DISCUSSION—ECONOMY IN FRAMED STRUCTURES.

ber of bridges to find how various were the designs, and it was incredible to think that all of these were the best. He was not thinking of a number of different designs sent in by competing engineers. For example, he had had before him quite recently the competitive designs for the great Sydney Harbour bridge. "They were sent in by various people; but even so, the stress-length method would reveal the better design, assuming the same stresses were adopted. In the case of the American bridges, the various companies had their standard specifications. He would very much like to apply this method to the two great bridges of the world—the Forth bridge and the great cantilever bridge over the St. Lawrence. The conditions were the same, but the designs were different. Which of the two would be the better? Would the stress-length method say that the American design was the better, or vice versa? The expense per ton of material would be about the same. The question was the amount of material used in the design. The system of erection was the same. The Forth bridge, which was by Sir John Fowler and Sir Benjamin Baker, was considered to be the best that could be done under the circumstances. But some leading American writers had expressed the opinion that it could have been bettered.

In a case like that, economy of material was of the first importance, because each ton of material carried its own weight. In smaller bridges 5 or 10 per cent. additional would make very little difference in the total load carried. Under such circumstances simplicity of construction might outweigh the quantity of material altogether. In gigantic bridges it was not so.

SURFACE CONDENSATION.

Paper read by Mr. J. A. Smith, December 6th, 1905, Vol. VI.

The President said the subject of condensation was specially important just now. He did not know whether members had noticed since the last meeting the accounts in the journals of the "Carmenia," the great turbine ship, which was immensely beyond anything attempted in turbines previously.

The system appeared to be spreading rapidly, both in large and small vessels; for instance, several vessels were being built for the British-India Co. The surface condenser formed an important part of the system, and any information in connection with surface condensation was to be welcomed as valuable.

Mr. M. S. Smith said he would like to ask the author a question in respect to the thickness of the tubes used in the experimental
apparatus. He noticed that they were very thin—about 26 gauge. His firm used nothing less than 18 gauge, and in a special case he had met they were ½ inch thick; that was to resist a water head equal to about 200 feet. He thought such a thickness was excessive, but the experimental tubes appeared to be unduly thin. He would like to know how that would affect the experiments.

Mr. Jas. Alex. Smith, in reply, said that he did not find that the thickness of the tubes, within the usual limits, made any sensible variation. Isherwood and others held the same view. The tube he had used was intentionally thin, in order that metallic resistance to thermal transmission should be eliminated so far as possible. The same tube was used throughout, therefore the results maintained their relative values.

Doubtless, theoretically, variation in thickness must vary transmission, but it must be remembered that for an equal thickness water had a resistance several hundreds of times greater than brass. The water films, not the tube thickness, were the limiting factors in this case.

Mr. Jas. Alex. Smith said there was a new point in reference to condenser practice he desired to refer to—namely, the effect of the method of circulating pump driving upon condenser efficiency.

If diagram 7 of the paper were referred to, it would be seen that the rate—i.e., velocity—of flow exercised a marked effect upon the rate of condensation at a given surface. Further, that efficiency was not in direct ratio to the quantity or rate of the flow.

This gave rise to a rather interesting consideration. In the case of a pump driven from any part of a reciprocating engine, or machine, in which an evenly rotating shaft formed the regulating element, the motion of the pump plunger, or piston, would constantly vary, usually in simple harmonic ratio, between the limits of a maximum motion and zero. Thus at each instant the condenser efficiency would be varied as a function of the piston speed of the pump. It would be found that a considerable difference of value existed between the mean efficiency deduced from the varying efficiencies consequent upon the varying motion, and the efficiency due to the use of the same volume of water flowing at a constant rate. Thus, in a degree, the method of linking the pump would have an effect upon the efficiency of the engine as a whole.

When independent pumps of the duplex type, with a pause at each end of the stroke, overlapping strokes, and fairly uniform motion between extremes, this effect might not be so marked.

The President said the usual practice seemed now to be to use centrifugal pumps.

Mr. J. A. Smith said that was the growing practice, but very many reciprocating pumps driven from the engine were in use.

