

**Response to Mr David Wardale's comments
on the Ph.D. thesis published as
"The Fire Burns Much Better..."**

For readability reasons the figures, as used in the thesis, are reproduced in the discussion on the functionality of the exhaust system.

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Sep 2013

Introduction

Mr. Wardale's comments were taken and transcribed from the .pdf version on the 5AT website. He certainly did a careful reading and produced 14 pages of comments.

The present answer is a very revised version from my original response in 2007, which I did not publish at that time for compassionate reasons.

I should start by explaining a bit about the narrow path to a Ph.D. I originally had a number of very unripe ideas, which were weeded out very thoroughly over four years by my supervisors at the University of Sheffield. Once the text of the thesis was produced, it was handed over to the examiners to study, and two months later I had a very stimulating discussion for a couple of hours during oral examination. After some minor corrections the thesis was finally academically approved. It should be remembered that this was an academic exercise in the history of technology, and not an engineering treatise intended to define a 'one best theory' of current practice.

The original idea for the thesis was just to write a history of the sometimes-winding path towards functional front-end design. This of course includes the contributions of a number of people who were important and often-cited figures in the past, but are considered of lesser importance now. I saw little reason to 'pre-censor' the history by only describing a path to success as if it were inevitable, particularly since what we regard as 'ultimate wisdom' today (September 2013) may well be subject in the future to a radical paradigm shift, perhaps similar to what overtook some of the historical figures in front-end design.

I have been invited on a number of occasions, by both Dutch and English supervisors, to dig into the theory of front-ends, not so much to extend known theory as to make the present state of the art understandable. In particular, I thought the "what is really happening in a front end" question needed to be addressed and, as far as possible, answered. In the thesis, I have made an effort to carefully guide the reader through the available research material, giving many examples, in part to find explanations for this specific question.

Before I begin, remember that some of the points Mr. Wardale has raised have already been through "the academic sieve" and properly peer-reviewed by experts, and as such some of his comments come as "mustard after the meal". Other points must be rejected out of hand, especially his problems with 'textbook' theory, here introduced for the first time in front-end theory.

I do not wish to go into a full detailed response to every comment. Many, of course, are proper corrections, for which I am greatly thankful. These have been added directly to the Errata page of the website, www.thefireburnsmuchbetter.nl, and I will not address them again separately here.

It appears to me that quite a few of the comments in the response are based on personal unsubstantiated opinion, normative rather than objective. I don't intend to take up a discussion about opinions, as nothing factual can be gained by that. Instead, I shall attempt to restrain myself to those points that I think can be proven. I have tried in the thesis to make a genuine effort to justify any suppositions with facts and figures, and substantiate them, as far as possible, with proper methodology. Admittedly this is in a

special academic context, but I believe the results of a proper analysis should stand technically as well.

Since a number of remarks center around identical themes I would like to take up such themes at first. Let us start by giving an explanation of the functioning of a front-end as it appears to me.

Functional description of the front-end

All the driving momentum of the front-end is provided by exhaust steam flow through the orifice(s). Once exhaust steam is ejected from the orifice, it starts entraining the surrounding gases by redistributing the exhaust jet momentum with them. It can be shown from experiments (photo p.88, Goss p.266) and the consequential textbook theory (Rajaratnam p. 424) that the spread of a pure evolving jet can be regarded as a straight cone. From measurements (Trüpel p.115, Young p.424) and calculations (Tollmien p.117) it can be proven that the velocity distribution in the jet when plotted develops the shape of a Gaussian distribution not unlike the normal distribution. The leading principle can be taken as “conservation of momentum” and as a consequence the momentum within the jet at the chimney entrance is still almost the same as that of the issued steam, there being only slight contribution from the entrained-gas characteristics close to the throat. Again from textbook theory, based on measurements (Rajaratnam p. 120), it can be shown that at a distance of about 6 orifice diameters from the orifice, about half of the gases to be expelled will have already become entrained. The other half of the entrainment occurs within the chimney.

Once the evolving jet is within the chimney, the momentum redistribution continues. Because it is now happening within a defined volume, the momentum and velocity distribution will gradually become more uniform. It should be clear that a parallel chimney has a limit to this effect, while a tapered chimney with proper ever-increasing cross area allows for lower uniform exit-velocity values.

It is possible to calculate the overall momentum from the chimney entrance velocity distribution, and compare that with the similarly-calculated momentum of a fully uniform velocity distribution at the chimney exit. It is clear (see the calculation on pp. 141-143 of the thesis), that these show two distinct mass distributions — the calculated uniform velocity distribution at the chimney exit contains more mass than that at the entrance. Since it is appropriate to assume that conservation of mass is at work, it is clear to me that the chimney has ingested (and accelerated) some additional mass, apart from that of the entering mixed jet. This can be clearly seen on page 88 fig.4.7. It should then also be clear that the ‘missing half’ of the gases to be expelled must be coflowing with the jet into the chimney.

The direct calculation of the redistribution of the momentum (as discussed on p.459) shows the same results, and a simulation of this was shown during the York symposium. (Website www.thefireburnsmuchbetter.nl , York Proceedings Text p.10 fig.23-26.) The uniform velocity distribution at the chimney exit is an idealized situation, and in practice it is not fully achieved, as measurements of the exit profiles by Young (p.424) and de Gruyter (p.429) show. As a consequence the chimney should be as long as

possible, to achieve as much uniformity as possible, the longer the better.

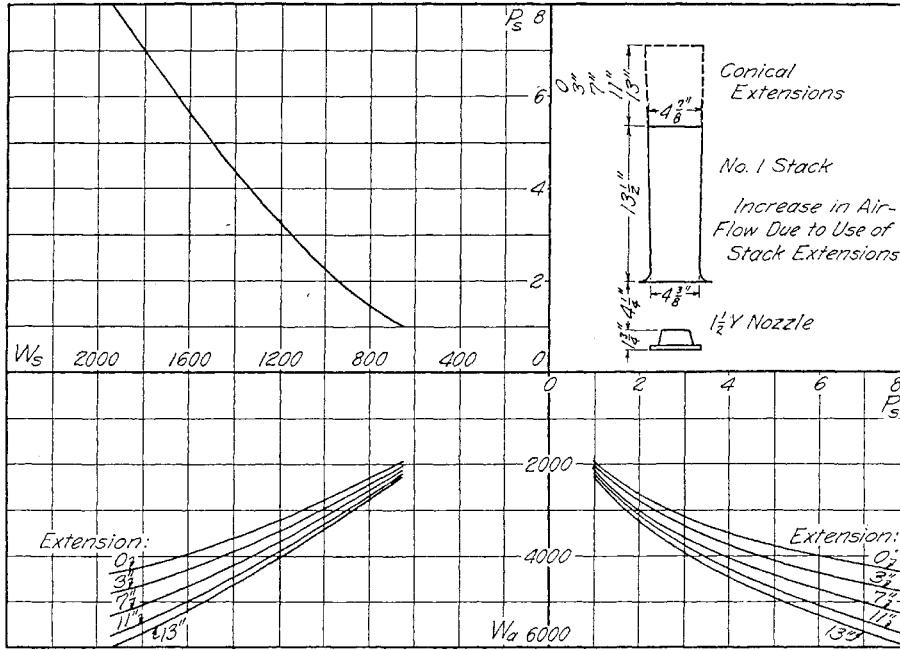


FIG. 17. STACK HEIGHT TESTS, No. 1 STACK

Conical extensions of four different lengths having the same taper as the No. 1 stack, were added to it; the increase in air flow was remarkable, but any extension greater than 3 in. represents an impracticable condition in actual locomotive service due to clearance limits.

Figure 1 The influence of chimney height for a tapered chimney

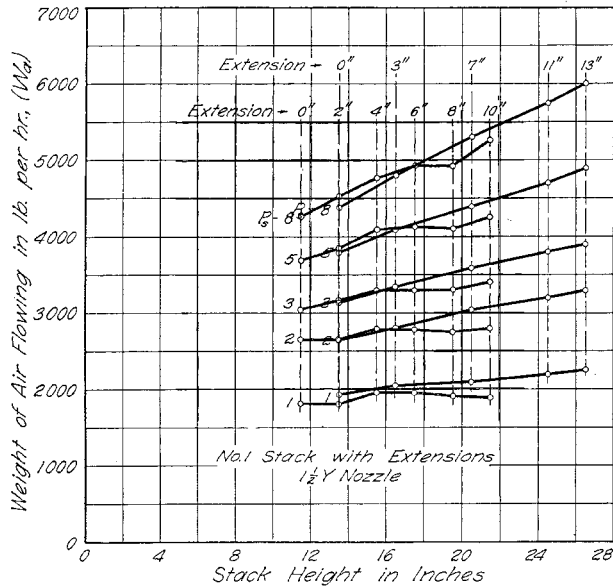


FIG. 18. RELATION BETWEEN STACK HEIGHT AND AIR FLOW, No. 1 STACK

The effect of chimney length is clearly shown by the tests of Young as depicted on pp. 420-421 of the thesis, figure 1 above. It is also clear from testing that a parallel chimney shows a limit to its usable length in this respect, while a tapered chimney with 'correct' ever-increasing cross section allows for quicker achievement of uniform velocity values. This limiting behavior of the parallel chimney can be seen by superimposing graph A25.9 on graph 25.11. as done in Figure 2.

