

For the Journal of the Franklin Institute.

Reply to the Criticism of SAMUEL McELROY, C. E., "*On the Erie Experiments on Steam Expansion by U. S. Naval Engineers.*" By ALBAN C. STIMERS, Chief Engineer, U. S. Navy (one of the Experimenters).

The paper, of which the above quotation is the title, appeared in the October and November numbers of this *Journal*, and is a remarkable example, no less of the looseness with which many engineers read professional papers, than of the great want of analysis they bring to bear upon the subjects of which such papers treat. It is well, perhaps, that such a reader and such a student should have given us the impression which the report of the Board made upon his mind, as he is the representative of a large number in the profession, who exercise in their numerical strength a great influence over the designs and proportions of a large amount of steam machinery. A reply, therefore, to one of them, who has had the boldness, the intelligence, and the literary ability, to come out and attack the report in a first-class magazine, though it may not be a reply in detail to each individual, will at least have the effect of showing them that a closer study of the report itself would answer most of their objections.

The following are some of the principal difficulties under which this writer labors:—

A failure to conceive the real matter which the experiments were intended to assist engineers in deciding.

A failure to separate incidental information, developed in the course of the experiments and generally valuable to engineers, from that which bore directly upon the main question to be decided.

A misconception of what the conditions are in an engine, the change of which will affect the economic result. And finally,

He fails to comprehend many of the simplest arguments, and to perceive that most of his objections are answered in the body of the report itself.

He says, "This is the real matter at issue—whether it is cheaper to carry high steam and expand, or to carry low steam and follow full stroke." He does not appear to be conscious that this mode of regarding the subject unites the question of high pressure with that of expansion; whereas, the experiments were instituted, not to ascertain the relative economy of using steam of different pressures, but "the relative economy of using steam with different measures of expansion." The report states, on page 23, in the very commencement of the "discussion of the results," that, "In examining the preceding two tables, it will be observed that particular care was taken to have the initial cylinder pressure (table 2, line 1) the same in all the experiments, as nearly as practicable, which, with proper area of conduit open in proportion to quantity of steam used in equal time, would necessarily make the boiler pressure equal. In fact, throughout the experiments, the boiler pressure (table 1, line 18) was nearly equal, the difference

being too slight to be of any practicable importance. That this is a proper condition for the purpose of the experiments will be obvious when it is considered that amount of pressure is purely a question of boiler, and not at all one of engine.

"If a given power be required to be developed by the same piston, working at a given speed, but with different measures of expansion for the steam, it is plain that the initial pressure must be increased as the steam is used more expansively, and the condition of equality of initial pressure could not obtain. This, however, would only be unnecessarily employing too large a cylinder when the steam was used less expansively; or, in other words, it would be an engineering blunder. The proper method is—for equality of power and of initial pressure—to proportion the cylinder to the measure of expansion adopted for the steam, making its capacity for equal speed of piston inversely (table 2, line 22) as the net effective pressures upon the piston (table 2, line 7).

"From the justness of these premises there is no escape, and it is preposterous to base a claim of economy for large measures of expansion, not upon the expansion *per se*, but upon higher initial pressure, when that same pressure can be employed just as easily and well when using steam without expansion, if the cylinder be properly proportioned to the work. Therefore, in making a set of experiments to determine the *practical* economic results of using steam with different measures of expansion, it is an *essential* condition that the initial cylinder pressure be maintained the same in all cases.

"It is now proper to give the reasons why a high initial pressure is desirable in view of the economical production of the power. And, first, of the generation of the steam.

"As the dynamic effect of a given weight of steam increases in a higher ratio than the heat required to evaporate it—owing to the accompanying increase in the temperature of the steam—it is obviously desirable to use it in the cylinder at the maximum pressure throughout the entire stroke. We say *use* it in the cylinder, for the economy in function of pressure *per se* attaches to the pressure under which the steam is used, and not to that under which it is generated, because its dynamic effect is developed during its use in the cylinder, and not during its generation in the boiler. Now, it is plain that, starting with the same initial cylinder pressure, the steam will be used with the highest pressure throughout the stroke of the piston when it is used without expansion; and the more it is expanded, the more is the pressure reduced in the cylinder, and the advantage lost that attaches to higher pressure *per se*. This is one *practical* point of gain for smaller measures of expansion over larger ones—a point entirely ignored when the problem is merely theoretically considered, but which must be included in a practical determination; because it is an inseparable function of the physical laws of steam."

