'The fire burns much better ...'

PhD. thesis by J. J. G. Koopmans, published 2006.

The present writer's comments on 'The fire burns much better ...' were made available on the 5AT website around early 2007. Dr. Koopmans has now responded to these comments, see www.thefireburnsmuchbetter.nl. The following are my further comments on items of this response where this is required. These comments are based on hard copy of his response, supplied by Dr. Koopmans, and it is assumed that this corresponds precisely with the web-published version. Note that the following is specifically to answer this latest response, not to revisit the thesis in detail. Headings correspond to those used by Dr. Koopmans.

1. <u>Introduction.</u> Dr. Koopmans objects to my "personal unsubstantiated opinion[s]." Dr. Koopmans' opinions are not unknown in his thesis — the statement on thesis page 156 that "the Lempor design is flawed" [etc.] might be regarded as just such an opinion, and one, incidentally, that started this particular ball rolling. The reality is that there are factors in exhaust system design for which design parameters founded on precise theory or accurate testing are simply not available, and in such cases opinions or judgements or assumptions have to be made, or there would be no design. So opinions have their place, and opinions forged from experience are not necessarily so unsubstantiated

2. There are two major issues taken up by Dr. Koopmans and a number of minor ones. The first major one concerns the reason for the better performance of multiple nozzles compared to a single one, the second is the inclusion, or non-inclusion, of the combustion gas kinetic energy or momentum in exhaust theory. These are dealt with as follows, but first let us consider Dr. Koopmans' first sentence in his heading 'The Lempor.' He should not be surprised that I have concentrated on the Lempor as (i) it is the type I am familiar with, (ii) it is, in his own estimation, a concept that "could be regarded as the ultimate form of a locomotive front end" (thesis page 100) and as such is the type that should be of most interest to those currently involved with steam locomotives, and (iii) his criticism of the Lempor-type exhaust proposed for the 5AT had to be answered. (Whether such criticism — unfortunately 'set in stone', so to speak, in his thesis — of something that was not historical and that Dr. Koopmans did not have full knowledge of at the time, had a place in an "academic exercise in the history of technology," as Dr. Koopmans' describes the thesis, is questionable, but having made it, it had to be answered).

3. <u>Functional description of the front-end</u>. This culminates in the following conclusion: "The relative longer chimney is *the sole* cause of the improved vacuum induction behavior of multi-orifice chimneys." The Italics are mine and serve to emphasise that this conclusion rejects the possibility of any other interpretation for the acknowledged improvement in performance achieved with multiple nozzles. It is not clear that the information presented, and from which this conclusion is drawn, is actually a proof of the conclusion rather than something that might just be interpreted this way, and grounds for scepticism can be found within the thesis. For brevity only one will be given here, which puts the emphasis on the effect of multiple nozzles immediately downstream of the blast nozzle exit plane rather than chimney length *per se.*