Figure 2 Combination of results of elongated tapered and parallel chimneys

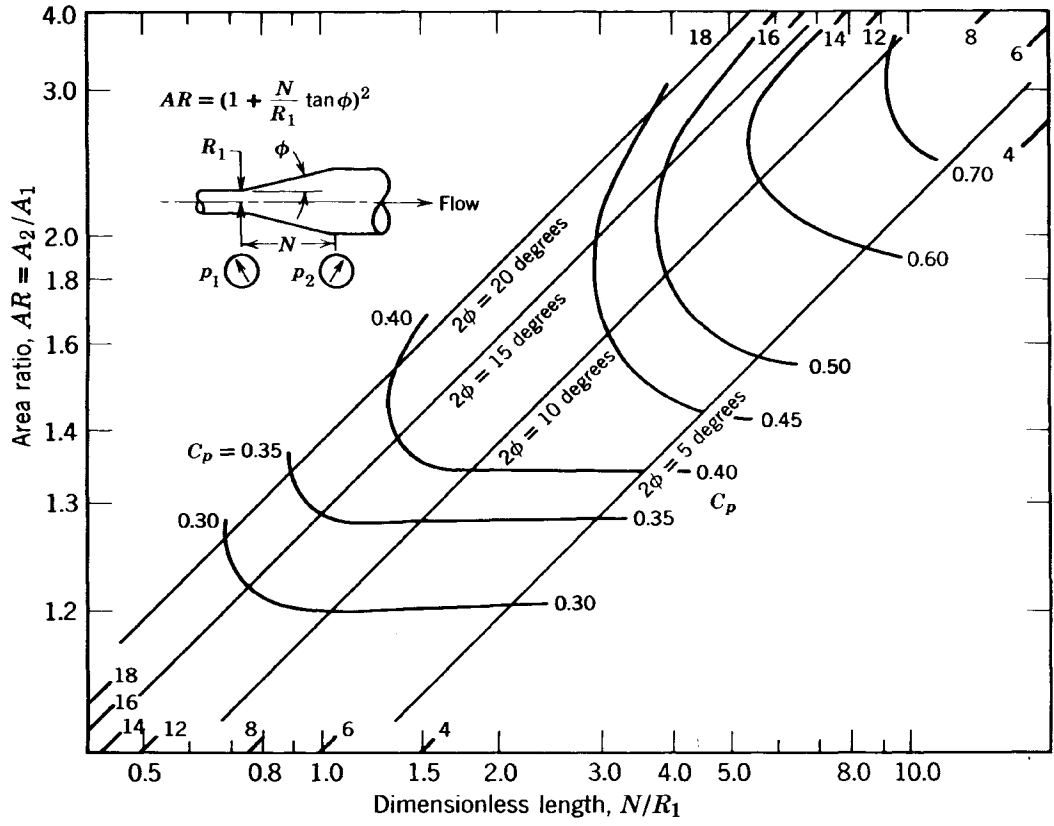


Fig. 8.18 Pressure recovery data for conical diffusers with fully developed turbulent pipe flow at inlet. (Data from [10].)

Figure 3 Diffuser pressure recovery coefficient , as presented by Fox

Also the diffuser data diagram as presented by Fox , figure 3 show this behavior: “walking along” e.g. the drawn 5 degree line gives ever increasing length- and area ratios which go hand in hand with increased pressure recovery coefficients.

The momentum equation

Since the front-end is working in a situation where some momentum must be lost due to the resistance to flow of the combustion products, it should be expected that the practical momentum at the chimney exit must be lower than that at the chimney entrance. This is why Zeuner developed the momentum equation in which the resulting vacuum (force) is the difference between entrance momentum in the exhaust steam and exit momentum in the expelled steam-gas mixture.

The next problem I address is why a front-end which uses more orifices is, if everything else remains the same, generally better-performing than one with a single nozzle.

The multi-orifice front-end

If dimensional analysis (using the Buckingham Pi-theorem) is conducted, it is possible to scale the front end, and apply for instance four half scaled chimneys with proportional

orifices. From this analysis we may expect that the flows, and consequently the resulting pressures, will generate a level of vacuum identical to that induced by the original single chimney arrangement. This behavior was demonstrated as early as 1863, by Nozo and Geoffroy, when they changed their model for one with 8 orifices and chimneys (as shown on p.229, Table A.3.1). Their original table is reproduced here.

PRESSION de la vapeur en millimètres de mercure dans le réservoir d'échappement.	DÉPRESSION en millimètres d'eau dans la boîte à fumée d'expérience.	NOMBRE de tours de l'anémomètre pendant deux minutes.	TEMPÉRATURE de l'air aspiré.	POIDS en grammes de l'air aspiré.
1° CHEMINÉE UNIQUE.				
300	141	4362	166°	384 ^g
250	115	4056	160	359
200	93	3605	160	327
150	64	3155	158	287
100	43	2459	156	217
50	28	1616	152	150
2° CHEMINÉE MULTIPLE.				
300	119	4178	177°	368 ^g
250	96	3724	174	329
200	75	3335	173	295
150	57	2778	170	249
100	40	2327	160	209
50	20	1675	157	153

As discussed on p. 234, they then equipped a full-size locomotive with 4 orifices, and ultimately tested a system of 36 units. (Keep in mind that this work was 50 years before Buckingham published his approach.)

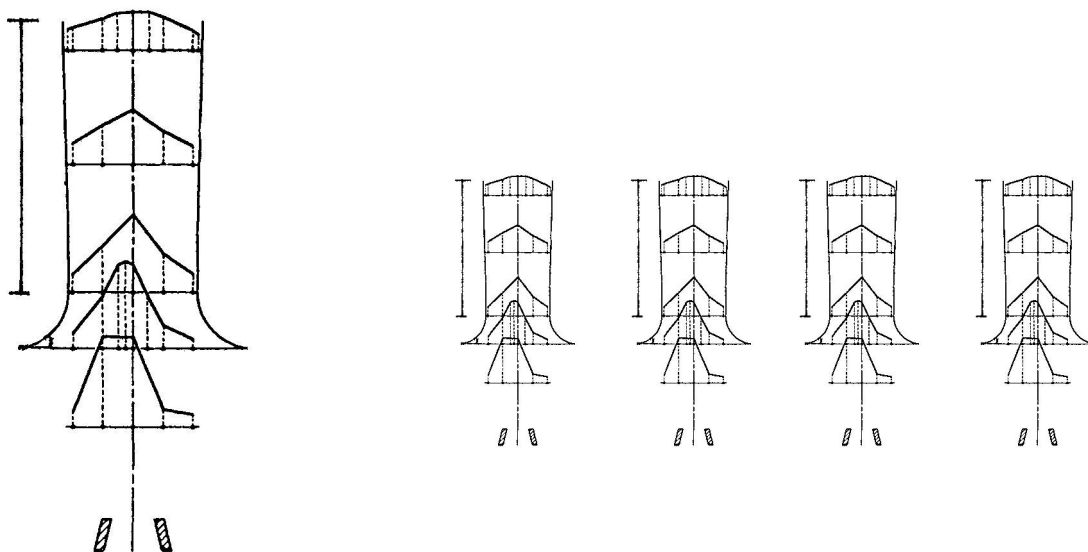


Figure 4 The single orifice chimney and its four half scale models

If as an example a single chimney is scaled into four units of half scale, each of the four scaled chimneys has the same behavior (at the smaller scale) as the single original; this is the consequence of application of rules regarding models, as formulated by Buckingham in 1913. If fed with the identical amount of exhausted steam these four chimneys process in parallel what the single chimney (of equivalent cross-sectional area) does on its own.

Having established this, however, it is clear that a real locomotive would not receive 4 'proportionally-scaled' chimneys of half length, but will keep a chimney (or chimneys) at the full height permitted by the loading gauge. Note that the continuing taper here will also give a larger diameter chimney exit for the aggregate of four chimneys lengthened to gauge height.

As a consequence the single-chimney design 'equivalent' to the four-chimney multiple would be **twice the length** of the original under discussion. That, of course, would be far too long to fit a practical loading gauge, but the effects of chimney elongation have been investigated by Troske, Goss (p.286), Young (p.420/1) and de Gruyter (p.436/9). They indicate improvements as might be expected from the above discussion on momentum redistribution and its consequence for velocity and mass distribution. For instance, Young shows in Fig. 1 (A.25.11) that the double length chimney increases air expelled from 4000 lbs to some 5200 lbs with 1600 lbs of steam exhausted.

