Reply to Doug Landau of 30th December 2019 from John Knowles on Steam Locomotive Resistance

A few introductory remarks:

1 Starting the above submission to the RPS as he did shows what was on Doug Landau's mind, ie replying to me, but also putting me in my place in his opinion, rather than putting forward the things he says he discovered at the NRM.

2 His submission of 26,000 words (some 84 A4 pages, a modest book) was far too long for a Word document. It should have been in several parts, and those parts put on the site as separate PDF documents. One of the parts could have been about me, if he cannot help himself, another on what was really new in what he saw at the NRM. The supposed Spreadsheet where the material is located in the RPS website needs tidying, so that each submission can be opened separately, preferably with each submission a separate PDF with date. Some of the diagrams in DHL's December 2019 submission did not open in a download. I hope nothing longer than about 7500 words is ever accepted for this site again.

3 I have had the Rugby test data since 1988, and gave copies to DHL plus some information from files before 2000. I really wonder what is new.

4 Is the RPS Journal (Milepost) or website the right place for his voluminous ideas on Steam Locomotive Resistance? Members are interested in well founded resistance information for steam locomotives to evaluate exceptional runs by such locomotives, but I judge from Milepost that such is a minority interest. They would normally look for such information in refereed technical journals. As steam is itself a minority subject in such journals now, I suggest that the Stephenson Locomotive Society Journal is the proper place for this subject, and then after the material has been distilled and much reduced in length – no journal will publish 26,000 words – and refereed. I wonder how many RPS members read the whole of DHL's submission? There was no need for such verbosity and multitude of graphs. Members who are interested but deterred by the length and debate might well say: let these people debate person to person; if they agree on something, then publish a summary in Milepost. Doug was a frequent contributor to the SLS Journal, but does not now contribute to it.

5 His submission (p 84) is preceded on the website by a piece of mine (p 64) on a defective approach adopted in BR steam locomotive testing. This appeared in the SLSJ for November/December 2019. I have no objection to that appearing on this site, although it would have been usual to ask my permission first. I certainly see no need for it to be published again almost immediately on the RPS website. As it happens, Doug Landau wrote a piece in reply to that, which was also published in the SLSJ in January/February 2020, in which he claimed that the method of Report L116 was scientifically sound. I disagree with that claim, but my reply awaits publication. I see that despite his having put forward his point of view, he wants to put it forward again on this website. Again that seems to be a vendetta against me.

6 I accept DHL's remarks on my use of the terms pressure absolute and pressure gauge.

Critical Speeds

When reading Doug Landau's preliminary remarks, especially were he says that the plant tests were preceded by calculations of the critical speeds for setting the Belleville washers, and presumably earlier the oil and air dashpot which were present in the connection to the dynamometer which gave the readings **o**f drawbar pull (DP), I looked for discussion on what they were intended to do, how that intention was fulfilled, what was the effect of the damping on pull, and how much there was of it. I was disappointed. The intention was to dampen the to and fro forces in the drawbar pull (item 1 below), but it was considered that damping falsified the readings, presumably of DP. Doug seems to think that it was the result of high traction forces, by which I think he means high piston thrusts at low speeds which caused the falsification. The lowest speed usually found in Rugby tests was 20 mph. The forces which are more likely to need damping because they can vary tremendously and have high values are the dynamic forces listed below, especially the first, because only the first passes through the drawgear.

Dynamic Forces in the Machinery Resistance

1 An alternating force along the locomotive and train through the drawbar, the result of unbalanced reciprocating masses in the mechanism. Work is necessary to create this force, which work is a source of MR. If any of that work is returned to the mechanism in moving the locomotive and train, that returned work would represent a reduction in the force created, and work performed. That would be possible only if the forward aspects of the alternating force consistently added to the tractive pull along the drawbar, and there was no other effect. The problem with that possibility is that the negative aspect of the alternating force reduces that pull by an equal negative amount. In addition, the forces in both directions, while of high maximum values at all but the lowest speeds, move only a tiny amount before reversing, rendering it impossible to obtain any sustained pull. Rather the alternating force is observed as a vibration. By observation, the alternating forces are gradually absorbed in the drawbar springs and in the inertia mass (unwillingness to move or change movement or direction) of tender and train. If not absorbed along the train by or at any particular vehicle, this force is the to and fro motion or rake felt by the passengers in that vehicle, causing them to nod slightly in unison and to hear the drawbar springs moving in and out. Even with the absorption in the inertia mass, the work required to create the forces remains as MR.