3.1. Consider the equation $\{Q / Q_o = 0.32 \,.\, x / d\}$ (this is dealt with in par. (8)). This equation is for circular nozzles in a free jet. The latter does not conform to the Lempor, but as we are concerned here with multiple nozzles *per se,* and in the apparent absence of a corresponding equation for a confined jet, it has to be used. It is assumed that this equation is valid for each individual nozzle in a multiple nozzle arrangement. This is a validity that probably depends on the nozzle spacing, but it is essentially the same as Dr. Koopmans' assumption that a multiple nozzle exhaust is equivalent to the individual exhausts of his Fig. 5, each with a single nozzle, so any error here would seem to be common to both arguments. Using the subscript (1) for a single nozzle and (n) for n nozzles of the same total steam flow area, then $\{(\pi / 4)\, d_1^2 = n \,.\, (\pi / 4)\, d_n^2\}$ giving $\{d_n = d_1 / \sqrt{n}]$. From the equation for Q / Q_o at a common distance (x) from the nozzle, for a single nozzle $\{Q_1 / Q_o = 0.32 \,.\, x / d_1\}$, for each of n nozzles $\{Q_n / Q_{on} = 0.32 \,.\, \sqrt{n} \,.\, x / d_1\}$. As Q_n / Q_{on} is nominally the same for each nozzle, and the total multiple nozzle $(Q / Q_o) = \Sigma Q_n / Q_o$ (where Σ = 'sum of'), for a given total exhaust steam flow Q_o, $\{\Sigma Q_n / Q_1 = \sqrt{n}\}$. If the gas mass entrained is given by Q_g, as $\{Q = Q_o + Q_g\}$ an expression relating the different gas amounts entrained by (n) nozzles and a single nozzle can be derived as $\{\Sigma Q_{gn} = \sqrt{n} \,.\, Q_{g1} + (\sqrt{n} - 1) \,.\, Q_o\}$, i.e. in practice very substantially more with multiple nozzles (the reader can find out by how much by substituting suitable numerical data). Now, it could be argued that Dr. Koopmans and I are saying essentially the same thing here, simply looking at the same phenomenon from different ends of the exhaust — if so, there would be no controversy. Looking at the nozzle end of the process, i.e. at the start of entrainment, the above shows it proceeds more rapidly with multiple nozzles, which is my argument, but the factor (x) has remained constant for both single and multiple nozzles, which could be interpreted in Dr. Koopmans' way as giving a longer length for the entrainment process when related to individual nozzle area. In fact Dr. Koopmans expresses both ways of putting it in the first paragraph of thesis page 149. But - Dr. Koopmans' reasoning suggests that individual velocity profiles for each nozzle would maintain their identity over the length of a common chimney, or at least for a good part of it, in the highly turbulent pulsating flow conditions in a locomotive exhaust (his Fig. 6). This is also effectively Dr. Giesl's reasoning for his oblong design, which I have always thought to be rather fanciful, even more so in a circular chimney. Dr. Koopmans' Fig. 6 rather conveniently shows flat velocity profiles at the chimney exit, but this is very optimistic for locomotive conditions, indeed he himself writes on page 3, "The uniform velocity distribution at the chimney exit is an idealized situation, and in practice it is not fully achieved ..." If that is so, would we expect to have, say for 4 blast nozzles, 4 individual velocity profiles at all points in the chimney up to its exit? I suggest not, in fact this factor was assessed in a rough-and-ready way on SAR 19D No. 2644 with 4-nozzle Lempor chimneys (*The Red Devil* page 111). This exhaust was one of the very best draught producers ever applied to a steam locomotive, yet its chimney exit velocity profile form was not 'fully developed' and was *qualitatively the same as a single blast nozzle would give*, e.g. see thesis page 86 Fig. 4.5, and page 117 Fig. 6.5. (This factor would be easy to assess on a test rig — with pulsating flow — and possibly on a (rebuilt!) Bulleid Pacific, although the extremely short Bulleid-Lemaître chimney is very atypical and might give misleading results. Such a test would need to be done at medium-high steaming rates, i.e. at running speed, and the H & S people might have something to say about that if it were done from the running board, as I did it.) To summarise: in contrast to Dr. Koopmans, I suggest the effect of multiple nozzles is confined to the lower part of the chimney by accelerating the initial phase of entrainment. Using the previous notation and equations, and taking the calculated 5AT data (which is independent of the type of exhaust used) for illustration, 100% gas entrainment would be achieved when $Q = Q_o + Q_g = 3.25\ Q_o$. The true Lempor blast nozzle exit diameter with a 4-nozzle cluster would be 50.5 mm, and the diameter of the equivalent single nozzle would be 101.0 mm. As the (Q / Q_o) ratio is sensibly constant for each of the four multiple nozzles, in a free jet gas entrainment would be complete when $\{Q / Q_o = 3.25 = 0.32 . x / 50.5\}$, i.e. at (x) = 513 mm = 28.5% of the available height above the blast nozzles. For a single nozzle the corresponding figures are 1026 mm and 57% of the available height above the blast nozzles. This seems to adequately justify characterising the effect of multiple nozzles as better (i.e. more rapid) mixing near the start of the exhaust. In the upper part of the chimney turbulence continues to merge the individual flows into one, but because of the better initial entrainment the velocity profile is always flatter than it would have been with just one nozzle, and diffuser pressure recovery and overall exhaust performance are consequently improved. Perhaps the neutral reader might like to pronounce judgement on this issue!