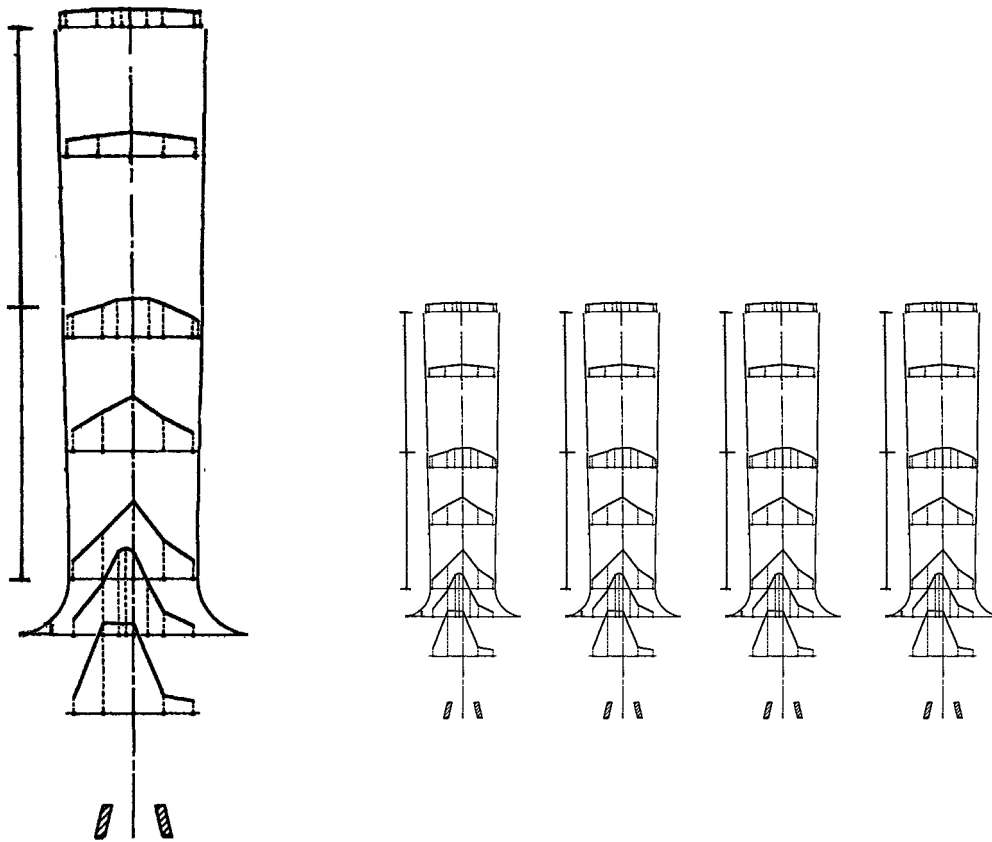
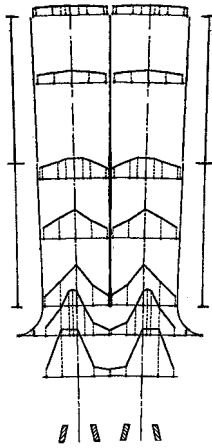
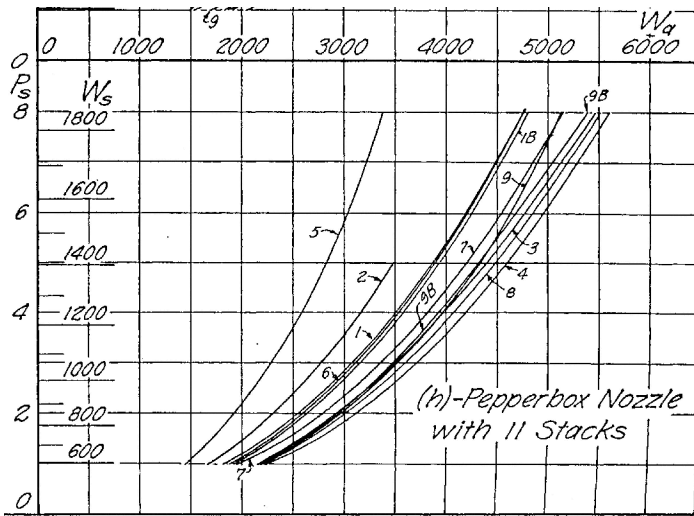


Figure 5 Double length chimney and its models with suggested improvement in behavior



We can now safely predict that there exists a four-orifice chimney that has comparable properties of the double length chimney. If we combine the four models together in a single chimney we arrive at a situation where each of the four orifices has access to $\frac{1}{4}$ of the original cross area of the chimney entrance, but the length-diameter ratio of that 'quarter part' of the chimney is double that of the original single-nozzle situation. For that reason alone we could expect improved behavior.

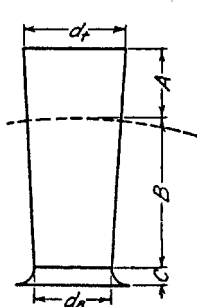
Figure 6 Four orifices combined in a chimney of original length



Young happens to have tested this configuration with four chimneys combined into one; the graph of the 4-hole "Pepperbox" orifice (A25.7, p.419, right lower corner), Figure 7, shows that with the same 1600 lb. steam mass flow, and standard chimney no. 4, around 5000 pounds of gas is expelled! It is slightly less for chimney #3 with its smaller exit diameter

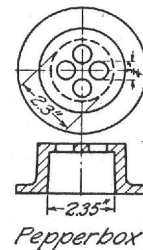
Figure 7 Pepperbox nozzle test with different chimneys, #4 is best

ILLINOIS ENGINEERING EXPERIMENT STATION



No.	A	B	C	d _f	d _o
1	4	8½	1	4½	4½
1B	4	3½	2	4½	4½
2	4	8½	1	4½	4½
3	4	8½	1	5½	4½
4	4	8½	1	5½	4½
5	4	8½	1	3½	3½
6	4	8½	1	4½	3½
7	4	8½	1	5	5
8	4	8½	1	5½	5
9	4	8½	1	6½	4½
9B	4	4½	1	6½	5

(All Dimensions in Inches)



Pepperbox orifice

Figure 8 Table of chimney dimensions, see #4

The data for chimney no. 4 can be found on p.426 (fig. 25.18), the data for the orifices tested are not in the thesis, but is shown in Figure 8. It should be noted also that the elongated chimney of 26,5 inch length as tested by Young would have an exit diameter of about 5 3/8 in, which is between that of chimney 3 and 4.

Fig. A25.12 on p.422 shows the results of the double length chimney and the chimney with “Pepperbox” orifice drawn in the same graph. It is repeated here as Figure 9. It should be clear that both systems have almost identical behavior.

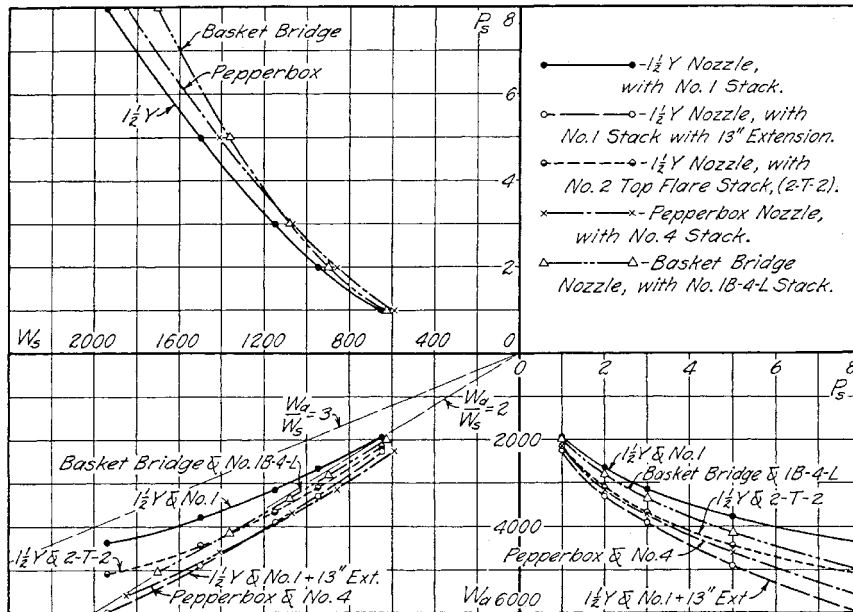


FIG. 28. COMPARISON OF VARIOUS STACK AND NOZZLE ARRANGEMENTS

Figure 9 Results of double length chimney and Pepperbox orifice with chimney #4 drawn together

Please note that the four orifices have a combined circumference of twice that of the single orifice, a total perimeter of $4\pi(d/2)$ or $2\pi d$ compared to the πd of the single orifice. Therefore we may expect the 4 orifices to show some more resistance and add to the observed back pressure. This behavior is explained on p. 149 for the more general case of n orifices. In consequence, the supposed uniform velocity profile of the jet at the exhaust must be “damaged”, giving a slightly lower momentum and in consequence a slightly lower performance.