The careful reader will observe as very prominent in the foregoing quotation from the report of the Board, that they were not endeavoring to ascertain how much it was desirable to expand the steam in

existing engines, but in those which were yet to be built, and in which proportions could be given that would do the required work in the most economical manner; that the question of expansion, as separated completely from that of boiler pressure, was the subject of investigation; and that they recognised fully and stated clearly the advantages of high pressure steam.

This writer discusses the rate of combustion in the furnaces, the evaporation per pound of coal, the pounds of coal per hour per horse power, the order in point of time in which the different experiments were tried, the speed of the piston, the amount of steam used in a given time, &c., &c. If these questions affected the economic result as properly ascertained by accurate measures, it would be very proper for him to discuss and criticize them; but he does not even attempt to show that they influence in the slightest degree the only measure of the cost of the power developed by the engine, which was pronounced as the only correct one by the Board. They state, page 31 of the report,—

“With regard to the cost of steam power, there are three different measures in use, all of which will be found employed in table No. 1: namely, the weight of the coal; the weight of the combustible; and the weight of water, by tank measurement, consumed per hour per horse power. The last alone is employed in table No. 2. Of these, the first is the least exact; the second is more exact than the first, because it eliminates the ashes, the amount of which is an accidental quantity; but the last eliminates everything connected with the generation of the steam, and is, therefore, critically exact, and should be accepted as the true, universal, and only measure of the cost of the dynamic effect, *per se*, produced by the steam in the cylinder.”

It will be perceived that, with this measure accurately applied, it would add nothing to the reader's information upon the subject under discussion, “to have known precisely the times and manner of coal supply, tank supply, cleaning fires, starting and hauling fires, variations of pressure, and the like, as to the boilers.”

To show that the Board recognised fully the effect upon the evaporative powers of the coal, of the change in condition to which it was necessary to subject that part of the apparatus in order that the essential one should remain unchanged, I quote from the report what immediately follows the above explanation of the true measure of *engine* economy.

“The weight of coal, under the most favorable circumstances, can only be considered as an indirect and comparative measure of the weight of steam consumed; but, even comparatively, it is not exact unless all the conditions of boiler and coal continue precisely the same, which is a manifest impossibility, as the calorific effect obtained from the coal varies with the skill and care of the fireman; with the quantity of water mechanically present in it; with the temperature of the air entering the ash-pit; with the rapidity of the combustion; with the thickness of the fuel on the grates; with the hygrometric and barometric conditions of the atmosphere; with the more or less copi-

ous supply of air to the furnaces; with the temperature of steam in the boilers; and with the per centum of refuse obtained from the coal; not to include the uncertainty in the determination of the precise weight of coal which generated the steam used in the engine."

The intelligent reader will observe, then, that all the long drawn-out discussions of this writer about the rate of combustion; the use of different coals with their attendant differences in per centa of ashes, &c., and their different evaporative powers, are all thrown away. All that is merely incidental information, and does not affect the question of "the relative economy of the use of steam with different measures of expansion" in the slightest degree, when the economy of each of those measures is determined by the amount of water required to be evaporated per hour per horse power. As it is true, however, that a doubt of the propriety with which any of the information given, even though only incidental, was obtained, would throw a doubt over the whole report, it is proper to reply to some of the principal objections which he has brought against the coal account. It must not, however, be forgotten that the primary objects of the experiments were not to be interfered with in order that engineers might know exactly the evaporative power either of the coals or the boilers used. The Board very naturally expected that there should exist in the profession generally, sufficient sagacity to perceive this and consider the incidental results shown, in close connexion with the conditions under which they were obtained; all of which are fully described in the report.

He tells us that, though in the text the Board claims that "In commencing an experiment, the engine was operated for several hours to adjust it to the normal conditions required to be uniformly maintained during that experiment, and to bring the fires to steady action," the tabulated record shows that, in one case, there was no interval at all, and in two others, only two hours.