Mr. Geo. Higgins forwarded the following communication, which was read by the Secretary:

"Re Mr. J. A. Smith's paper on 'Surface Condensation,' may I ask Mr. Smith to be good enough to state in what respects he con
siders that his experiments and reasoning throw new light on the question? In an ordinary surface condenser, there is a constant flow through it; his experiments appear to have been made with vessels through which flow of vapour and atmospheric gases does not take place, and his results appear to me to be what we might have expected. Certainly, there is nothing novel in the chief conclusions given on p. 30."

Mr. C. W. U. Adamson said he thought the best way to judge of a paper was by the author’s own conclusions. Well, he could not see anything new in the conclusions.

Mr. G. Weymouth read the following contribution to the discussion:

If we desire to discuss a paper with justice alike to the writer and his subject, we must first clearly ascertain the object the writer had in view.

In the paper now under discussion the author states—Page 176, paragraphs 2, 3, and 4:—"The principal object is to contribute to the knowledge of an imperfectly investigated subject fundamental data derived from direct experimental researches by the writer. The aim is to stimulate discussion and examination of hypotheses that have been accepted upon premises that are no longer sufficiently accurate to meet the exigencies of modern design.

"The key note is the effect of gases—specifically atmospheric air—upon working efficiencies and capital investment.

"The chief deduction is that air, even in minute traces hitherto considered innocuous, should be, by initial design and subsequent management, rigorously excluded from high-grade surface-condensing installations. Certain laws of condensation are also enunciated."

Paragraph 5:—"Steam engine practice is inferred throughout."

This seems to imply that the author intends his treatment of the subject to have a direct bearing on practical engineering problems, rather than on the more abstract theoretical aspects of the question.

Also paragraphs 6 and 7:—"Even a superficial glance reveals the inadequacy of the older data. First, because the trend is to expand steam to degrees undreamt of in the past, when those data were collected, and to reject it at temperatures so low that crude rules, simply embodying average conditions, are useless. Second, because it appears to be tacitly assumed that generalisations derived from incomplete investigations of pure steam are equally, and without modification, applicable to mixtures of steam and gas."

"Experiment shows that such assumptions are fallacious, and that the presence of air in quantities that might have been deemed insignificant may, in fact, become the factor limiting the efficiency of a whole steam plant. For instance, air equal in pressure to only one-twentieth of an inch, by the mercury gauge, will at 90 degrees Fahr. reduce thermal transmission some 25 per cent., whilst three-twentieths will lower it about one-half. Before these special effects could be determined it was necessary to investigate the whole subject of surface condensation."
Here the author implies that the data previously in the hands of engineers are inadequate, that their methods of reasoning are fallacious, and that the presence of air in quantities which might have been deemed insignificant are now, as the result of his researches, shown to have a marked effect on the efficiency of the condenser.

Assuming that my interpretation of the author's objects are reasonably correct, I will now endeavour to direct attention to one or two matters which appear to require some explanation.

If the investigations were intended to have a direct bearing on the practical design of condensers, or on the elucidation of difficulties which are met with in their operation, it is evident that the experimental apparatus used, and the methods adopted, should be such that the conditions obtaining in the experiments have their counterpart in actual condensers.

Confining our attention to the most common type of condenser—viz., the marine water-tube type—this appears to be the least divergent from the author's experimental apparatus; still we find the disparity in the actual physical conditions so great that it is difficult to see which results, if any, derived from the experimental apparatus throws any light on the action of the surface condenser, or how they can be used to assist in its better design.

In the ordinary surface condenser the exhaust steam, etc., enters with a considerable velocity, and flows across the water-tubes towards the air pump suction inlet, where the volume and velocity of the steam, etc., leaving the condenser is relatively small. As the steam condenses on those tubes with which it first comes in contact, there is a tendency for a film of air (or other non-condensible gases present) to form on them. This, however, is rapidly swept away by the rush of steam past these tubes, and is carried forward, causing the percentage of air in the mixture to increase during its progress through the condenser from that which existed in the incoming steam to that which exists at the air pump inlet.