As the principle behind a multi-orifice system is now well understood, the general behavior can be treated mathematically using the Saunders formulae (p. 463), for example to examine the consequences of a changeover from a single BR7 chimney to an 8-orifice Kylchap type on the BR 71000.

Conclusion: The relative longer chimney is the sole cause of the improved vacuum induction behavior of multi-orifice chimneys.

This explanation also indicates why the number of orifices is relevant. Increasing the number of orifices from 4 to 5 allows for an effective length of square root (5) or 224% of the 'equivalent' single chimney; this also gives an understanding why gains from further 'multiplicity' produce diminishing returns to scale.

It is beyond me why geni of steam, like Giesl-Gieslingen or Porta, did not take up and use this explanation, especially since Giesl-Gieslingen made it clear that his front-end design should be regarded as a bundle of slender individual ejectors. I believe that *any* correctly-proportioned multi-orifice ejector is acting like it is such a bundle.

The control volume

The momentum equation as applied from Zeuner onwards, and discussed on p. 124 is mostly used with an implicit control volume, taken from the orifice upwards with the chimney walls as side borders where they are present and with the chimney exit as upper border. Now, the agreed rules for control volumes state that any momentum in the direction under consideration at the control volume borders should be taken into account. As such both Giesl-Gieslingen and Porta are correct in taking the vertical component of gas momentum adjacent to the jet momentum at the orifice into account. However, more fundamentally, once the control volume has been defined from the plane of the orifices, some distance below the chimney entrance, it should be clear that attempting to take the gas momentum flowing into the chimney into account is clearly wrong as it is one of the consequences of the single source of momentum, the exhaust jet, *within the control volume as defined*. Throwing in additional momentum here would be in violation of the rules regarding considerations within control volumes.

Having said that, the case for a gas-momentum argument could be enhanced if we were to define the control volume from the smokebox bottom upwards and not allow any vertical component of gas flow into this control volume. All changes within such a control volume are then more clearly due to the momentum from the orifice jet(s). (Please note that this is considered for the sake of argument only!)

Since in fluid dynamics a control volume is chosen for mathematical convenience, all these different control volumes are technically valid ones. However, locomotives steam merrily on, blissfully unaware of the math, and the results from the momentum equations for different control volumes should be identical (within the limits of the assumptions) as there is really only one "answer", the amount of gas actually ejected.

It appears clear that the arguments for including combustion-mass momentum, including Mr. Wardale's, are generally intended to help explain the improved behavior of a multiple-orifice front end. However, as I have explained, the relative chimney length is the cause of the improved behavior. Including gas momentum, then, might be thought of as an improper correction for a wrong concept of front-end behavior. *I reject, unconditionally, any need for addition of the momentum of the surrounding gases*. The total energy or momentum in the system is limited to that of the driving exhaust jet. Period!

“Not Invented Here”, theory from other disciplines.

When I started using scaled chimneys as proof for certain arguments, I was told by my supervisors to stick to dimensional analysis, the Buckingham Pi-theorem, as used in standard textbooks used by students. To my knowledge, Buckingham is applied for the first time to front-end behavior in the thesis. Since this has turned out to explain so much, in so simple and elegant a way, I am delighted to have been steered onto this course.

Other areas of fluid dynamics were introduced in Chapter 6, including theories around jets, their shapes, and their velocity distributions. The mathematical treatment of momentum at chimney entrance and exit was part of my own experience, as is the simulation of what happens in a chimney (as shown during the York lecture). It is curious to read that some of the questions of Mr Wardale’s comments appear to center around these ‘textbook’ theories. It is precisely the distance kept by most locomotive designers from appropriate textbook theory that has made the front-end problem so misunderstood.

Dimensional analysis: the Buckingham Pi-theorem.

Mr. Wardale seems to have some difficulty with my use of the orifice-to-diameter ratio (d/D) and the Euler number (Eu , vacuum/2*blast-pressure). My use of these ratios and numbers is a consequence of the use of the Buckingham principle. This has been a useful and established approach in fluid dynamics since 1914, but Mr. Wardale does not seem to appreciate the fact that it or its results should be used and accepted.

Most of the dimensionless numbers that derive from dimensional analysis appear to have a proper use in front-end dimensioning considerations. At least the strict application of that approach frees us from the mix of ratios and dimensions that, for example, Ell used (e.g., “chimney not less than 26 in.”).

In Mr. Wardale’s comment on my use of the Pi theorem (p. 457) he brings up a number of other parameters. There is a distinction between major and minor parameters, however. I invite Mr. Wardale to write up a full contribution with definitions of Buckingham ratios for the parameters he cites, and show they are major ones.

On the orifice diameter to chimney throat diameter ratio d/D , I have observed that almost all (workable) front-ends have that ratio about the same as the ratio of steam to mixture ejected Q_0/Q . This is a practical proof of the recommendation in ESDU 85032 (the textbook on ejector design) to have almost equal Re (Reynolds number) in the orifices and in the chimney entrance.

Regarding the Eu number, Mr. Wardale questions its use. In his book “The Red Devil” [RD] he provides graphs, for example Fig. 34 on p. 129, showing the performance of the front-ends of certain locomotives with curves that indicate the relation between vacuum and blast pressure for each. I am sure that Mr. Wardale regards the locomotives that show the steepest curves as “best”. Since the *Euler* number is defined as vacuum divided by twice the blast pressure, it is a single number that represents that ‘steepness’. Does Mr. Wardale realize that he is essentially using embedded Eu numbers himself? If not, what is he trying to communicate in his graphs that is different?

Further regarding use of the *Eu* number: in the specific case of the Merchant Navy class, I concluded that the front-end of this class could be made better. Of course, it is a functional front-end as is, but it carries a (needless) price-tag in excess coal and water consumption. In Mr. Wardale's above-mentioned graph, the MN class is second worst. Now, if the corresponding vacuum and blast pressure are measured off this graph, it is easy to calculate the *Eu* number for the endpoint (the result is 0.01466). Now, remember that table 7.2 on p.144 of the thesis gives *Eu* numbers between 0.0119 and 0.0145 for the MN class locomotive. What is supposed to be the difference between my conclusion from the numbers and the conclusion logically to be drawn from Mr. Wardale's graph?

As for the use of the *Eu* number: Goss used it in his publications and Ell (p. 97) wanted "one inch water for one inch of blast pressure", very much a single *Eu* number as target!

The orifice to chimney distance: x/d

In the past a great deal of attention has been given to the position of the orifice. On p. 267 of the thesis is one of the results of Goss (1896); this is an example similar to many of the other results in showing that the induced vacuum increases with greater distance from the orifice to the chimney entrance. On p.417 are some results of the tests conducted by Young (1933), where the smallest distance from orifice to chimney shows the worst vacuum. (Young also gives results of his tests with varying orifice distance in his fig. 27, which was not shown in the thesis, and these show comparable results.) For a very long chimney, it appears that the vacuum could decrease after some point as x/d is increased; however, Young states (in the text on his p.100) that decrease of the distance from an optimal point resulted in "considerable falling off of air flow".

Since these results are of ripe old age, I decided to model a Lempor via modern CFD (Computational Fluid Dynamics) calculation, to see if the results would apply in that design as well. The Lempor is supposed to be very sensitive to radial mismatch of jet and chimney, so my first step was to determine the maximum vacuum obtained as the orifice is systematically shifted in the entry plane from left to right while centered front-to-back. Determining an optimal position, the orifice was then lowered towards a position that showed the greatest vacuum. The results are shown on the website under the label Lempor CFD: www.thefireburnsmuchbetter.nl This modeling appears to confirm the Goss/Young results. If these results can be falsified (in the Popperian sense) I would gladly accept a 'better' understanding, but I must see proof that the understanding is the true result of the theory and not mere empirical configuration.

Mr Wardale's opinion on the location of the orifice of a Lempor appears to be based not on available neutral evidence, but on his own conclusions from the Datong tests described in [RD]. There is no description of the test equipment or tabulation of measured results in that book, or in any other location I have been able to find. As a result, no one is (or can be) in a position to independently reproduce his methodology and experiments and thereby verify or falsify the conclusions he has reached. Since the

conclusions as he presents them violate generally-accepted prior knowledge, they may represent a legitimate break-through ... but so far there is no published or independently-verifiable explanation why a Lempor chimney system would behave differently from any other arrangement. I feel that this situation should be addressed as rapidly as possible, ideally by providing statistically-adequate raw data as well as the detailed experimental protocols and methodology used to obtain those data.

Of course, if the longest possible chimney is wished for, it might prove to be advantageous to position the orifice above the plane of the chimney entrance.