In the case of there being no interval, there was nothing to be changed, except the damper in the smoke-pipe, and the point of cutting off. It probably took *one second* to change the former, and *ten* the latter. The other changes upon which he dwells, were simply the immediate results of those two simple movements. The general statement in the text is perfectly true as a general statement intended to cover the experiments as a whole. The above happens to be an exception when it was not required to sweep the flues, change the kind of coal used, or to add to or remove from the wheels any paddles. As for the two instances of there being but two hours between two of the experiments, it must be a very firm position that it is necessary to assail by drawing the nice distinction between the general statement of "several" and the particular one of "two," occurring twice out of six times.

With regard to estimating the "efficient" amount of coal in the furnaces at the commencement and at the end of the experiments, the Board considered that in obtaining the number of pounds of coal per hour per square foot of grate; the number of pounds of water evaporated per pound of coal; or the number of pounds of coal per

hour per horse power, the last decimal place given would not be affected by any error it would make in this estimate, as each experiment was continued sufficiently long to reduce such errors beyond any change in the third decimal figure. The results as given are, therefore, mathematically accurate to that decimal, notwithstanding possible errors in such estimates.

He endeavors to make a point of the difference in the per centum of refuse obtained from the different coals used. He does not appear to be aware that different coals may contain different amounts of non-combustible material, or that different cargoes of coal from the same mine cannot be expected to contain precisely the same per centum of refuse.

It is not necessary to follow him further in his remarks about the coal. Enough has already been written to show that his positions upon it are untenable, and to refute each detail would only add to the length of this paper without increasing its force.

With regard to the corrections of the back pressures in the cylinder to the uniform one of 2.7 lbs. made in table No. 2, he says:—

“There is a difference of vacuum against No. 2, which we do not find credited. Experiment No. 7 ($1\frac{1}{2}$ ths) shows an average back-pressure in the cylinder of 4.2 lbs., while No. 2 shows but 2.8 lbs. In all these trials, it appears that expansion reduces back pressure by a descending series, except a change in No. 7.* But without accepting this *experimentum crucis* of the condensation argument, the Board decides that back-pressure must be assumed at a common standard, which it accordingly takes at 2.7 lbs. from No. 3. Consequently, the mean pressure of No. 2, which is 13.6 lbs., is to be charged with the standard of 2.7, while that of No. 6, at 29.8 lbs., is credited with the difference between 4.2 and 2.7. This varies the relative horse power, and is highly creditable to the treatment of the questions at issue.”

The Board made out one table as the experiments progressed, containing, complete, the exact experimental determinations; but although this is given in the report, it was considered that it would be more complete for the purposes of correct comparison, if differences, obviously due to accidental circumstances, could be eliminated. Upon this subject the report states:—

“The quantities given in table No. 1 are the precise ones obtained by experiment; but some of them require to be slightly corrected, for the purpose of making exact comparisons. The quantities to be corrected are only those of the mean gross effective pressure on the piston, and back pressure against it, (table No. 1, lines 22 and 23,) together with those of the mean gross effective, total, and net horse power depending on them (table No. 1, lines 24, 25, and 26). These corrections are made necessary by the fact of the inequality of the back pressure during the experiments; but, as it was caused by such accidental circumstances as air leakages, different proportion of cylinder steam-port to weight of steam discharged at the end of the stroke of

* He has evidently become confused in his numbering of the experiments, and intends here the one cutting off at 4.45ths.

the piston, and different speed of piston, all of which can be made the same and the back pressure rendered equal, it is necessary for a proper comparison to make the back pressure the same in all the experiments, and to rectify the mean gross effective and net pressures on the piston in accordance with this equality. This has been done in table No. 2, which contains, in addition, all the other data and calculated results requisite to a complete determination of the relative economy due to the different measures of expansion."

It will be observed in this quotation from the report, how completely the Board sank all considerations of existing engines in their search for the true principles which should govern future constructions, and as the amount of back pressure is dependent entirely upon freedom from air leakages, area of exhaust passages, and condenser and air pump capacity, all of which, in every well proportioned engine, are designed with reference to weight of steam used in a given time, and not with reference to size of cylinder, nothing can be more proper than to assume that it shall be uniform. It was obviously impossible to have it uniform experimentally without changing the structure of the engine used for the experiments; but it was considered that a matter so self-evident would only require that a proper quantity should be taken as the standard for all. It was determined to take the smallest that was experimentally obtained in any one trial; this was 2·7 lbs., and occurred in the experiment in which the steam was cut off at $\frac{1}{4}$ th.