3.2. It should be noted that Dr. Koopmans himself does not dismiss the possibility of nozzle design in itself affecting exhaust performance, without reference to the chimney length factor, e.g. see thesis page 477. The ultimate example of this should be the American Kiesel exhaust (see the 1941 AAR Locomotive Cyclopedia page 349) in which not only was the nozzle perimeter (= steam / gas contact length and therefore interface area) greatly increased but great care was taken to form the nozzle so as to facilitate this contact, and, it seems, to minimise eddies in the exhaust steam flow such as are found with Goodfellow tips. Nozzle performance was thereby improved without any alteration to the (chimney length : nozzle discharge area) factor by altering the nozzle geometry only, for which it is suggested that the primary effect was to accelerate the goal of a more uniform mixture velocity profile, the key point. (Comparative performance data for the Kiesel vs. circular nozzle are not to hand: any readers who have such data might supply it to the 5AT webmaster.) Dr. Koopmans writes on thesis page 477, "In the opinion of the author, the effects of these [the described] orifices are all autonomous, i.e., if applied together it is quite possible that the effects add up. The ultimate orifices in that case could very well be rectangular, with Goodfellow projections, and certainly applied in a multi-orifice blast cap." The present writer would tend to agree that favourable effects can add up, and whilst practical considerations must be observed (those and the law of diminishing returns are what dictate the number of nozzles in a cluster) might not the 'ultimate' be a multiple Kiesel? But beware the effect of carbon deposits!

3.3. Dr. Koopmans notes that multiple nozzles have the disadvantages of greater exhaust steam flow resistance than a single nozzle, and its consequences. Potentially the most serious disadvantage (not mentioned) is indeed more rapid reduction in total discharge area for a given thickness of carbon deposit, if this occurs. These factors are the price that has to be paid to gain the benefits.

4. The control volume. On the one hand Dr. Koopmans states that "... 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." On the other hand we can gather together all his latest objections to including the vertical component of gas momentum, as follows:

> "Including gas momentum ... 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 momentum equation should not contain any added factor for momentum of the induced mass flow."
>
> "... the Rugby [Giesl ejector] test results can, and should, be regarded as hard proof that an assumption of additional secondary-flow momentum is superfluous."
>
> "In the Porta equation this difficulty [i.e. factoring in the effect of multiple nozzles] is "solved" by fudging in some momentum for the secondary flow but this is neither a particularly scientific approach nor a particularly correct one."

The reader may be left rather bewildered. As Dr. Koopmans has drawn this particular circle it should really be up to him to square it, but nevertheless I will try. What I think he is saying is that when considering the exhaust flows in isolation within a defined control volume boundary, including the gas input momentum may be correct, depending on the exhaust design, but in the wider thermodynamic context it is not. As explained in par. (2), the Lempor exhaust is of greatest interest to contemporary engineers, so the following observations are limited to the validity in principle of the Lempor theory. This is given in thesis Appendix A.28, and takes the exhaust as a control volume bounded by the entry plane at the blast nozzle tip, the chimney wall, and the exit plane at the top of the diffuser. This entirely accords with the rules of a control volume and is the control volume Dr. Koopmans himself advocates. Dr. Koopmans writes, "... once the control volume has been defined from the plane of the orifices, *some distance below the chimney entrance*, [my Italics] it should be clear that attempting to take the gas momentum flowing into the chimney into account is clearly wrong ..." [etc.] But thesis Fig. A.28.1. shows that the Lempor blast nozzles are not below the chimney entrance, so this statement of Dr. Koopmans' does not apply. The Lempor theory itself is a classical energy balance for the defined control volume in which *all* entering and leaving energy and energy dissipation *must be* accounted for and equated to zero (thesis equation A28.1). It follows that not to include gas input kinetic energy would be wrong — it is immaterial to the control volume whether an input is dependent or independent, all that matters is that it is there. (In fact (presumably) very minor factors, such as the potential energy rise in vertical chimneys and heat lost by forced convection from that part of the chimney projecting beyond the smokebox are ignored, as they probably are in all exhaust theory.) That is all it is. This energy balance is used to calculate critical exhaust dimensions that make the sum of the individual energy losses, and therefore the necessary input in exhaust steam energy, a minimum (this was Porta's explanation to me). There is nothing in this that appears to contravene the rules of control volume theory, and as such it is valid as it is given. This being the case, *in this context* — and, as the Lempor theory and its aim are as explained above, there is no other context in which to consider it — the first statement by Dr. Koopmans quoted earlier is correct and all contrary statements would appear to be inapplicable. As written on thesis page 100, "The Porta theory was basically an extension of the Strahl equation, the gas velocity now correctly included ..."