Finally, I cannot really imagine why, if all evidence points to a diminishing degree of vacuum as x/d decreases, there would be a sudden surge in vacuum induction with the orifice in the chimney entrance. Would that not have been observed at least at some point in 200 years of front-end designing, with documented examples in, for example, Sinclair, "Development of the Modern Locomotive Engine" having been empirically tried?

Textbook theory

With respect to the treatment of the jet, Mr. Wardale questions the use of my equation 6.6.1 ($Q/Q_0 = 0.32 x/d$). This equation is intended to give an expression for the increase in mass ratio of a jet with distance from the orifice. The (empirical) value of 0.32 has been gradually defined as a result of testing performed since the 1930s. It should be obvious that the equation is valid only from about 6 orifice diameters' distance between chimney and orifice — that is so obvious, in fact, that it was not even mentioned in the original text. The reason is simple, and explained in my text: the core of the jet contains fluid at the original orifice velocity up to that six-diameter distance from the orifice. We find in the literature that properly-designed locomotive exhaust nozzles are almost always positioned at some 6 orifice diameters down from the chimney throat. As a result, Mr Wardale's questioning the efficacy of this formula does not add much understanding or help to what was previously known.

Rajaratnam's book (referenced in the thesis) contains a chapter on coflowing primary and secondary flows.

Momentum Theory

As stated earlier, the momentum equation should not contain any added factor for momentum of the induced mass flow. However the momentum from the chimney exit cannot be precisely determined unless measured, so it remains mostly guesswork in mathematical models. It should be possible to quantify the momenta at different points in the front-end further by applying CFD.

Mr. Wardale makes reference to the paper by Kentfield and Barnes on ejector design. Like ESDU 85032, this addresses the general case where the secondary fluid is already under pressure or has an established velocity *induced by an exogenous cause*. Under these circumstances, momentum should be included in any ejector calculation. Please note that in the first part of this paper the authors state the assumption that the exit

velocity of the ejector is regarded as zero, and also that at the end of the paper it is mentioned that the proper position for an orifice appears to be just below the entrance.

The Giesl ejector

Mr. Wardale, in discussing the SAR Giesl ejector ([RD], p. 11), mentions ‘test 1’ which showed that the tested Giesl’s steam-raising ability “at higher speeds and heavier gradients ... became worse”. ‘Test 3’ indicated that when using the Giesl it proved impossible to maintain steam pressure at lower speeds. (This is the only mention of a Giesl test with bad performance at lower outputs.) On [RD] p. 14 Mr. Wardale generalizes this test to all Giesl tests conducted by SAR staff, but this assumption does not appear to be supported by any data in that book. Shall we conclude by stating that from Mr. Wardale’s observations, Prof. Dr. A. Giesl-Gieslingen was unable to calculate a properly-proportioned Giesl ejector for the SAR 25NC class?

Mr. Wardale comments “For a Giesl ejector at least some gas momentum should be added as an input.” Through the kind services of Dr Pawson I am in possession of the results of 31 of the BR Rugby tests (those numbered 2147 to 2248) of class 9F 92250 as equipped with the Giesl ejector. I myself have inspected the whole archive box containing the test data and further correspondence in it, and it is clear that Prof. Dr Giesl-Gieslingen was satisfied with the quality of the tests. However, no matter what validatable calculation I use, I always find the result indicating there should be a larger amount of combustion gases ejected than were actually measured at Rugby. (I would add here that making an allowance for momentum following Mr. Wardale’s expressed preference only makes the calculated results worse!) As far as I am concerned the Rugby test results can, and should, be regarded as hard proof that an assumption of additional secondary-flow momentum is superfluous.

The Lempor

In general, I think that Mr. Wardale’s comments are a bit too much directed toward the application of the Lempor. In fact, he uses the word “Lempor” 66 times in his 14-page comment while it only appears 29 times in total in the whole thesis of 483 pages.

One of the remaining problems with multi-orifice systems is that their improvement cannot, yet, be quantified with certainty. Any strictly energy- or momentum-based calculation does not (and effectively cannot) take chimney length into account, and as a consequence such a calculation for a single vs. multiple system would produce identical results - in obvious difference from what is observed in real-world systems. In the Porta equation this difficulty is “solved” by fudging in some momentum from the secondary flow, but this is neither a particularly scientific approach nor a particularly correct one. In my opinion, this could be best regarded as a case of an erroneous theory giving proper results... but only when empirically modified using the result of experience.

In the thesis I attack the length problem by recognizing that a chimney-length based addition for the momentum equation can (and in my opinion should) be used. For this I used the experimental data of Fox for diffusers (pp.145-150). The pressure-recovery

coefficient in the graph shown there is a function of the exit/entrance ratio and the length/entry-radius ratio of the diffuser and chimney. Consider a chimney of double length moves with its performance along, say, the 5-degree-diffuser-taper line. If its length ratio is 3.6 it will become 7.2 and its pressure recovery coefficient C_p will change from 0.4 to about 0.56. As a consequence this approach may fully account for the consequences of the number of orifices and dimensional ratios in the chimney system. In particular, equation 7.7.5.12 on p. 150 shows this aspect, and the use of this approach with the SAR 26 Lempor is indicated in the first paragraph of p. 151.

Now, this is one of the principal issues under discussion in the thesis. However, given Mr. Wardale's comment regarding my p. 182 that the situation "suggests that other factors are at work," it appears that he has missed this point. Alas.

The limits of formulae

Mr Wardale criticizes both the use of the Q/Q0 formula (p.120) and the Saunders formulation for a tapered chimney, p. 464, by inserting limiting values in the formulae. But what will happen with the Porta momentum equation if such an operation is applied?

Taking the primary velocity, that of the exhaust flow, to zero, will the equation develop into a "perpetuum mobile" with the secondary flow driving the system? Or is it more logical to assume that the velocity of secondary flow of the combustion products is a function of the velocity of the exhaust jet?

The following points need separate addressing:

Page 55, 3rd par. Chimney : orifice area ratio is confused with chimney : orifice diameter ratio here. The latter is typically $\approx 3 : 1$ in modern exhausts (see Table 7.1 and item 7.5.1, page 131) giving an area ratio of $9 : 1$, i.e. over twice Clark's ratio, not less than it.

Discussed is an orifice to chimney area, not the other way round; as such it is 1:4.4, larger than 1:9.

Page 110, item 6.3. The data given is over-simplistic. For example, it makes no mention of smokebox resistance, which can be very high in the case of spark arresting and self-cleaning smokeboxes, nor does it acknowledge the high resistance which can occur in the firebox due to turbulent mixing of combustion gas and secondary air (e.g. see [RD] Figs. 106 & 107). Overall boiler gas flow resistance for a modern high-resistance boiler is better computed in the manner undertaken for the 5AT.

The calculation of the boiler resistance of the BR5 and BR7 is only used as an example that shows that such a calculation can be verified against a test. This kind of simple calculation would probably be useful only in the case of an oil-burning locomotive. I agree that the FDC methods would be better for detail design.

Page 112, top table. Taking tube bundle cross sectional area as the sole criterion for tube draught loss is inadequate: the equivalent diameter of the boiler tubes must be factored in. In the case of flue tubes this is heavily influenced by superheater element design, which in turn has a major influence on tube gas flow resistance .

The boilers of both the BR5 and BR7 were designed with the Wagner criterion of almost equal resistance of flues and tubes in mind. Therefore, in this case, we may legitimately use the figure for tubes only.

I included the flue resistance calculation as part of the same spreadsheet since it was also in the Barske approach.

Page 116, 5th par. ‘... the assumption of a uniform velocity profile for a jet entering a chimney is not supported.’ What about the case of Lempor-type exhausts where the exhaust steam jet exits the blast nozzle within the mixing chamber?.....

It must be clear that the Trüpel tests are meant for a free jet such as the vast majority of locomotives have had.

Page 117, Fig. 6.5. Does this apply to a free or confined jet (or both)?

Tollmien calculated for a free jet.

Page 120, item 6.6. Note that as the whole of a Lempor type exhaust is a confined jet (as defined by Fig. A.29.2 on page 454), all contents of this section referring to a free jet are inapplicable to it. That ‘about half the available smokebox gas is entrained in the free jet’ in a conventional exhaust is no indication that it would not be entrained more efficiently in a mixing chamber. Equation 6.6.1: using Koopmans’ notation on page 107, as the equation is written, as $x \rightarrow \text{zero}$ so $Q / Q_o \rightarrow \text{zero}$. But $Q = \text{total mixture mass flux (i.e. exhaust steam + combustion gas)}$ so in any ejector as $x \rightarrow \text{zero}$ so $Q \rightarrow Q_o$ and $Q / Q_o \rightarrow 1$. Equation 6.6.1 as given, with no caveats, is therefore wrong. It may only be valid for values of x giving $Q / Q_o \geq 1$, meaning entrainment only starts at $x = 3.125d$, surely worse than with confined entrainment in a mixing chamber. If equation 6.6.1. is incorrect, all deductions from it may also be wrong.

