11 from which Table 2 (JK’s page 73) **was drawn above were fitted by free hand, with the initial pressure for each speed picked by eye.”**

The curves appear in accord with the formulae derived from the Rugby test data plots.

“Thirdly, there are insufficient observations at each of 30 and 50 mph (ten each) to analyse the effects of those speeds properly.”

This is unsubstantiated dogma.

In summary John Knowles assertion that there was “no thermodynamic reason to be found (in L116) why steam rate at a given blast pipe pressure varied with speed” is in defiance of the thermodynamic reality. Likewise his belief that for the purposes of analysis, blast pipe pressure should have been expressed as absolute pressure. It all amounts to another travesty of confused thought, and supposed science.

<u>A few more general points</u>.

“The higher (Crosti) **LR accords well with the back pressure, as shown by the Perform program. The frequently quoted idea that the resistance of the Crostis was high because they had weak frames is unsubstantiated; those quoting it as the reason for the high LR need to consider where the effects of the higher back pressure were felt,”**

……………………………………………………………………………………………………

***** *Flow Measurement Memoranda,* George Kent Ltd, undated. The firm later became Kent Instruments Ltd, and provided instrumentation for the test plant.

The point regarding frame flexure as unproven is fair enough, there was however a significant reduction in the inherent stiffness of the Crosti arrangements. Back pressure affects the mean effective pressure as determined by ‘Perform’ or an indicator diagram. and thus the Indicated Horsepower. There is no evidence of MF sensitivity to back pressure within the Rugby data. 9F 92250 returned the same mechanical efficiency for a given effort (ITE) in both guises; double chimney or Geisel ejector. The Giesel back pressure reduction was significant.

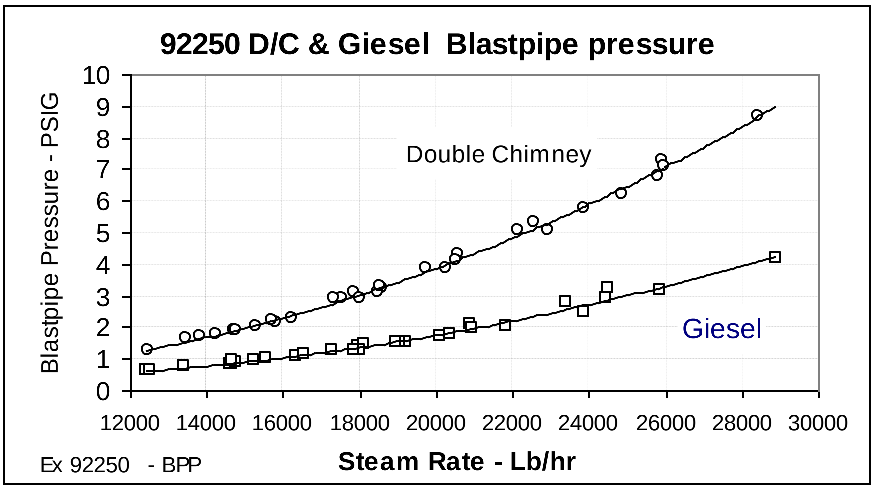

Figure 45 The significantly reduced back pressure with the Giesel ejector is evident.

The improved cylinder efficiency and reduced back pressure brought no measurable changes in mechanical efficiency, ref Figure 46. The back pressure reduction is implicit in the lower specific steam consumption and the increased blast pipe area with the Giesel injector fitted: the total nozzle area ratio was 302 sq.in. v 25.1 sq.in.

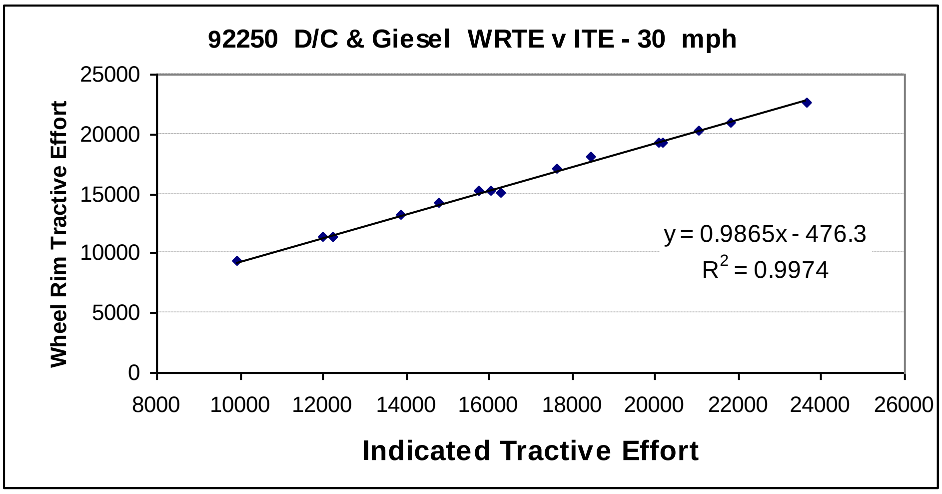

Figure 46 No discernable evidence here of a back pressure effect on mechanical6
efficiency. If such an effect exists, it must be very small.