4.1. In general, if the control volume entry boundary is at the orifice plane then the general layout of the exhaust and the position of this boundary in relation to the gas flow into the chimney will dictate the extent to which a fraction of the gas input may be considered to pass this boundary flowing parallel to the exhaust steam. This would have to be assessed for each individual exhaust design, and would, for example, appear to be significant for the exhausts shown in thesis Figs. 5.2, 5.12, 7.11, A11.7, A21.9, A21.10, and A27.4.

4.2. Dr. Koopmans is incorrect to interpret inclusion of gas momentum as an argument in favour of multiple orifice front ends. This is dealt with in par (11.1).

5. Other points. "Not Invented Here", theory from other disciplines. "It is precisely the distance kept by most locomotive designers [apparently including the present writer] from appropriate textbook theory that has made the front-end problem so misunderstood." By inserting the word "most" Dr. Koopmans hopefully absolves the likes of Chapelon, Giesl-Gieslingen and Porta of misunderstanding the front-end problem, and as we need look no further than their work for guidance on exhaust design there is no problem here. It is probably true that there has often been a lack of fundamental theoretical understanding in steam locomotive engineering in general. But to lay this at the door of the locomotive designer shows a lack of appreciation of the scope of his work, which usually covered such a range of engineering and engineering-related problems that he rarely, if ever, had the luxury of contemplating abstruse theory on just one part of the locomotive. What the locomotive designer — who was inevitably a 'jack of all trades' and usually a master of none — needed were clear methodologies for designing what had to be designed. These may well have originated in a rather better understanding of the theory than is apparent at first glance, and may have been made suitable, and if necessary simplified and refined by experience, for application to the actual situations facing the locomotive designer, by which time the embedded text book theory may not have been too obvious. Such, to give one example, is the Lempor theory, which is given in terms that mean something to the locomotive engineer and which gives a precise, well-defined route from which exhaust dimensions can be found starting with basic input parameters. If there has been a lack of theoretical understanding this should not be blamed on locomotive designers but rather on railway administrations and locomotive builders for failing to provide adequate steam locomotive R & D sections, which in turn could have provided the locomotive designer with the practical design information and methodologies he needed.

6. Dimensional analysis: the Buckingham Pi-theorem.

6.1. Dr. Koopmans completely misconstrues my comments. The Pi-theorem was mentioned only once, not to query it *per se* but purely to point out that there are more factors, and therefore dimensionless groups, influencing exhaust performance than those he has considered, some of which were given. As the use of dimensionless numbers is Dr. Koopmans' approach to design, it is entirely up to him to consider more fully all such influencing factors and their effective dimensionless numbers if he wishes to. It is equally true that these factors do not figure in classical exhaust theory either, but precisely because of this it is just these where uncertainty lies and where a contribution of real value to exhaust design is necessary.

6.2. Euler No. Questioning the use of the Euler No. *per se* (my comment on thesis page 133) was a reference to reservations made only by other people. My own reservation was limited to apparent contradictions in the thesis data. For example, Dr. Koopmans takes both D / d and the Euler No. as important parameters, but from data in the thesis these are not always mutually consistent, which must at least cast doubt on the validity of one (or both) as definitive exhaust criteria (see also my point concerning thesis page 230 regarding the D / d ratio).