This textbook formula has been determined from the results of years of experiment showing linearity some distance away from the orifice. The formula is not meant to be used inside the flow development space or where its result is meaningless. For obvious reasons we are interested in its outcome near the chimney entrance for which $x = 6d$ is used.

Please note that this text is part of a Ph.D. thesis, *it has had peer review from experts in contemporary fluid dynamics!*

Page 124, par. starting ‘For practical use ..’ If the control volume and its planes are as stated, the assumption of zero gas input momentum in the vertical direction is false for a Lempor type exhaust, where the steam and gas flows are essentially parallel at the nozzle exit plane. Section 7.1. therefore applies only to a free jet type of exhaust (the type for which conservation of momentum is said to apply). The question of the inclusion of input gas k.e. / momentum is considered later.

Chapter 7 deals with theoretical approaches from other branches of science, and is mainly directed at the majority of exhaust systems. It does not concern systems such as the Lempor.

Page 131, Table 7.1. If, as appears, Q_m / Q_c is a measure of the accuracy of calculation of exhaust performance using the actual exhaust dimensions and the momentum equation, the correlation is patchy (up to 33% error). Note that the D / d ratio for the 9F Giesl is the lowest of all, and by the 1st par of page 132 should be inadequate (i.e. D / d should be \geq about 2.8). However the Giesl ejector gave superior performance to the standard 9F exhausts with larger D / d ratios (see Fig. 5.5 page 93). This points to there being other factors at work besides those considered by Koopmans, which may well include the value of accelerating the combustion gas before parallel mixing. As a general point, one may question the whole analysis as BR exhausts had quite mediocre performance.

The approach I used in chapter 7.5, with a “factor analysis,” is meant to see whether standard dimensionless numbers can provide some insight in comparable front-end behavior of a group of different locomotives, even with insufficient technical data. The relative ‘worth’ of BR exhaust-system design is not fully relevant to that issue.

Page 131, item 7.5.1. The first equation ($Q_1 w_1 = Q_2 w_2$) is not valid for a Lempor type exhaust with a cylindrical mixing chamber as (i) there is not simple conservation of momentum in a confined jet (see page 121 item 6.6.2.) and (ii) the input gas momentum must be added to the l.h.s. of the equation. The conclusions of item 7.5.1. regarding the D / d ratio are therefore also not applicable to a Lempor-type exhaust.

The analysis given in that equation was never meant for a Lempor type (see above).

Pages 135 – 136, item 7.5.7. The important influencing parameter in the pulsating locomotive exhaust may not be so much the frequency of the exhaust beats as the varying intensity of the flow during each beat (this is hinted at in the 3rd par. on page 136, which tends to contradict the previous paragraph). Consider two locomotives of the same class, operating at the same speed and using the same amount of steam. One has even beats, one is ‘off-beat’. The time-average exhaust steam pressure of both will be the same, as will the frequency of exhaust beats, but, other things being equal, experience tells that the off-beat engine will steam better. This shows that a factor is at work which Koopmans has not considered, and it is probably the fact that the k.e. and momentum of the flow during each beat continuously vary from release to the end of the stroke and during the exhaust stroke. The mass-average values of all flow

parameters will be different from the time-average ones which are measured. Most exhaust steam flow is at release at a much higher exhaust pressure than that shown by the exhaust line on the indicator diagram (which is what counts for cylinder efficiency), and correspondingly higher k.e. and momentum. This increase of mass-average over time-average is presumably intensified by an off-beat engine, which is why it steams better. Thus it is a fallacy to consider pulsations on the basis of frequency alone and ignore the variable intensity of the pulsating flow, and until the latter is incorporated into theory, there has been no real advance in the question of pulsating flow. The analysis of item 7.5.7 is therefore not considered to be significant. Note that de Laval (supersonic) nozzles make the most use of the pulsating phenomenon (see below).

A negative outcome of an analysis is also a valid result. Should it have been left out?

Please note that others factors regarding exhaust beat frequency and peaks are mentioned in the text, p.136. For instance the different orifice sizes of two and three cylinder locomotives are not fully explainable. I might add here that this is also true for non standard crank settings as the “Lord Nelson” class of locomotives used.

Since the thesis was published, I have acquired a copy of the paper “The Locomotive Blast-pipe and Chimney” by W.F. McDermid in the “Journal of the Institution of Locomotive Engineers” 1932 p.397. In that paper, McDermid concludes that the intermittent action “increases the blast effect by about 10 per cent”. I regret not having seen this before.

Page 138, 4th and 5th pars. It appears that the function of a converging-diverging nozzle has not been understood. It is a ‘diffusing’ nozzle only at less than critical pressure ratio, above this ratio it expands the steam to higher (supersonic) velocity than is possible in a plain converging nozzle. As explained above, most steam flows during release, at relatively high pressure, so although the time-average exhaust steam pressure would normally give a pressure ratio across the nozzle $<$ critical, the mass-average flow is normally at $>$ critical pressure ratio and needs a de Laval nozzle to get its full k.e. and momentum potential. Such a nozzle does not therefore cause a ‘lower orifice velocity’, exactly the opposite during release, i.e. for most of the exhaust steam flow. The ‘curious observation’ Koopmans notes from the Datong tests is due to the fact that whatever the nozzle area ratio may be it is ‘right’ only for one particular pressure ratio, yet the pressure ratio of the steam flowing through it is continuously varying, therefore each nozzle tested was only ‘right’ for a fraction of the total flow, and clearly this fraction did not change significantly for the various nozzles tested, thus giving similar nozzle performances. The question of de Laval blast nozzles is explained more fully in [RD] pages 96 – 97.

I do understand the functioning of de Laval type orifices. However, there is simply not enough information available for a proper discussion. The above explanation Mr. Wardale gives for time-average and mass-average flows and pressures would need additional proof with measured data.

Page 138, penultimate par. The admission of ‘a large “shearing stress” between the large velocity jet and surrounding stagnant gas mass’ seems an acknowledgement of the mixing ‘shock’ loss which is elsewhere dismissed (e.g. see page 116, penultimate par. of item 6.5.2) A stress must be due to a force, and as the jet is moving this must give rise to a power requirement (= force x velocity). This power is certainly used to give momentum and k.e. to the gas, but no thermodynamic process has 100% efficiency, so there must be a (mixing ‘shock’) loss here.

That may be so, but the leading phenomenon is “conservation of momentum”, be it in translation, rotation, or mixing. Since velocity distributions show a very gradual and smooth decrease to the low velocity at the jet boundary, doubt can be raised about the presence of a ‘shock’ phenomenon.

For example, in the Trüpel case (p.116) it could be shown that the measured momentum diminished by 8% over a distance of 1200 mm from the 90 mm orifice. At 6 orifice diameters, the calculated trendline shows the translation momentum to be diminished to 98.8% of the orifice value — this with all the measurement and numerical integration errors included.

Page 144, penultimate par. ‘A D/d ratio of under 3 ... appears to lower the general level of the calculated theoretical Eu .’ But the Giesl 9F D/d is only 2.23 : 1 (Table 7.1.) and its measured and calculated Eu numbers (said to be a measure of its actual performance and its maximum potential performance respectively) are the best of all the exhausts considered in Table 7.2. A factor in this is that the input gas momentum is ignored in item 7.5.1. explaining the significance of the D/d ratio, but at least some of it must be included for a Giesl ejector (and all in the Lempor). The better performance of the Giesl ejector (or similar exhaust) than might be predicted by the D/d ratio is because there is this added input momentum in the form of accelerated combustion gas. This would seem to negate the D/d ratio is a definite yardstick of performance for all such exhausts.

Why would the performance of a non-standard exhaust like the Giesl negate the results for the vast majority of conventional exhausts? More proper and applicable comparisons are discussed in the thesis. I concur that the D/d ratio is not a ‘definite yardstick’ of performance for all front end designs.

Page 145, 5th par. This paragraph appears completely at odds with Table 7.2. which shows the highest Eu to be for the Giesl BR 9F. Although the SR Merchant Navy shows up worst in the Euler number analysis, this class had an excellent reputation for steaming, both on test and in service and up to what were, by UK standards, high evaporations, which, frankly, could not have been realised with a poor exhaust. This again casts doubts on the validity of the Euler number as a definitive exhaust criterion.

Yes, this was the result of an unnoticed typo creeping in. The data for the 5.125 in. orifice, line 11 of table 7.2, should have been mentioned for the BR5, (p. 145 5th par).