2 An alternating couple about the crank axle, again needing work for its creation, causing the locomotive proper to sway from side to side, resisted by the inertia mass of the locomotive proper, and in the side operating springs (or other devices such as inclined slides) of the leading or trailing trucks or bogies, so sprung or inclined to control curving. This sway has to be resisted because excessive sway, which rises with speed squared, can be dangerous.

3 A couple at the origin of the drawgear in the locomotive frame on account of any vertical difference in the height above rail of the coupled wheel centres and the drawgear, really a leverage, usually small.

4 An oscillation in the vertical plane at the line of reciprocation leading to up and down motion on each side of the balancing masses in the coupled wheels on to the rails and bridges, the well known hammer blow, resulting from the balancing masses in the coupled wheels rising and falling as those wheels revolve. Hammer blow worries the bridge engineers, and leads to the percentage of the reciprocating masses balanced being less than 100, usually 40 to 70%. The hammer blow is itself not part of MR, but the work involved in making the

oscillation is. (Revolving parts are usually completely balanced, but the balancing masses in the coupled wheels are not in the same plane as the parts concerned, leading to modest couples as well.) The 90 degree separation of the cylinders leads to uneven occurrence of the maximum hammer blows, requiring work to oscillate these uneven masses, and to operate a couple of the same kind as 2.

The operation of these four effects all require work, which is part of MR, and cannot be avoided with the engine moving, even when not under steam. I have noted the alternating force in trains hauled by many locomotives, from 15 inches gauge upwards, from four to ten coupled wheels, even with three cylinders.

The only way to eliminate the alternating forces is to balance the reciprocating masses completely or to oppose the forces directly. At Rugby, neither was done. Instead resistance was incorporated into the connection from the drawbar to the dynamometer to dampen (not eliminate) those forces, as Carling said. He did not explain how it was set up or operated, the extent of damping for any test, nor the resistance (DR). He considered that the testing apparatus needed protection from effects of resonance. Doug Landau has not found out or explained the matter either (as above). In any case, he does not give a file or library reference or title for these calculations, for those interested in discovering the meaning or value of the calculations. On a Testing Station, the distance between the coupled wheel centres and the dynamometer was not allowed to change, to avoid the coupled wheels moving back or forward and the locomotive to be subject to slight up or down gradients on the rollers, a source of defective readings compared with being on the level. The distance was controlled by a hydraulic mechanism, which recorded the effect on the work done, as Doug has previously explained.

The history of the damping was: having oil in a dashpot, air in the same dashpot, no damping, and Belleville washers. Jim Jarvis (a deceased engineer at Rugby) explained that there was a valve controlled by-pass to the dashpot, used to vary the extent of the damping, down to nil. I know of no record of the damping employed in particular tests. That means that the presence of DR affected the DP and consequently the value of (ITE – DP) (ie the TSAMR, or Testing Station Apparent MR) in any test to an unknown extent. Without knowledge of the DR employed in any test, the TSAMR cannot be interpreted properly (the constant of the CWBR, also present in the (ITE – DP) can be reasonably calculated).

The value of all the dynamic forces, the work involved in operating them, at every point in coupled wheel revolutions (or in the piston strokes, or at every crank angle) and the average for every stroke or revolution can be calculated from the masses and distances involved. They vary considerably during a stroke, and the maxima can be very high, even exceeding the tractive effort (here I quote from Professor Dalby in his book “Balancing Engines”). A method of exactly opposing these forces in the rod between the drawhook and the dynamometer at all speeds and DPs would require a much more complicated device, I suggest, than the dashpot or a stack of Belleville washers employed. Even if the damping could absorb say 75% and above of the alternating forces at a certain speed, the remainder are still considerable, and need to be known for interpretation, as do those absorbed. Any friction in the mechanism arising from these forces (say at the coupled wheel axleboxes) is also part of MR.

Readers are told that the calculations do not support my suppositions about how the damping functioned. So DHL interpreted them to that extent. If critical speeds are of interest, however, that can leave many other matters necessary to evaluating the Ruby DPs and their usefulness if any. If DHL was able to check and decry my so-called suppositions he must have been able to decipher more than he has declared about what was considered in setting up the Belleville washers and why, especially the resistance built into the Belleville washers for a particular test.