Now this writer complains that in the trial which expanded the least ($\frac{1}{4}$ ths) the mean pressure obtained by experiment is credited with the difference between the 4·2 lbs. back pressure obtained by experiment and the 2·7 lbs. taken as the standard; while the trial which cut off at $\frac{1}{8}$ th has only the difference between 2·8 lbs., obtained by experiment, and the same standard of 2·7 lbs. That is, according to him, the Board gave the $\frac{1}{4}$ ths trial an advantage of 1·4 lbs. per square inch in the final comparisons. To many people it will appear quite clear that taking the lowest back pressure obtained experimentally as the standard was giving the greater measure of expansion all the advantage it could claim, as with equal initial pressures the greater the measure of expansion the less is the mean total pressure. To make it clear to every one, however, let us suppose that the highest back pressure experimentally obtained (4·2 lbs.) be taken as the standard, instead of the lowest (2·7 lbs.), and observe the difference in the comparative economy.

	As per table.		Supposed case.	
	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{8}$
Point of cutting off,	$\frac{1}{4}$	$\frac{1}{8}$	$\frac{1}{4}$	$\frac{1}{8}$
Mean total pressure,	34·0	16·4	34·0	16·4
Mean net pressure,	29·2	11·6	27·7	10·1
Per centum which the latter is of the former, . .	85·9	70·7	81·5	61·6
Relative net pressures between the different points of cutting off,	1·000	·822	1·000	·756

The greater measure of expansion would therefore lose

$$\left(100 - \frac{.756 \times 100}{.822} = \right) 8 \text{ per cent. if calculated as desired by this}$$

critic, instead of by the method adopted by the Board.

When this writer comes to the subject of the friction of the engines, he does not appear to understand that the friction of the load, varying directly as the load, may be considered as a part of it in all calculations for comparison between different trials with the same engine. It was only the friction caused by the gravity of the different parts of the engine that the Board considered constant, that is, that it required a constant pressure to overcome. So that, instead of controverting "Morin, Weisbach, and others," it was upon the general principles laid down by them, and experimentally found strictly applicable to the steam engine a few years ago by two naval engineers, that the Board proceeded.

With regard to reducing the size of the engine when the measure of expansion is reduced, he tells us that only the diameter of the cylinder would be reduced, all other parts remaining precisely the same; but it is well understood by all good designers of steam engines, that all the parts through which the power of the steam is transmitted must be proportionable to the initial pressure upon the area of the piston. With the given initial pressure, therefore, the larger piston, required when using larger measures of expansion, requires also larger piston-rods, cross-heads, connecting-rods, beams, cranks, shafts, pillow-blocks, engine frames, bed-plates, &c., &c.—condenser, pumps, pipes, and valves being the only parts that undergo no change.

In remarking upon the fact that engineers were aware before the trial of these experiments that there was a loss by steam leakages and condensation, he states:

"So fully, in fact, are engineers advised on this point, that when any experiment is presented to them, no matter by whom conducted, which claims to have found but 2.91 per cent. loss between the tank and indicator, they respectfully deny its accuracy. It is impossible to avoid a greater loss in the boiler itself, and between the boiler and steam chest, and at the valves as well as in the cylinder."

The steamer *Michigan*, in which these experiments were made, had been out of commission twelve months, during which time new boilers were put in and the engines thoroughly overhauled by one of the most experienced and careful engineers in the naval service; and, in addition to this, the Board spent two weeks in preparing everything for the experiments, taking especial care to prevent the possibility of leakage. With regard to leakage from the boilers, the report states:

"The only outlet from the boilers was through the blow-off valve. Any leakage to occur here had first to pass the blow-off valve, and next the stop-cock placed in advance of the Kingston valve. The blow-off pipe, its valves and cocks were frequently examined during the experiments, and the undersigned are certain there was no leakage from the boilers. The boilers themselves were quite new, and double

riveted; they were so tight that the water would stand in the glass gauges without appreciable fall for days."

It is considered that there will be little difficulty among engineers about choosing in which of the above explanations and assertions they will place their confidence.

His illustrations of losses by leakage and condensation, immediately following the above quotation from his paper, do not require recapitulation, as their errors are certainly too evident to deceive any reader of this *Journal*.