With respect to the Merchant Navy class: of course it had a workable front-end. However, from the point of view of this thesis, the detail design of that front end carried its own high price tag. For an evaporation area of 2450 sq.ft the MN had 5 orifices of 2 5/8 in. while the LNER A4 and BR8 with slightly different areas have two 5 1/2 in. orifices each. As a consequence the MN class had to cope with a larger back pressure, diminished power and increased water and coal consumption over the whole of its working life

Page 149, penultimate par. of item 7.7.4. Even with the blast discharge at the inlet of the mixing chamber, the Datong tests indeed showed the advantage of multiple nozzles, allowing a shorter mixing chamber. This is confirmed by implication in Kentfield and Barnes, The Prediction of the Optimum Performance of Ejectors, I. Mech. E. 1972. The number of nozzles is therefore not ‘almost irrelevant’ in the Lempor, and to link the number of nozzles solely to the orifice – throat distance has to be over-simplistic.

This chapter discussed the stimulating of developed inlet flow by using more orifices. Since Lempors appear to have almost universally 4 orifices, what is the argument?

Please note here that Kentfield and Barnes remarked (p.679) “..the best performance was obtained with the primary nozzle withdrawn slightly from the entrance plane of the mixing tube.”

As I regard a multiple-orifice front-end system as a combination of single chimneys, the distance to the chimney is related to the orifice diameter, $x/d = 6$, which would be different for a 4 or 5 orifice system based on identical performance.

Page 152. The data used for the 26 Class is mutually incompatible:

Apparently I did not succeed in collecting proper data from the contents of [RD].

Let it suffice to state that, recalculating with the given numbers (28350 kg of steam and 55224 kg gas), for a vacuum of 5500 Pa an *Eu*-number could be calculated of 0.0399, and a theoretical maximum gas amount of 31500 kg/hr. The calculated blast pressure is 69135 Pa, proper for de Laval orifices.

The calculation could be refined of course but the result would be comparatively slight, and I doubt would show any degeneration from the results of my analysis. The SAR 26 front-end works properly.

Page 153, last par. of item 7.9.2. Diffuser angle is 12° for a short diffuser (see earlier comments on page 100, 2nd par.). Orifices were angled to get more uniform mixture velocity at the diffuser entrance to improve diffuser efficiency (see last par. on page 146) and especially to avoid the problem Koopmans gives in the 1st par. of page 146. Koopmans claims the orifice inclination appears too large and doubts that ‘this

[inclination] would give a sufficiently developed flow' at the diffuser entrance, but he does not say what to do to improve matters (note that the importance of placing and/or angling multiple nozzles to increase mixture velocity near chimney walls is acknowledged by Koopmans on page 443).

On the 'best' inclination of the orifices, we can only exchange opinions. Proper test results with this being the only variable were, and are, not available to me.

My opinion is that the orifices should cover their part of the chimney by being directed at the centres of the inscribed circles of throat and exit. This has an additional consequence in avoiding the necessity of making a correction on the momentum because the flow from inclined orifices is not strictly in the proper direction for a momentum calculation.

Page 153, 1st par. of item 7.9.3. If the draught of the MN class was 'good' and the combustion 'never very good' then one must look elsewhere for the problem. 'Over-draughting' can produce combustion problems of its own, and one can look, for example, to over-rapid release (itself a sign of a free exhaust) and to firebar design as important factors. This is well covered in chapter 2. 3. of [RD], and the first application of the Giesl ejector to the Chinese QJ Class was reported as giving very poor combustion because the rapid release tore the fire.

'.... look elsewhere for the problem...'

Yes, but what we're actually 'looking for' would be a proper understanding of the working and dimensioning of a front-end. The MN class seems to work on the principle that all the combustion products are entrained because there is a large distance from orifice to chimney. The chimney cannot really contribute anything since there is no further meaningful 'entrainment' remaining to be done, and it probably acts, substantially, only as a kind of valve against atmospheric 'leakage.'

Pages 155 – 156, section 7.9.4. (see also comments on pages 461 – 464). Koopmans criticises the 5AT exhaust but there are a number of erroneous assumptions and conclusions here:

I would like to leave the cause of the 5AT alone. I can understand the arguments.

Let it suffice to state that I found, sometime in 2005, that orifices of 50.7 mm were proposed. I calculated that they should be 47.25 mm, providing the methodology and calculations I used in the thesis and notified you. In early January of 2006 I read the presentation the 5AT group had made to the Derby Engineering Society (DRES), in which orifices of 47 mm were specifically mentioned.

If a comparative analysis is desired, the proper data should be supplied. My days of data-mining in websites and literature have been over for some time.

Page 178, 1st sentence. This is not true if, as it should be, the total (Pitot tube) exhaust steam pressure is taken, see [RD] footnote on page 128.

Yes, but only static pressure was measured, one pressure take-off mounted in the side of the blastpipe.

Page 178, 2 bulleted items at bottom of page. (1) This statement is only valid for an exhaust system that fully complies with the momentum treatment given in section 7.5.1. Not all exhausts do. (2) Objection is raised to the Q/Q_o equation... see comments on page 120.

You are entitled to your objections, but since this was carefully peer-reviewed at the Sheffield, I feel myself in pretty good company for its use as appropriate to purpose.

Page 180, Tables 8.3. and 8.4. The data of these tables does not seem credible – the very large discrepancies between the calculated and measured exhaust steam pressures means that one (or both) of the respective figures must be wrong. Note that the calculated figure is always the higher, which is opposite to the case of the 26 Class exhaust given earlier (see comments on page 152). No mention is made of excess air – it is clear from the data that there must be excessive excess air with the smaller orifices.

The tables do show the difference between the calculated overall pressure and the measured static one which is of course lower. A proper measurement would need a Pitot arrangement in the blastpipe to cope for the velocity pressure. Given the amount of cylinder oil in the exhaust steam I expected fouling of the Pitot velocity pressure tube right away, making results of the measurements of doubtful quality. I stuck to static measurements only, like the BR tests used (and they were rightfully criticized for doing so!).

I am not aware of measured Class 26 exhaust pressures.

As the locomotive was not equipped for measurements of amounts of air, a standard 20% excess was estimated. You may be quite right on the excessive air.

Page 182, Momentum. States that momentum from single and multiple orifices is approximately the same. But we have ample evidence of the superiority of multiple orifices (page 182, last par., page 184 item 9.1.3, last par.) which (again) suggests that other factors are at work. There does not seem to be anything in the momentum (or energy) approaches to exhaust design that can account for the improved results with multiple orifices, rather more rapid ‘mixing’ (momentum and energy transfer) is probably the key. How can the better performance of multiple compared to single nozzles be squared with the statement that ‘No specific addition to the [Zeuner single orifice] theory for multiple orifices is needed’ (page 182, 4th par.)?

It appears that the explanation I gave on page 149 of the thesis is being neglected.

The chimney of a multiple orifice unit is relatively longer, *effectively*, than that for a single orifice, for the reasons I gave above. The development of the velocity profile towards ‘flatness’ at the chimney exit is enhanced by this characteristic.

Page 183, last par. ‘The velocity of smoke gas should be disregarded if the orifice is at a distance from the chimney’ (my italics). But if it is not, as in the Lempor, then it must be included (this is confirmed in the 1st sentence on page 100). This adds significant k.e. and momentum input to the exhaust and is important in view of its non-inclusion in Koopmans theory, see later.

Yes, it is the consequence of having the control volume starting on the orifice plane. See the comments I made earlier, where I explain why I reject the inclusion of gas momentum in ejection performance.

Page 184, ‘The single orifice ...’ ‘There is little wrong with it ...’ This ignores the fact that with larger locomotives it simply isn’t possible to maintain good ratios with a single exhaust because of the height limitation. This is what is wrong with it. And if, in the previous paragraph, it is stated that with four orifices it is ‘possible to use a far larger orifice area than if one uses a single orifice’, how can it now be stated that ‘There is little wrong’ with the single orifice?

The sentence Mr. Wardale quotes continues, however, with: “.... as long as proper ratios according to Chapter 7 are maintained.”

If the loading gauge prevents proper ratios for a single-orifice design, I firmly believe other means, like double chimneys or multiple orifices, should be used.

Page 185, Lemaitre. Should be ‘converging – diverging’ orifices, not v.v.

Looking at figure 4.6 on page 87 or fig. A.27.4 on page 422, my view is that they are first diverging or parallel, then converging in the direction of the exhaust flow. I think we may be looking at different areas of the flow path in this front-end design.

Page 256, item A.9.2.1., 1st par. (Von Borries). Although this is in \diamond marks (i.e. paraphrased) it can scarcely be believed that it is what was originally written. It would have us believe that combustion gas rising from the front of the fire crosses over that from the back inside the firebox to exit via the upper tubes, whilst that from the back exits from the lower tubes. (!!!)

Here is the original German text, so that any confusion can be eliminated: “...Steht das Blasrohr zu hoch, so ziehen die Feuergase vorwiegend durch den oberen Feuerrohre, das Feuer liegt auf dem hintern Theile des Rostes todt “

and

“...Steht das Blasrohr zu tief, so ziehen die Gase vorwiegend durch den untern Rohre, das Feuer brennt vorwiegend auf dem hintern Theile des Rostes”

That is what was written.