I have previously calculated MR including 1 to 4 for the LMS 5 and associated LR. These are much higher than anything DHL has ever deduced or calculated, and than the figures for the very similar BR 5 from Rugby LTS. That seems to have been the inspiration for all the thousands of words from him criticizing me ever since.

Analysis of Data

Rsqd has a special meaning when numbers are presented. It is called the coefficient of determination, and gives the extent to which the data concerned has been explained by some relationship, one inherent in the science of the subject, or a hypothesis being tested for such. Doug uses it for all data he presents in graphs. Because he is limited in his ability to form and test hypotheses and conduct multiple regressions, using two or more explanatory variables, indeed any regressions, the Rsqd he uses in many graphs is misleading or wrong, as in purely demonstrative graphs (figs 7, 8 and 9 are not causal), while WRHP against speed on a graph does not explain anything. It can of course exemplify, but then it is necessary to explain whether it comes from the pure science or some experiment; in the latter case, if a relationship has been fitted, giving Rsqd would be correct. I notice that Doug always presents the Willans lines as near straight lines, and says he doubts my claim that the relationship between ITE and Q and V together in Rugby ITE data is well founded. Try ln ITE = a ln Q/b lnV. Just to make sure I am understood, however, I am not claiming that regressions of experimental data make the data correct. Anything but. If ITE or DP is poorly measured (see below), nothing can rectify that. Indeed trial regressions are often the best way of showing the hypothesis to be completely wrong, or that the experiment to test it has been poorly done. It is odd that Doug does not know of multiple regression – it is the basis of the quadratic equation used to give the VR of railway vehicles, which he uses in the calculation of locomotive power.

What happened in May – June 1967? The LTS had long closed. Am I accused of 12% error in WRHP? Reference needed.

Figure 5 has MR falling from 15 mph to almost zero at 75. A little thought will show that this is rubbish. As speed rises, all the dynamic forces rise with speed squared, the frictions in the mechanism mostly rise with speed, while no sources of friction fall to zero as speed rises.

I know that the DP reading for any test was averaged and I have some ranges. Why were recorded DP readings for a test greater than any observed, whatever the date? Doug expresses satisfaction that observed MR (as ITE – DP) turns positive (with Fig 7). That MR includes CWBR and DR discussed above. When these are deducted, the TSAMR is not necessarily positive.

As usual, Doug uses his own version of how to analyse data and how to reach and present conclusions on the value of what he does. He has learnt nothing of statistics as a science (he seems to think it has to do with anything which can be shown on a graph, which limits him to a single explanatory variable, as in every relationship he puts forward). It is also why he puts forward relationships between ITE and DP to explain MR, and discusses Mechanical Efficiency in the context of MR (I realise Mechanical Efficiency on a LTS can be defined as DP/ITE), but the subject is MR, and the mechanical efficiency figures he gives are improbably high, reflecting the same old problem, the TSAMR figures are improbably low. And he regards linearity as of great value, but then notes that the sign on the constant implies impossibility with the regulator shut. That is correct as a point, but the lowest x variable he shows is 14,000 lbs ITE. The linear form does not allow for the behaviour to change at lower ITE values; anything can happen at lower ITEs; the form of the relationship he chose does not allow whatever that might be to emerge.

He makes a big thing out of eliminating observations to improve Rsqd, already 0.99, although no other statistical tests are given, Rsqd being the only test he uses, which he says runs counter to an edict of mine that the more observations the better. That interpretation is part of the indulgent approach he adopts to science and statistics, and his search for negative things to say about me. It all depends on what observations might be removed and the criteria for their removal, as a good book on statistics will tell him. See also the end of this note, where I remove some observations, and reduce the value of all remaining observations by the same amount in an attempt to extract some slightly better value from Rugby TSAMR data than is available from a "first pass" regression. The point is that all the data from an experiment which might influence the explanation of the subject under investigation should be used (experiments which turn out badly should not have been recorded), all speeds together, because speed can be expected to be a major determinant of MR, and should therefore be an explanatory variable among others. Apparent outliers should not be excluded in a first pass. Apparently imperfect data should be tested to see if some influence not initially considered by the analyst should be tried, and tested for whether the outliers show any influence on the subject, especially if only single explanatory variables are being used, as in the Landau method.