It will thus be seen that those tubes which are nearer to the steam inlet will have to deal with a mixture in which the percentage of air is small, and the sweeping effect due to the velocity is great, both of which tend to prevent the formation of an air film of appreciable density or thickness round the tubes. Those tubes more remote from the steam inlet are surrounded by a mixture in which the percentage of air is greater and the velocity, and therefore the sweeping effect of the flow less, both of which enable the film of air round the tubes to become denser and thicker. Still it must not be overlooked that there is a flow not only towards, but past all the tubes, and that no permanent accumulation of air can take place. The tendency of the flow is to bring into contact with the tubes at any part of the condenser a mixture of steam and air of the average quality which left the preceding tubes.

In the author's experimental apparatus the conditions are widely different. The flow is almost wholly towards the tubes, and there is no direct flow across them. The small quantity of air which is put in the apparatus will therefore be carried towards the tubes as con-
densation of steam takes place. When the steady state is reached the rate of diffusion of air against the direction of flow will be equal to the rate at which it is being carried forward towards the tubes by the flow. It will be apparent therefore that the density of the air will increase from the surface of the water, where it is a minimum, to the surface of the condensed water film on the tube, where it will be a maximum, and that this accumulation of air round the tubes will be permanent. Hence it appears obvious that, however small the quantity of air put into the author's experimental apparatus, the processes of evaporation, flow, and condensation will sweep the air towards the tubes, and cause it to permanently accumulate round them; the change of density of the air with distance from the tubes conforming to the condition already mentioned.

As the author has given no data of tests which will enable us to calculate the composition of the mixture round the tubes, a simple knowledge of the mass of air present and the volume of the space in which it is enclosed is of little or no value.

In paragraph 7, on page 176, the author states results (presumably based on experiments with his apparatus), which are intended to show that a very small quantity of air will materially affect the efficiency of a condenser. We have already seen that such a result could only be anticipated with apparatus such as described in his paper, but it must not, without further demonstration, be assumed that the proportion of air there stated would have anything like the same effect if the mixture was discharged into a surface condenser operating under ordinary practical conditions of rapid flow across and past the tubes.

In our endeavours to follow the author we are confronted with another serious difficulty—viz., the absence of all data with respect to the author's method of determining the temperature of the steam in his experimental apparatus; true, temperatures are given, but it is not explicitly stated where or under what conditions these temperatures were taken. As far as can be determined from Fig. 2, the only thermometer entering the steam space is the one shown with its bulb between the two condensing tubes. We have already observed that when this apparatus is in operation, and has reached the steady state, that the air density will be greatest close to the condensing tubes, and least at the evaporating surface of the water.

By reference to Fig. 2 it will be seen that the thermometer just referred to is situated between two portions of the U condensing tube near the free ends, or where the difference of temperature between them will be considerable. The thermometer bulb is thus in a place where convection currents will be set up, and bring the air which has been cooled by contact with the tubes into contact with it. This thermometer will therefore indicate a temperature less than that at which the steam leaves the surface of the water.

This question may be considered from another standpoint. For all practical purposes the pressure in the steam space may be regarded as constant at all parts. We have already seen that the density of the air, and therefore partial pressure due to the air, increases
from the water surface towards the tubes, consequently as the total pressure is constant, the partial pressure due to steam must diminish from the water surface to the tubes. We must therefore regard the steam as existing at different pressures, but uniform temperature in different parts of the apparatus (which is inconsistent with the properties of a saturated vapour), in which case we must admit that the steam becomes superheated as it moves towards the place of its least partial pressure.

If the steam is saturated at all parts, then its temperature must correspond with the partial pressure at all points, and will fall from a maximum at the water surface to a minimum near the tubes. In either case we cannot expect the thermometer placed where shown in the steam space (see Fig. 2 of the paper) to indicate the temperature at which the steam is formed. The author does not appear to have endeavoured to determine the differences of temperature or physical conditions which exist in the steam space of his apparatus, in common with all other surface condensers, which have such a well-known and important bearing on the process of condensation, and further, which throw so much light on the operating conditions, defective or otherwise, of a condenser.

The fall of temperature between the water surface and the tubes may be several whole degrees even when the whole mass of air present is quite small compared to that of the steam.