He complains that the notes of the experiments are omitted in the report. It would be interesting to know of what possible use it would be to engineers to have placed before them 21 pages of tabular record and the engravings of 1008 indicator diagrams. Why does he not ask for a verbatim report of all the discussions which took place daily between the members of the Board, while the report was being elaborated and written?

He concludes his paper with an elucidation of "a certain mechanical principle," which speculative engineers of slight practical experience sometimes indulge themselves in dreaming about. The "principle" being that, as the inertia of the mass—the motion of which is accelerated during the first half of the stroke and retarded during the last half—absorbs a portion of the power of the steam while being accelerated, and returns it again while being retarded, the engine will work more smoothly and properly when cutting off at less than half stroke than when following beyond that point. A little practical experience soon dispels all this kind of visionary speculation. He must certainly admit that the difficulty he predicts for an engine cutting off at a point beyond the half stroke would increase with an increase of the speed of piston. Now, it so happens that the present writer took a trip only a few days ago from New York to Amboy, N. J., and return, in the steamboat *Richard Stockton*, for the express purpose of observing the operation of her steam machinery, accompanied by the engineer who designed it. The engine is the ordinary over-head beam, and was built in 1852. The cylinder is 48 inches diameter by 12 feet stroke of piston. The average revolutions during the above trip were 27 per minute, equal to a speed of piston of 648 feet per minute, and I was informed that it had been driven at the rate of 31 revolutions, or 744 feet per minute. Probably this speed of piston is not exceeded, if equalled, by any engine of equal dimension, and it certainly worked with remarkable smoothness and regularity, yet it was cutting off at *three-fourths* the stroke from the commencement. The designer of the engine explained why he followed so far by saying, that the engine was being driven so fast it was impossible to make it work smoothly and regularly when cutting off at any shorter point in the stroke.

In treating the above subject, he accuses the present writer of historical mistakes in publishing an abstract of the report in the April number of this *Journal*. To this it is only necessary to reply, that the historical statements were gathered from standard works upon the history of the steam engine, and will naturally stand as true until *proven* incorrect.

In conclusion, I would remark that it is seldom that a report of 38 pages of printed matter, giving the results of an extensive set of experiments upon an important subject, has not some point so weak that it may be successfully assailed; and if the paper to which this is a reply is the heaviest artillery that can be brought against it, the Board have abundant reason for congratulation that they have been so successful.

To Check the Warping of Planks.

The face of the planks should be cut in the direction which lay from east to west as the tree stood. If this be done, the planks will warp much less than in the opposite direction. The strongest side of a piece of timber is that which in its natural position faced the north.—*Dingler's Polytech. Jour. Bull. Soc. d'Encour. pour l'Indus. Nation.*

For the Journal of the Franklin Institute.

Resistance of Wrought Iron Tubes to External and Internal Pressure. Deduced from Experiments of W. Fairbairn.

By CHAS. H. HASWELL, C. E.

(Continued from page 306.)

No. 2.

Resistance of Wrought Iron Cylindrical Tubes to Internal Pressure.

Taking the mean of the results of Experiments 31 and 34 on iron tubes,

$$P = \frac{P' \times d}{2t}, \text{ or } \frac{425 \times 6}{2 \times .043} = 29,651 \text{ lbs.},$$

$$\text{and } \frac{2t \times P}{P'} = d, \text{ or, } \frac{2 \times .043 \times 29,651}{425} = 6 \text{ ins.},$$

$$\text{and } \frac{P \times 2t}{d} = P'.$$

Hence, *To ascertain the Thickness of a Wrought Iron riveted Tube or Flue, the Diameter of the tube and the Pressure in pounds per square inch being given.*

RULE.—Multiply the pressure in pounds per square inch, by the diameter of the tube in inches, and divide the product by twice the tensile resistance of the metal in pounds per square inch.

EXAMPLE.—The diameter of a wrought iron flue is 6 inches, and the pressure to which it is to be submitted is 425 lbs. per square inch, what should be the thickness of the metal?

Assume the tensile strength to be as above, 29,651 lbs.

$$\frac{425 \times 6}{29651 \times 2} = \frac{2550}{59302} = .043 \text{ in.}$$

The tenacity or tensile resistance of wrought iron boiler plates, ranges from 62,000 to 42,000 lbs.* per square inch; hence it appears,

* Including English plates.