"The conclusions of L116 should be forgotten, such as they are. That includes the supposed LR of a 9F."

Report L116 may not have been without some questionable facets, but it's general scientific thrust was sound, unlike John Knowles' tendentious ideas as exposed above. No locomotive resistance curve can be declared as perfect simply because it is a variable: modestly with the level of effort, and potentially more significantly, according to environmental circumstance. The latter itself can only be roughly determined, and can vary from minute to minute. Beyond that, as amply evidenced by this long running debate, small remainders inevitably render such determinations at best approximate in outcome. Whether on test plant or road test, possible error bars of ten or horsepower seem realistic. The Crosti and standard 9F LR resistance formulae given in Report L116 closely reflect the differences in machinery friction established on the test plant

and manifest on road tests - Figure 47 It has been assumed the LR values are for a steam rate of 16,000 Lb/hr, as on the comparative road tests.

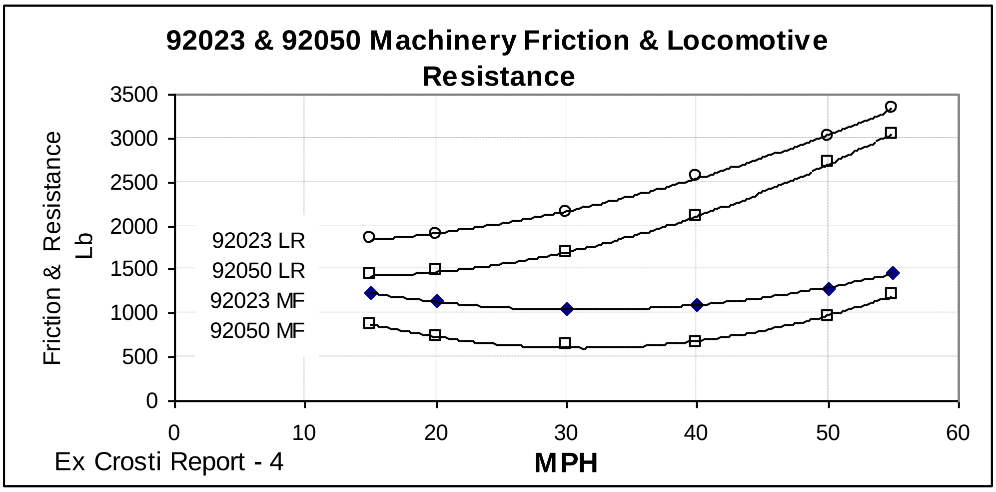

Figure 47. The plotted MF data at given speeds is as determined by Willans lines at a 16,000 Lb/hr steam rate. The Crosti 92023 v 9F 92050 differences in MF and LR are similar for both conditions in accord with the trends and magnitudes recorded on the test plant and the L116 LR formulae as Figures 2 & 3.

The 9F test bulletin includes a resistance curve for 16 ton mineral wagons as for a 7.5 mph 45 degree headwind. Presumably similar conditions apply to the L116 LR curves.

"It was Doug Landau who changed the subject to Steam Locomotive Resistance. Why did he do that? In my view he has not advanced the subject of steam locomotives one jot."

I will simply reply by asking if he thinks that such poor work, untenable concepts, statistical misadventures and false attributions put into the public domain should be beyond challenge?. Well over 90% of what I have presented is simply setting out the empirical evidence as recorded at Rugby in various ways and the difficulties and uncertainties associated with it. It is ironic to be accused of "playing with the data" given his corruption of the recorded data in the futile pursuit of dissecting ESRMs (even smaller remainders!). Far from playing with the data, I have highlighted its limitations and uncertainties, how, even within the contractual measurement limits, exact fits falling neatly across the full data range remain elusive. Ultimately therefore, stitching test data and bulletins together was inevitably something of a black art.

Overall, the Rugby test data was far from perfect, but it was also by far the best and most informative locomotive test data to become available. The simple linear relationship between WRTE and ITE comes through loud and clear in principle, uncertainties as to exact magnitudes notwithstanding.

Intrinsically, road testing, away from the ‘steady state’ conditions of the test plant, proved to be a more difficult proposition. Anomalies in both the Derby and Swindon test data reflect this. Derby road tests in particular, were compromised by the assumption that the Rugby cylinder characteristics, in the absence of indication, would be safely replicated by the supposed control of steam rate alone.

Readers will have to make up their own minds. My own view is that aside from one or two statements of the obvious, John Knowles has been wasting everybody’s time, including his own. Likewise his website on L*ocomotive Resistance*: another charade of confused thought and superficial scrutiny. He needs to have a serious rethink.

Doug Landau

30 December 2019