It is difficult, therefore, to understand why the author should, whilst ignoring these considerable differences of temperature, which exist in the different portions of the steam space, still lay such strong and repeated emphasis on his warning to avoid the error of commercial thermometers and other instruments.

On pages 185 and 186 of the paper, the author casts doubts on the truth and generality of Dalton’s law. (Sentences 8, page 185, and 1, page 186, Vol. VI.)

Dalton’s law as at present accepted by physicists and engineers has been of enormous assistance to them in their investigations, and the alleged discrepancies referred to by the author have caused no trouble in the past.

It will be admitted by all that the utmost pains should be taken to thoroughly grasp the true meaning of the generally accepted statement of a law before doubting its truth. If after such careful study uncertainty still remains, nothing but the most thorough and rigidly accurate investigation can justify the investigator in publicly assailing it, and even then his views must not be accepted until his conclusions have been verified by others.

Dalton’s law states that the total pressure equals the sum of the pressures of the several gases or vapours acting separately.

The author has nowhere shown that this statement is not true under every condition, other than when one or more of the components is near the critical temperature and pressure, or when chemical affinities exist between them. It has been previously shown that in the author’s apparatus, as illustrated in Fig. 2, there will be a gradual fall in the partial pressure of the steam from the surface of
the water to the tube surface, and a corresponding increase in the partial pressure due to air, and that at all points within the steam space the sum of these partial pressures must be practically constant. At what point in the steam space does the author endeavour to determine the partial pressure of the air present? From Fig. 2 of the paper it appears that but one thermometer bulb enters the steam space, hence he can only determine the partial air pressure at the place where this bulb is for the time situated.

It is obviously impracticable by this means to determine with sufficient accuracy the distribution of air pressure throughout the steam space, and consequently its mean pressure cannot be determined. The author has taken the temperature at one point of the steam space, and with that and the known total pressure has endeavoured to determine the average air pressure throughout the whole space. We might with equal reason take the pressure and temperature of the atmosphere at one point, and from that endeavour to calculate the total mass of water suspended in the earth's atmosphere. If the author had experimentally determined the form and dimensions of the surfaces in the steam space over which the partial air pressure was constant and known, he could then have found roughly the total mass of air present, and its mean pressure, and would have found no need to discredit Dalton's law.

Again quoting from paragraph 2, page 186, of the paper, we read: "It will also be noted that the law that a saturated vapour and its fluid in contact are at the same temperature ceases to be invariably true when gas is present."

Are we to understand that the author seriously desires to throw doubt on the results of the labours of Regnault and others who have investigated the properties of vapours?

Unless the conditions under which a given law is stated to hold are complied with, there is little need of surprise if it appears to fail. The author has shown us no reason for doubting that where a vapour is in contact with its fluid, the temperatures are the same.

In a working surface condenser, the steam is in close proximity to water, either on the tubes or in the form of spray falling through it, and consequently at each point the steam will be saturated, and at the pressure corresponding with the temperature at that point.

The truth of Dalton's law, and the fact that the temperature of a saturated vapour and the fluid in contact with it are the same, has enabled engineers in the past, and will enable them in the future, to determine the relative proportion of air and water vapour present at any desired point of their condensers.

Dalton's law is equally true for vapours which are not saturated, and where their fluids are not present, provided due attention be paid to the actual physical conditions of the vapour at the desired points. We must not, however, presume from known conditions at one point to predict the conditions at another, unless we know the laws of distribution of the quantities throughout the region considered.

The author says on page 198, last paragraph:—"The writer thinks it probable that tube length, temperature differences, and
rate of flow will be found to be expressible by a general law, at
times he has thought that he has glimpsed it, but hitherto he has
been unable to crystallise it.”

We would direct his attention to papers in the proceedings of the
Inst. C.E., London, by Dr. Stanton.

Mr. J. A. Smith said that Dr. Stanton had not dealt with
the question of air in relation to surface condensation in any form,
nor had he enunciated any general law connecting the matters re-
ferred to in his (the speaker’s) paper. Did Mr. Weymouth affirm
that he had?

Mr. Weymouth said not all of them, but that did not matter.