At the end of his enormous document, DHL claims that one of the reasons he changed the subject to steam locomotive resistance was my poor dealing in statistics. What a hide! What an accusation from he who knows so little about the subject! I have some education, and considerable experience in the subject and can say with confidence that he knows so little about the subject that his declarations are waste of space. The things to use to try to explain TSAMR are at least and at first, piston thrusts and speed squared, because from first principles they are the major variable influences on it. A constant should also be allowed to emerge. CWBR (calculable from bearing dimensions and masses borne by them with high accuracy) should be deducted, as should DR. Then all test statistics should be examined. Despite my attempt to show DHL in a previous post what these are and what they show, he shows no evidence of knowing about them, as he should before he tells the world about my qualifications and abilities in statistics. If the fitted equation proves little, I would change its form from added terms to multiplied, or to have an index on speed to be given by the regression. See at the end my reasons for finding that the Rugby data does not explain MR.

I suggest DHL forget his ideas on explaining the data, or finding "proof", and obtain a good book on statistics and study it.

What is a differential pressure element in a hydraulic dynamometer? What is the Mi-o index, what does it show and how is it obtained?

Fig 14a – why is data from 92166 included with that from another locomotive? Are the delta mechanical efficiency figures statistically different from zero? In the context of MR, why such attention to mechanical efficiency? (I know the connection between them, and would say that such very high ME is an indicator of inadequate registering of MR).

With Fig 17, the reader is informed that WRTE is linear, with ITE, but that is not said in the headline, and is not shown to be universally so, being exemplified only for 30 mph. As ITE at a given steam rate is not linear with speed, and as MR is composed of elements which are not linear with speed, such linearity would be surprising, indeed wrong. I conclude that it is the result of a single variable equation as DHL uses with an Excel line to a set of data. See above for what he should have done. He should also calculate MR from first principles based on its causes or sources. I have done that. I find it much higher than anything derivable from the Rugby data. It is all very well to claim that people at Rugby were highly qualified, the devices the best available, and properly checked, but that does not give the results special credence. Those results must satisfy explanatory equations which accord with the physics.

I consider (based on the evidence of all he says and does in these posts, especially the claims he makes) that Doug Landau is not competent in technical and statistical terms to derive MR from the Rugby data, or to express any judgement on the value of that data for such a purpose. He seems to believe that MR is a function of ITE. That is simplistic and wrong, but his view has not been refereed and will mislead others (that is not to say, that MR might be expressed in certain ranges by lbs mep as a shortcut, as I have done, but that is a stage down the line), hence the following. The MR of a two cylinder steam locomotive with the usual arrangement of the pistons on the two sides separated by 90 degrees and results from items which reciprocate (move to and fro) and which revolve. The parts which reciprocate are the pistons with their rings and rods, the crosshead and its slipper, the leading end of the connecting rod, and similar parts of the valve gear. Those which revolve are the remaining mass of the connecting rod, the big ends, the coupling rods, the bushes of these rods on their pins, the journals of the axles in the CW axle boxes, the balancing masses in the wheels, while the links in the valve gear oscillate. Friction occurs on the cylinder walls, the glands, the crosshead pin, the crosshead slipper on its guides, the big end, the coupling rod pins, and at various points in the valve gear. MR also arises from the work done in the operation of the dynamic forces, and in the oscillation from lack of balance of various masses, including the reciprocating parts and the balancing masses themselves, plus any friction from the action of those forces.

On a Testing Station like Rugby, there are problems in measuring the WRTE, which is in principle DP deducted from the ITE to give the MR. At Rugby, this was measured not at the CW rims, but by a dynamometer at the end of the drawbar, on the assumption that the pull there (DP) and the WRTE are equal. This assumption was obviously not true. The CWBR, usually considered part of the Vehicle Resistance, occurs between the ITE and the DP, and its constant value can be calculated, from the bearing dimensions, the mass borne by them, the resulting pressure, the appropriate coefficient of friction (Cf) for that pressure, and the ratio of the bearing diameter to the CW diameter, and should be deducted. So long as that is understood, it matters little whether the CWBR constant is deducted from the raw data or from the regression result. In addition, at Rugby, an unknown DR was incorporated between ITE and DP. And of course no tender was attached; on the road, it was a source of VR before the drawbar. It had no MR.