6.3. Vacuum - exhaust steam pressure curves: these are not a definitive measure of exhaust system performance (see *The Red Devil* pages 127-8). However in practice such curves are a reasonable yardstick of comparative performance, and a very convenient one because vacuum and exhaust steam pressure are the (easily) measured parameters, hence their widespread use. That is all. In equating the Euler No. to these curves Dr. Koopmans has to accept that the Euler No. itself can be no more a definitive measure of exhaust performance than the curves are. He takes the MN data to show their equivalence. That in Table 7.2 of the thesis gives a MN measured Euler No. at the *highest* steaming rate = 0.0119 and *lowest* = 0.0145, whereas he calculates 0.01466 at the "endpoint" (presumably meaning *highest* steaming rate) from Fig. 34 of *The Red Devil* - a rather unfortunate mismatch. But the real argument in favour of graphical presentation - and *inter alia* against the use of the Euler No. - is that the former shows its interpretation of exhaust performance over the full steaming range compared to any given Euler No. at only one point, which is clearly more useful, especially for comparative purposes. Because of this, assessing an exhaust by an Euler No. may fail to predict any potential deficiency at a different part of the operating range. Take the case of the SAR 25NC Class Giesl ejector - its Euler No. at full load may have been acceptable but would not have revealed the fall-off in performance at low output that was self-evident from the characteristic curve, see par. (10). The Chinese Railways' Giesl had the opposite characteristic, which again is best illustrated graphically (*The Red Devil* page 473). This is what graphs readily communicate that the Euler No. does not. When designing an exhaust, however, having "A single Euler number as target" would seem to have the same significance as, say, designing by the Lempor theory for a single (maximum) operating condition.

7. The orifice to chimney distance: x / d It is true that my judgement of blast nozzle tip to chimney position is influenced by the relevant result of the Datong tests, this conforming to the position adopted by Porta in the Lempor theory, see thesis Fig. A28.1. It is also agreed that the weight of opinion prefers a lower position. The following is relevant to Dr. Koopmans' points.

7.1. The Datong tests were conducted over 25 years ago and no more information can be given about them than is already in *The Red Devil*. Their purpose was to investigate specific factors for the QJ exhaust design and they were conducted accordingly. I am not able to judge their level of sophistication compared to other exhaust tests, but they did attempt to replicate actual locomotive working conditions as far as possible, for example in respect of pulsating flow.

7.2. Placing the multiple blast nozzle tip at the mixing chamber entrance gave the best result in these tests, and at the time no reason connected with the tests was found to discount this. It was therefore naturally adopted for the QJ Lempor design, there being no contrary opinion at that time for such an exhaust. I do not recall any "sudden surge," as Dr. Koopmans puts it, in vacuum for this position, nor has this ever been claimed. The result was simply the high point of what was, from memory, a quite shallow curve (or curves).

7.3. Dr. Koopmans quotes old tests made with exhausts quite different from the Lempor, and with single blast nozzles. The validity of applying such conclusions to a modern exhaust is questionable. Consider:

7.3.1. Sophisticated exhausts all seem to have the nozzle tip either in or very close to the chimney entrance, e.g. the Kylchap (thesis Figs. 5.1 and A24.6), Giesl (Figs. 5.2. and 5.3.), Lempor (Fig. A28.1), also the Kylpor (not illustrated in the thesis), so I am not alone. All these go against the finding of Young that Dr. Koopmans quotes.

7.3.2. Dr. Koopmans writes, "... if the longest possible chimney is wished for, [which it is, e.g. see Koopmans' Fig. 5] it might prove to be advantageous to position the orifice above the plane of the chimney entrance." Yes. Note that Young's experiments, for example, (thesis p. 417) altered the (x / d) ratio by raising or lowering the blast nozzle (or chimney), not by varying chimney length, so could not account for this factor (neither, probably, did the Datong tests).