Mr Smith thought otherwise. As the hour was late (9.50), and
as Mr. Weymouth’s contribution was in written form, he thought
that, in justice to the subject, he should have an opportunity of
perusing it at leisure before replying.

In reply to Mr. Higgins, the paper must speak for itself, but it
might be pointed out that each of those who had contributed to the
discussion had dealt with one phase of the subject only; the greater
portion of the paper was not referred to.

The President said the discussion would stand adjourned until
the next meeting. (Refer to page 29.)

PAPER.

NOTES DESCRIPTIVE OF THE ELECTRIC SUPPLY
STATION OF THE MELBOURNE CITY COUNCIL.*

By Mr H. R. Harper, City Electrical Engineer.

In connection with the proposed visit of the Victorian Institute
of Engineers to the Electric Supply Station of the Melbourne City
Council, a few notes on the history and development of the under-
taking may not be amiss.

In 1894 the City Council started the lighting of the streets by
means of electricity, generated at the present Supply Station in
Spencer-street. The scheme at that time contemplated only public
lighting, that is to say, streets and markets, and a sum of £80,000
was spent on buildings, generating plant, aerial lines and lamps for
this purpose.

About three years later further expenditure to the extent of about
£30,000 was incurred in installing plant for supplying current for

* Issued in advance in anticipation of Visit (see page 27).
paper had been discussed for six months, but no discussion of any kind had appeared in the "Proceedings," except the President's own official remarks.

Mr. Smith said this was not correct. The paper had been read at the last meeting of the past session, and, owing to the vacation, there had been no opportunity for discussion save that at the preceding (April) meeting, a month ago.

Mr. T. Hill said that if members would cast their memories back they would recollect that under the old system papers had been read—and a good many of them—and the discussions were never printed at all. They were absolutely lost. This would give precedents for deferred publication.

After further desultory discussion it was decided, at the President's suggestion, to remit the question to the Council for consideration.

The President said he presumed they had met for the purpose of completing the discussion, and giving Mr. Smith an opportunity to reply; his paper had gone to England, appeared in some of the leading Engineering Journals, and arrived back here some weeks since.

Mr. Mitchell said if the paper had been sent privately it was against the rules.

Mr. Smith said that was not so. After publication by the Institute the author had full rights (vide rule 64—last line—a and d) to republish, otherwise they could not expect to receive papers relating to original work.

Mr. J. A. Smith's paper on "Air in Relation to the Surface-condensation of Low-pressure Steam: an Experimental Study of Condenser Problems," was discussed, and, after Mr. Smith had replied, the discussion was closed.

After the meeting had recorded votes of thanks to Captain G. F. Wilkinson for the visit arranged by him to the Submarine Depot at Swan Island and the Forts at Queenscliff, and to Mr. H. R. Harper for throwing open for inspection the Melbourne City Electric Supply Station at Spencer Street, Melbourne, the meeting adjourned.

DISCUSSION.

SURFACE CONDENSATION.

Paper read by Mr. J. A. Smith, December 6th, 1905, Vol. VI.

Mr. W. C. Rowe said thanks were certainly due to Mr. Smith for the interesting paper he had read, and the very careful exemplification by experiment he had placed before them. Mr. Smith never brought anything under their notice which was not fully illustrated. The paper had been republished in well-known English engineering journals, and also in foreign literature, and commented on in edito-
rials. That showed that the matter was recognised as new, and the subject of importance.

Mr. A. G. M. Michell differed from the views of the preceding speaker.

The President read the following notes:

Mr. Smith has experimented upon the state of things existing in a condenser where there is an almost perfect calm, but Mr. Weymouth has pointed out that in any actual working condenser this is not the case, but that the contents are moving "across the tubes," "with considerable velocity." Feeling curious as to the magnitude of this velocity and its probable effect on the action of the condenser, I have made the following calculations, the results of which I confess somewhat surprise me:

I took an actual case of the engine of a large steamboat, of which I had tolerably full particulars, including indicator diagrams. The piston speed was 750 feet per minute, and the diameter of the main exhaust pipe to the condenser one third of that of the cylinder; hence, assuming no change of volume in the steam, the average velocity in the exhaust pipe would be 9 times 750, or 6750 feet, per minute. But there was a considerable change of volume. The pressure in the cylinder at the instant the valve opened to exhaust was 8 lbs. per sq. inch, while the condenser pressure was only 3 lbs. per sq. inch. Hence the volume of the steam was more than doubled, and its average velocity becomes fully 15,000 feet per minute. But this is only the average; the maximum is far higher. An inspection of the indicator diagram shows that the contents of the cylinder drop from 8 to under 4 lbs. per sq. inch during one-tenth of a revolution, while the expulsion of the cylinder full of steam at this latter pressure occupies fully four-tenths of a revolution. In other words, half the steam gets away in one-fourth of the time that the other half takes, and so must travel at four times the speed. The second half will get away at the speed already calculated—namely, 6750 feet per minute, so the first half must move at the enormous velocity of 27,000 feet, or more than 5 miles, per minute. Now what is the effect of these prodigious blasts of steam on the condenser tubes and the air and vapours in the condenser? The best formula connecting the velocity of wind and its pressure on a flat surface at right angles to its motion gives for a speed of 5 miles per minute, a pressure of 300 lbs. per sq. foot, but as steam at 3 lbs. per sq. inch is only one-tenth of the weight of air at ordinary atmospheric pressure, this would be probably reduced to 30 lbs. per sq. foot, which is still equal to hurricanes that blow down chimneys and houses, and overturn railway trains. Of course we must not build too much upon a formula that has been experimentally verified to velocities of less than one-third of that here given, but still it appears certain that a most tumultuous state of things must exist in the condenser instead of the comparative calm of Mr. Smith's apparatus. I have heard of an unusually long condenser in which great trouble was caused by violent vibrations of the tubes, and in view of the preceding calculation do not wonder at the experience.
Again, the fact that the steam has been lowered in pressure from 8 lbs. per sq. inch to about 3, by passing through a gradually opening slide valve, indicates that the steam reaches the condenser in a superheated state, and this must considerably affect its behaviour.

The preceding case is by no means an extreme one. I have diagrams in my collection showing a fall of from 12 to 3 lbs. per sq. inch in less than one-tenth of a revolution, the engine making 60 revolutions per minute, but, unfortunately, I do not know the diameter of the exhaust pipe.

In view of the enormous velocities attained in the exhaust pipe of a condensing engine, and the consequent difference of pressure between the cylinder and condenser, every effort should be made to render this pipe as large in diameter, short, straight, and well formed as possible. I am convinced from not a little actual experience that this point has not been as fully recognised as it deserves. Especially in the case of non-condensing engines that have afterwards been turned into condensing engines is insufficient area of exhaust pipes likely to be found.

As to the condenser itself, the practically useful research that should be made is to carefully test existing condensers under working conditions, compare the results, copy the more successful forms, and so by a gradual process of selection evolve the condenser of the future.

Mr. D. Buchanan said he could bear out what Professor Kernot had said with regard to the vibration of the tubes of condensers. He had, in some cases, been compelled to put in baffle plates to prevent the steam bending the tubes. The vibration was so great that sometimes the tubes split.

The President called upon Mr. Smith to reply to the discussion.

Mr. J. A. Smith said he would first take the President's own contribution. Several of the points raised would be dealt with in the reply he intended to give to the notes read by Mr. Weymouth. In the interim he would point out that when an engine exhausted at 8 to 12 lbs. to the sq. inch, the steam temperatures (183 to 202 deg. F.) would be so great, and the consequent steam and circulating water temperatures differences in the condenser so large, that there was little use in considering minutiae of design. This was a state not to be perpetuated, but to be avoided; at the present time engineers were striving to save the last ½ lb. of pressure. Exhaust at 8 to 12 lbs. per sq. inch implied a waste of perhaps 20, or more, per cent. of engine efficiency. Further, the velocity in the exhaust pipe was much in excess of the velocity in the much larger area of the condenser casing, where it met the tubes. Professor Kernot had not referred to this.

Mr. Buchanan's remarks were of practical interest, but tube bending might be due to projected water, not steam.