The Rugby TS also had water dynamometers, through which the load was put on the locomotive at the CWR by churning water and raising its temperature, one per CW. Each gave a reading of WRTE directly, but Rugby preferred to obtain what it considered to be the WRTE for the whole locomotive at the drawbar hydraulic dynamometer, because it was considered that was easier. In his piece, Doug Landau thought the change in water temperature not very accurate; he is poorly informed. Many powers and efforts are accurately obtained through change in water temperature, of motor vehicles especially. Furthermore, it is amazing that no check was ever made at Rugby using the two sources of information on effort at the CWRs, especially as the braking dynamometers were used in monitoring the braking, and the braking had to be accurate, and the inability to find sense in the readings at the drawbar.

No tests were conducted at Rugby with all the reciprocating masses balanced solely for the tests, and the DR eliminated. That would have revealed proper WRTE as the DP of the dynamometer which was used, subject of course to the ITE being correctly measured, and the values of the WRTE as a thick line of small variations being capable of interpretation.

I consider after all the above, that the Rugby plant does not reveal steam locomotive MR for four major reasons:

1 The TSAMR is so low in itself, and does not accord in any respect with the factors which should, from first principles, explain it. For the latter point, I have taken the LTS data for tests on 9F 92250, the last steam engine tested on the plant, from April to September 1959, by when the testing procedures should have been as perfected as they were to be. There were 62 observations. (ITE – DP) (TSAMR) is low by observation, from -37.5 to 1472, with an average of 719.6. Four are less than the expected constant value of the CWBR of 228 lbs, eight less than 400 lbs and 22 less than 600 lbs. The PTWR can be expected to be a little less in tests where the engine had a double chimney or a Giesl Oblong Ejector, but the effect of those fitments on MR should be comparatively modest in relation to all the sources of MR together. I have done some new regressions, five with one explanatory variable, of the kind Doug Landau favours, and three with two. In the following, Q is the steam rate in lbs/hr, PTWR the piston thrusts at the CW rims, Rsqd the coefficient of determination, SEE the Standard Error of the Estimate, t a test statistic, with 1 for the constant, 2 for the first or only variable, and 3 for the second variable. All the data from the Rugby LTS records at the NRM. The low value of most coefficients is the result of the high values of the variables, Q, ITE and PTWR occur in tens of thousands, and Vsqd at and above 40 mph in thousands.

Equations for TSAMR in lbs

(1) -73.3 + .0417 Q; Rsqd .285, SEE 279, t1 -0.43, t2 4.81
(2) 498 + .0145 ITE; Rsqd .07, SEE 318, t1 4.33, t2 2.06
(3) 433 + .015 PTWR; R sqd .04, SEE 324, t1 2.22, t2 1.51
(4) 629 + 2.59 V; Rsqd .008, SEE 328, t1 4.39, t2 0.69
(5) 1775 - .05Vsqd; Rsqd .02, SEE 2867, t1 1.47, t2 -1.1

(6) 666 + .046 PTWR - .0339 Vsqd; R sqd .02, SEE 2888, t1 0.23, t2 .04, t3 -0.57
(7) 1695 +.024 PTWR -33.2 V; Rsqd .027, SEE 2883, t1 .041, t2 .024, t3 -0.73
(8) 16.9 +.047 Q -.01 PTWR; Rsqd .30, SEE 278.7, t1 1.09, t2 4.59, t3 -1.02

These equations do not reveal any close dependence of TSAMR on the variables which, from first principles, were chosen to explain it. I shall leave it to Doug to find out the usefulness of t and the SEE, both of which have to do with the range in which the results overall and for particular coefficients can be accepted with a certain level of confidence. In this case, however, the data are such that it would not be expected that they would display anything much of value on MR. It is however possible for the analyst to use experience and observation to perform some slightly different regressions. One is to exclude observations of TSAR which are clearly not sensible. As the calculated constant of the CWBR of a 9F is 228 lbs, and the DR might be considered as 172 lbs (my estimate after considering the maximum values of the alternating force at the higher speeds) total 400 lbs, any values of TSAR of 400 lbs or less are clearly wrong, and have been excluded for the next two regressions. That reduces the observations to 52. A regression of Vsqd and PTWR on the remainder yields TSAMR of

(9) 514 + .04Vsqd + .0115 PTWR; Rsqd .02, t1 1.85, t2 0.7, t3 1.05, SEE 269

Apart from giving a reasonable blessing to the constant, this is of greater help. If 400 is deducted from each value, and the remainder regressed on V sqd and PTWR with no constant, and the 400 added back to the result, a reasonable equation is obtained

(10) (400) + .605 Vsqd + .0106 PTWR; (numbers apply only to the second and third terms) R sqd .70, SEE 269, t2 1.62, t3 5.4.