Page 289. Equation for ‘b’: b is not given in the notation, page 288: what is it? This is

a common fault repeated throughout the thesis, i.e. symbols are introduced in the text without appearing first in the notation or, as here, being defined at all. For example, what are 'a', 'm', and 'R' on page 316 (they are not given in the notation, page 301)? The 'burying' of symbols in the text (even if this is done at all) makes it difficult to follow equations. Other instances of this are not commented on further.

A number of these older texts do not have a list of symbols, they are used embedded, defined only when first used, and not always that easy to catch.

In this case, b is the width of the flare, (to which the text points in fig. A14.2 on the next page, 290). In Strahl's text a and m appear to be coefficients for which the value is determined later in the text. R stands for Rost, grate area. I am sorry for any confusion ... I assure you it was unintentional!

*Page 334, penultimate par. 'The introduction of the velocity of the gas at the chimney entrance ... should be regarded as incorrect.'....
...remains constant by the Bernoulli equation.*

This has been treated in the separate chapter on that topic.

If the control volume is established from the orifice upwards, it is simply not allowed, by the rules applicable to control volumes, to introduce the chimney entrance flow as a separate entity. I do not think I need to repeat why that is so. This is an elementary characteristic of any properly-conceived control volume.

Since the Bernoulli equation is only about pressure and velocity pressure, other physical variables such as densities and temperatures must remain constant or assumed to be steady-state. I have great reservations on the use of an unmodified Bernoulli equation to characterize the region just outside the orifice, given the temperature and density differences there.

Page 374. item A.21.14, point 2 (Chapelon). This doesn't make sense as written: presumably it should read 'Thanks to the decrease in back pressure the boiler needs to produce less steam,'

Correct. My text was mangled.

Chapelon indicates that either the boiler can produce more steam or the combustion is improved.

Page 403, item A.25.3.2, page 405 last par. (Young) and page 429 top par. (De Gruyter). The previous remarks in connection with pages 135-136 apply. As the experimenters are adamant that draught production is essentially the same with pulsating as with steady flow, and as the practical experience on actual locomotives points to it being otherwise, one may have to look at the effects of pulsating draught on heat transfer and combustion, rather than at the effect of a pulsating exhaust on draught.

Young must have repeated Goss' results, perhaps without comment. As far as de Gruyter was concerned, he repeated comments made by others without further 'scientific' or mathematical analysis. He was a true railway engineer who performed his improvements with real locomotives.

Page 434, item A.26.2.6, point 2, (De Gruyter). On what basis are the lines of Fig. A.26.8 extrapolated beyond the test results? That a chimney may have an optimum length, beyond which efficiency decreases, would appear to be true only for very long chimneys (outside the range possible in locomotives) where frictional effects may outweigh other advantages from long length.

It is not a full extrapolation, but a way of expressing that the trend continues further on. Please note that this is a Dutch convention! Graphic conventions for showing trends may be radically different in other countries!

Page 443, 2nd, 6th and 7th pars. Supports aiming jets at periphery of mixture to even out its velocity profile, and suggests exhaust performance is quite sensitive to jet position and aim.

In this case that is probably an incorrect conclusion, as the Lemaître used a central orifice that could be closed. With this orifice closed, the jets would then aim at the remainder of the area now not being 'serviced' by the central orifice. Redrawing showed the inclined orifices to be on a circle of 270 mm and inclined 5.2 degrees each.

Using the same chimney I would position orifices on a circle of 220 mm and an inclination of 2.4 degree each, without central orifice. I do not make any claim on 'perfection' for that layout.

Page 444, 1st par. 'It appears only to be available as an internet document'. The present writer, and others, use copies of Porta's original hand-written document. Strangely Koopmans has not included in his thesis the much more comprehensive 1957 paper by Porta and C. S. Taladriz to the 9th Pan American Railway Congress entitled 'The Exhaust of Locomotives', which deals with both the Kylpor and Lempor exhausts. The 1974 Lempor work is merely 'a revision' (Porta's own description) of this earlier work, which is in the public domain.

I do, in fact, reference that paper (on p.105).

I have never been able to get an actual copy; it is not really in the public domain. More recently, the PARC office in Buenos Aires did not respond to my request for a copy. If you have a source for a copy, please provide it!

Page 448, Koopmans' note in small type under equation (A28.9). The only difference between equations A28.8 (Porta) and A28.8a (Koopmans) is in the final term. $(1 - \xi_b) = 0.96$ (Porta) and $1 / (1 + \xi_b) = 0.962$ (Koopmans), an 'error' well within the accuracy of the calculations as a whole and therefore justifying Porta's simplification on page 447 that $(1 + \xi_b) \approx 1$ for the purpose of these calculations.

Correct, my comment is more directed at the fact of the simplification, and its later reintroduction.

Page 449, Koopmans' note in small type. Kentfield and Barnes, The Prediction of the Optimum Performance of Ejectors, I. Mech. E. 1972, confirms diffuser efficiency can be as high as 0.8 (for smooth, accurate diffusers). In addition it may be expected to be high in a locomotive exhaust because of the limited length.

Yes, but Porta was discussing an area ratio of about 2 for which the calculated efficiency would be about 73.1%, as noted in the text. It appears that below area ratios of 1.5, and the accompanying limited length, diffuser efficiencies of 80% occur.

Pages 456 – 458. There are significantly more independent variables affecting exhaust performance than listed here,

Yes, but there must be major and minor ones among them. Moreover the variables should be expressed as dimensionless numbers for proper comparison.

I would note that Mr. Wardale submits no proof of these 'significant' additional variables, and does not supply any additional numbers or data for what some of them might be. I do not mean this as sharp criticism, only that as full a list as possible ought to have been provided as part of that comment.

Page 464, point 1. The admission for the 'ideal p model' that when $R_c = 0$ (i.e. when chimney exit area is a maximum) E_u is not a maximum is an indication that the pressure distribution model is wrong, because if $R_c = 0$ diffusing is a maximum and for a given input momentum flux the vacuum created should then be a maximum.

The text itself already states that Mr Saunders had some reservations. I doubt whether it is useful to consider such an extreme, since most mathematical models are meant for a limited working range only.

Page 468 – 469. The data used for this appendix is mutually incompatible, see comments on page 152. Any 'fit' of practical and theoretical results is therefore suspect. The chimney throat diameter (item 3 of top table, page 468) is 422 mm, not 211 mm.

The calculation is redone, and has been discussed earlier.

The 211 mm throat dimension is the consequence of the calculation based on the "bundle of scaled double length ejectors" approach, this with the data for the diffuser in mind.

Page 477, 4th par. Goodfellow projections, or indeed any type of (invariably blunt) tip or projection into the blast nozzle, are to be avoided because of the high increase in exhaust steam pressure they cause, this in turn because of the reduced flow coefficient of a nozzle with such projections (net flow area is then significantly < nominal nozzle cross-sectional area). Such tips were used in practice on the SAR on exhausts of large diameter but limited height because to get 'sealing' at the chimney choke with limited

nozzle – choke axial distance and a presumed steam blast angle of spread the blast nozzle diameter had to be quite large, therefore with such a nozzle, tips (or indeed sometimes a full cross) were used to reduce the nozzle area to that required to give the necessary blast velocity.

Yes, correct, this was independently tested and presented at the York symposium of December 2006.

Conclusion:

In conclusion, it may very well be that, as Mr. Wardale states in his ‘summary 3’, “no clear advance over existing knowledge nor new insight can be found in this work”. I reproach myself, perhaps, for having evidently been so unclear that Mr. Wardale could not find it.

I might add that Mr. Wardale seems not to recognize or acknowledge the significance of putting all the historic elements of front-end design, including both the textbook-theory ingredients and the discussion of a proper recipe for the calculation of a diffuser type chimney system, ‘on the table’ in one place for the first time.

Regarding Mr. Wardale’s ‘Recommendation’, I think I have made my position clear in the discussion of the 5AT calculation I provided in the thesis. I am not really interested in waiting years or decades for a full test of such a system, and in the years since I published the thesis such a test may be even further in the offing, if indeed ever realized at full scale. I am already fed up with having to search for things like “unpublished data”, “private communications” and the like, which I often cannot or will not be allowed to see.

Instead of waiting for the ‘Recommendation’, I would like to propose a test on one of the Bulleid locomotives equipped with the Lemaître front end, given a cooperating heritage organization. Such a locomotive has all the available space for a “correctly-proportioned” Lempor that accords with existing Lempor theory. European funding may be available to assist with the cost of such an effort, since this is valuable ‘cross-border’ research.

I specifically invite Mr. Wardale to submit a calculation of a Lempor system for that locomotive, with full appropriate dimensioning and construction data, with the expectation that testing of this design will provide full experimental verification of the theory he grounds his design theories and calculations upon.

JJGK
Sept 2013