7.3.3. A possible explanation for the Datong result is that lowering the blast nozzle results in exhaust steam exiting where there is (increasingly) a radial component to the gas velocity, which may deflect the exhaust streams from multiple nozzles inwards and thereby reduce their effectiveness (see thesis page 142, 2nd par. for the effect of the gas flow on the exhaust steam flow). Deflection from the vertical would not be present in any test conducted with single nozzles (e.g. Goss and Young, as given in the thesis).

7.4. Remembering that I am specifically concerned with the Lempor, the experimental evidence sited by Dr. Koopmans does not support his case in this instance. But I am not convinced that the position I have advocated is best either. It is still an open question, perhaps to be answered separately for each individual exhaust design. Here CFD, a tool not available in the past, should prove of value provided the mathematical model used accurately reflects the real situation. As this factor is not clear in the CFD modelling made by Dr. Koopmans (e.g. did it replicate the exact pulsating flow conditions of a locomotive exhaust and Lempor-type multiple blast nozzles?) no comment can be made on the results. Any critical assessment would have to come from someone much more familiar with the technique than I am. A general point here is that preference should be given to observations of how things actually are (when this can be done) over the predictions of mathematical modelling of how they should be, given the assumptions and simplifications usual with the latter.

7.5. Finally, where does the contention that the "Lempor is supposed to be very sensitive to radial mismatch of jet and chimney" come from? I would say it is less sensitive than a 'normal' exhaust with large x / d ratio. It is the Giesl ejector that is highly sensitive to (lateral) misalignment. Its construction aims to prevent this from happening, although see *The Red Devil* pages 11, 306 and 474.

8. Textbook theory. Dr. Koopmans' thesis presents the equation concerned with no specific caveats or restrictions as to its validity. I pointed out that this leads to an incorrect result as x → zero. He has now provided the missing restriction, i.e. that the equation is not valid for $x <\approx 6d$, something he considers "... so obvious ... that it was not even mentioned in the original text." If it had been, the problem Dr. Koopmans thinks I have with textbook theory would not have arisen. Engineering being of necessity as precise as possible, the equation should be written as: $Q / Q_o = 0.32 \ x / d$ (for $x \geq 6d$).

9. Momentum Theory. The issue of the first sentence is dealt with in par. (4). I thank Dr. Koopmans for pointing out the assumption of zero exit velocity in the Kentfield and Barnes paper — the validity of using this paper will therefore be reassessed.

10. The Giesl ejector. "On [RD] p. 14 *Mr. Wardale* [my Italics] generalizes this test [Test 3, giving bad performance at lower outputs] to all Giesl tests conducted by SAR staff …". I invite Dr. Koopmans to find any text on pages 10 to 17 of *The Red Devil* where what is written about the details of the SAR Giesl tests originates from me rather than being quoted from official documents and letters. Dr. Koopmans' sentence continues: "... this assumption does not appear to be supported by any data in that book." Well, try looking at page 305, Fig. 126. Dr. Koopmans adds, "Shall we conclude by stating that from Mr. Wardale's observations, [actually the SAR's observations if one takes notice of the quotation marks in *The Red Devil*] Prof. Dr. A Giesl-Gieslingen was unable to calculate a properly-proportioned Giesl ejector for the SAR 25NC class?" Unfortunately the answer is 'yes.' Giesl himself admitted this, I have copies of the correspondence between Giesl (and/or his associates) and the SAR to prove it, and it is explained in *The Red Devil* page 14, last paragraph. Dr. Koopmans then quotes from me as follows: "For a Giesl ejector at least some gas momentum should be added as input." He then proceeds to demolish this, ending with "... the Rugby test results can, and should, be regarded as hard proof that an assumption of additional secondary-flow momentum is superfluous." This has been dealt with in pars. (4) and (4.1).