This shows that the estimation of (9) is taken over by estimation of the constant, leaving little data for the other terms to explain the rest of TSAMR. In (10) with the 400 being added back, it is not possible to say how good it is, the t2 for Vsqd has a wide range of uncertainty, but the PTWR term is well established. The procedure is repeated for the sum of the CWBR constant and the DR to be 500. The number of observations is reduced to 46. The results are:

(11) 36 + .08Vsqd + .01PTWR; Rsqd 0.04, SEE 253, t1 .135, t2 1.32, t3 0.96
(12) (500) + .09Vsqd + .011 PTWR; (second and third terms) Rsqd 0.66, t2 2.23, t3 3.95

In (12), if only the reliability of the added back constant in this case were available (and obviously it should be high), this would be regarded as a useful equation. Hence, if the true constant of TSAR is 500 lbs or a little below, this is a reasonable equation for TSAMR from the data for 92250. It is important, however, to say that the cofficients on Vsqd and PTWR are decidedly low compared with values calculated from first principles, the result of the generally low values of the data.

3 Values of MR calculated from first principles are much higher than observed at Rugby, even more so if CWBR and plausible values of DR are deducted from TSAMR as above.

4 D R Carling, Superintending Engineer of the Rugby plant, considered that what the plant registered was DBTE and not ITE. He considered that the ITE recorded there was suspect.

That does not cause me to withdraw my views on the problems with the DP and associated MR recorded there – they are too well founded, but if ITE was suspect, it can be the source of low MR, and aspects of the apparent behaviour of MR; indeed, tho' Carling does not say that a consequence of suspect ITE is suspect MR, it can explain the difficulties with interpreting the low values of MR. It is worth saying that a low ITE leads to low values of PTWR.

The Perform Programme

I explained that I used this programme to obtain indicator diagrams, from which to derive piston thrusts, leading to another series of negative remarks from Doug Landau. He criticises the programme, derived by the late Prof W B Hall. He has not so far as I know made these comments public previously, nor has anyone else, in the 21 years since the programme was released in the SLS Journal for May/June and July/August 1999. Prof Hall was very much aware of literature going back a very long way, especially efforts of many people to explain the "missing quantity" in steam consumption. The coefficients he uses and suggests are used in science for steam expanding under load, and uses a system of continuous differentiation and integration to explain the behaviour of valves, and pressure difference to explain steam flow and action. The programme has been used to explain as never before many aspects of the operation of steam locomotives, as he should be aware. I have used it myself to explain how and why certain locomotives I have known well have behaved as they did, and how improved versions of certain locomotives improved efficiency compared with the unimproved. How else would Doug Landau obtain the pressure along a stroke to calculate piston thrusts, positive and negative? It must be better than any mechanical indicator. Given what is known of BR testing, it is perhaps unfortunate that Prof Hall used some BR test results to exemplify the programme. How does Doug Landau, stout defender of everything done at LTS Rugby, criticise BR testing as implied by his remarks?

Until be died, Prof Hall made alterations to the programme which satisfied various criticisms, leading to Perform 2, which I use. David Pawson used that version to test a large number of the Rugby ITE results, and found, with slight tweakings of certain items that Perform reproduced the BR results very well (all published in the SLSJ in the early years of the century). Doug Landau's concerns are therefore not valid, nor Mr Carling's concerns about the ITE readings there (4 immediately above).

The Crosti 9F

It is almost standard for those writing about these engines to say that they had higher TSAR than the standard 9Fs, on account of reduced depth of frame stretchers, to fit in the preheater drum below the boiler barrel. That is not why I think they had higher TSAR. It is because they had very restricted blast nozzles in the chimneys, which created very high back pressure in the exhaust strokes of the cylinders, as can be seen using Perform, and calculating the piston thrusts. They were designed with numerous U shaped frame stays under the preheater drum, of some strength in the U itself, intended to reproduce the frame stiffness of the standard 9Fs, and were of course well stayed at the cylinders, firebox and dragbox. It is not clear that shallow frame stays would in any case have led to high friction in the axleboxes; if the frame was less stiff, the friction might well have been reduced.

Conclusion

I shall return to comments on the rest of DHL's massive piece of December 2019, although I expect to do that in less detail. I doubt that the LTS as set up and operated can reveal MR. I am perfectly willing for the content of this piece to be refereed. Indeed, I would like it to be.

6th November 2020.