11. The Lempor.

11.1 "In the Porta equation this difficulty [i.e. factoring in the effect of multiple nozzles] is "solved" by fudging in some momentum for the secondary flow ..." Can Dr. Koopmans state exactly where in the Lempor theory he gives in Appendix A.28 of his thesis the inclusion of secondary (gas) flow momentum [which he regards as "correct" (par. (4)) — or is it "not particularly correct" as he now states in his Lempor section?] is in any way linked to multiple blast nozzles? Can he state where in the theoretical equations nozzle design figures at all? In fact nozzle design does not play any part in the equations to determine Lempor exhaust dimensions — these would be exactly the same with a single nozzle, as Dr. Koopmans notes, ("Any strictly energy- or momentum-based calculation for a single vs. multiple system would produce identical results ..."). Where nozzle design does figure — as it *must* do — is in the recommendation for mixing chamber length : diameter ratio, given on thesis page 450 right at the end of Appendix A.28. The value of this will depend on nozzle design, the given figure of 2.5 (later reduced to 2.0) being judged to give an acceptable diffuser entry velocity profile when using a 4-branch multiple nozzle, and being significantly less than it would have to be for a single nozzle, this in turn being based on (Porta's) judgement of the more rapid entrainment with multiple nozzles, in accordance with par. (3.1). On thesis page 100, Dr. Koopmans states of the Lempor: "The effects of the four orifices were not taken into account separately," but the foregoing shows how this was in fact done.

11.2. Dr. Koopmans' remaining argument in (exclusive) favour of the chimney length explanation for multiple nozzle superiority has been covered in pars. (3) — (3.2).

12. The limits of formulae. The query raised concerning the Q / Q_o equation (par. (8)) was entirely legitimate and has resulted in Dr. Koopmans now supplying the missing caveat. As to the Saunders' formula, where in my comments on thesis pages 461-4 have I inserted "limiting values" in any of the Saunders' equations? All these equations are correct as they stand, but what I have done is to point out that none of them fits to a 100% confined flow Lempor type exhaust where the assumption that "Q_g has zero vertical component of velocity" (thesis page 461) is not valid — see the difference between a Lempor and all figures in thesis Appendix A.32. Dr. Koopmans' comparison with the result that would obtain if the exhaust steam flow → zero in the Porta exhaust theory is spurious, and one must wonder why he thinks that "... both Giesl-Gieslingen and Porta are <u>correct</u> in taking the vertical component of gas momentum adjacent to the

jet momentum at the orifice into account" when this is so obviously "a function of the velocity of the exhaust jet."

13. Individual points, where required (by thesis page no.)

    Page 55. Dr. Koopmans is correct. I seem to have completely misread the thesis here.

    Page 120. This has been answered in par. (8).

    Page 144. The point here is the non-universality of the D / d criterion.

    Page 145. Dr. Koopmans may be right in his assessment of the Bulleid - Lemaître MN Class exhaust, although this would need investigating using somewhat different data than he gives, for example the relative maximum steaming rates of the boilers concerned. He compares the MN exhaust to the A4 and BR 8P double Kylchap exhausts, which were no doubt better. But note that as built with a 'classical' plain double blastpipe and chimney (presumably inspired by Ell), the BR 8P had two 4" nozzles giving it a blast nozzle tip area only 93% of that of a MN. It is worth quoting from the Rugby test report on MN No. 35022 (as given on page 184 of *Bulleid, Last Giant of Steam*): "… none of the combinations of [classical] blastpipe and chimney gave results that were up to the standard set by the multiple jet arrangement and none of them enabled the boiler to produce an amount of steam comparable with that produced with the multiple jet exhaust ..."

    Page 152. The 26 Class data is still incompatible: the *blast nozzle* steam flow should be 23150 kg/h and vacuum about 5100 Pa for a (nominal) gas flow of 55224 kg/h.

    Pages 155 - 156. The true Lempor 5AT blast nozzle diameters according to the calculations I made are 50.5 mm exit, 47.1 mm throat (1.15 : 1 area ratio as a starting figure). Obviously the proof of which exhaust theory gives the best results using the basic Lempor concept could have been definitively determined by tests on the 5AT, which would have been relatively easy to do as all exhaust proposals are of similar size and could therefore have been fitted in without undue difficulty - provision for interchangeability could have been built into the smokebox. Unfortunately this will never be done.

    Page 178, second item. This has been answered in par. (8).

    Page 180. It was not stated in the tables concerned that one pressure was total and the other static, hence the query.

    Page 182. This has been covered in pars. (3) - (3.2).

    Page 183. The mistake is mine for not making clear that it is 'control volume input' that features in the Lempor theory for the purpose of determining ejector dimensions, see par. (4).

    Page 185. I agree with Dr. Koopmans that the figures concerned seem to show the blast nozzles as he describes them.

    Page 256. Dr. Koopmans' translation appears correct, although as his German is probably far better than mine he will not need me to tell him so. All I can say is that I revise my comment from "... it can scarcely be believed that it is what was originally written" to "what was originally written can scarcely be believed" (!) Either that or I'm going dyslexic.

    Page 334. The Lempor control volume entry boundary is at the blast nozzle exit plane, through which all combustion gas flows, see par. (4).

    Pages 456 - 458. Agreed that of all factors influencing exhaust design and performance, some will be more important than others. I listed eleven such factors in my comment, and others were given in summary item (3) at the end of my comments. These were given simply to show that the list of variables given on thesis page 456 was not all-inclusive. However, in fairness it must be said that the great majority of these do not figure in classical exhaust theory either, hence the need for more meaningful information on them (see par. (6.1).

14. Conclusion. Dr. Koopmans writes, "... 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." I think if my original concluding comments - which I will not repeat in full here - are read, this conclusion cannot be supported. I did indeed recognize the historical value of the thesis, and also its value towards making successful substitution of multiple for single blast nozzles, something that should be of great benefit to preservationists who can thereby improve locomotive performance easily and cheaply without altering appearance. I stated that had Dr. Koopmans' work been available for the substitution of multiple for single blast nozzles in South Africa "... the results would certainly have been better" than what was actually achieved. What I did criticise was precisely the "... putting [everything] 'on the table' in one place ..." I wrote "... the inclusion of so much material, sometimes irrelevant, contradictory, or simply wrong when seen from today's perspective, is not much help to the practical engineer." It is, of course, from the perspective of the latter, and not that of the historian, that I view the work. The overloaded lot of the locomotive designer is hinted at in par. (5) earlier, and the sure way that he would not have grasped "appropriate textbook theory," as Dr. Koopmans laments, would have been to present him with something like this thesis to wade through. Dr. Koopmans has himself described the thesis as an "academic exercise in the history of technology" and I feel that it would have been better to stick with that for satisfying the historians and *entirely separately* to combine the ultimate results of all this development with current engineering knowledge to develop a concise and practical methodology illustrating how exhaust systems should now be designed, for both simple alterations, e.g. multiple blast nozzles, and 'state-of-the-art' design. Dr. Koopmans obviously feels that he has accomplished both, but long before they have reached the latter part of the thesis the minds of those who ought to be interested in it have probably glazed over and the thesis consigned to a dark corner of a bookcase, never to be reopened (a fate probably shared by many copies of *The Red Devil*!) Just how many of the hundreds of heritage steam locomotives now running have had their draughting improved by the easy, cheap and externally invisible method (which I supported) given in this thesis?

Regarding a Lempor for a Bulleid Pacific, it would certainly have to be at least a double chimney, especially as available height is very restricted by the inside cylinder (thesis Fig. 7.18), also a spark arrestor may be mandatory, so the bland assumption that there is "all the available space" might well cause some strong language from whoever had to actually design the proposal. Fortunately it will not be me, as I consider myself retired from steam locomotive work with no intention of returning to it. But the remainder of the 5AT Group has all the information necessary to design a Lempor exhaust, and I am sure they would be delighted to accept the challenge. It is suggested they be contacted about this. It would, of course, be necessary for Dr. Koopmans to supply his own proposal for comparison — any wagers made on the outcome might be used to finance the contest. A "cooperating heritage organization" may, however, be rather hard to find.

As a final point, Dr. Koopmans' last sentence mentions "... his [i.e. my] design theories ..." It should be pointed out that no exhaust design theory whatsoever originated with me or is mine.