Mr. Smith then read the following general reply:

The paper under discussion, and that—"Air in Relation to Boiler Feeds"—to which it is a sequel, appear in different volumes (VI. and
V. of the "Proceedings." As some time has elapsed since the reading, it will be advisable to consult the originals before accepting quotations divested of their explanatory and limiting context as sufficiently, or accurately, expressing my views or the facts.

To avoid elementary detail, it was premised in the first paper that "... It is presumed engineers will peruse the subjoined matter in the light of their own experience, therefore that elaborate explanations are unnecessary." For a similar reason the paper now under consideration specified that "steam engine practice is inferred throughout"—i.e., definitions, magnitudes and methods were to have their engineering significance.

It was not anticipated that the material given would enable non-specialists to solve at sight complex problems. When penning the introduction, Bacon's words were in mind:

"Crafty men contemn studies; simple men admire them; wise men use them: for they teach not their own use; that is a wisdom without them, and won by observation."

*In reply to Mr. G. Higgins—*

The question of the experimental and other conditions is dealt with in the reply to the communication read by Mr. Weymouth, and in the text of the paper itself.

*In reply to the communication read by Mr. G. Weymouth—*

The paper under discussion is condemned in toto, but illogically, since none of the strictures apply to a large section which has not been discussed. Water Initial Temperature, and Rate of Flow Effects; Tube Temperature Grades and Length; Compound Condensation or "Vacuum Augmentation"; Rate of Condensation in Pure Steam, etc., included in the paper, are not referred to in the various criticisms.

One of the critic's opening statements is, "... it is evident that the experimental apparatus used, and the methods adopted, should be such that the conditions obtaining in the experiments have their counterpart in actual condensers." With this issue may be joined. The statement would be true only if existing condensers were ideally perfect, a condition often notoriously non-existent. Not the perpetuation of defective present conditions, but the attainment of yet higher standards, with certitude by the establishment of fundamental functions, is the end in view.

In the critique points do not follow in sequence; in the interests of lucidity the replies, when not supplied by the text of the paper, will be classed as follows:

(a) Condenser working conditions.
(b) Applicability of experimental apparatus.
(c) Methods of measurement.
(d) Physical laws.

(a) Condenser working conditions—

The critic's contention is that vapour flow "across the tubes"—
that is, a current that would remove solid matter from the surfaces into the spaces between them—differentiates actual condensers from the experimental apparatus. But there are large and important classes of condensers where flow of this nature has no place—i.e., when the steam is within the tubes, or between surfaces.

The tubes may be long or short; straight, curved or coiled; the surfaces may be plane or corrugated plates; or, as in the case of certain evaporative condensers, the steam may pass through convoluted tubes of 100 feet, or more, in individual length, with hollow fins, flanges or annular pockets at each few inches, thus:

\[ \_\_\_\_\_\_ \]

In each of these cases there is continuity of vapour contact with the surface, and uninterrupted air concentration along the surface. To all can the teaching of the experimental work be adapted, to none does the concept of the critic apply; it would, indeed, be difficult to imagine a further deviation from it than in the case of the preceding type of evaporative condenser, with prolonged air contact and multiplicity of sheltering pockets.

Thus the critic's specific limitation of consideration to a selected type cannot be entertained. Even in the "marine-water-tube" form he selects, his assumptions require very considerable modification. Frequently the exhaust inlet and air-pump branch are so placed, and the condenser casing is so proportioned, that the flow is largely one of constant contact, with discontinuity of contact in a minor degree only. Diaphragm plates may also accentuate the continuity.

As a generalisation, the critic's simplifying assumption requires reconsideration.

As to flow velocity. Before non-parallelism of conditions can be established, not only must it be shown that the velocities found in the practice of leading designers are relatively great when contrasted with steam efflux in the ordinary significance, but also that they are intrinsically sufficient to materially disturb the causes to which retarded thermal transmission is due. On these points not one iota of proof is advanced.

Indefinite expressions are used—i.e., "... the exhaust steam enters with considerable velocity," "... is rapidly swept away by the rush of steam," "... the sweeping effect due to the velocity is great," "... rapid flow across and past the tubes." This may apply in some plants of older date or indifferent design; not so in up-to-date work. Engineers now know too well that when striving for the best economy in engine practice, they can afford to throw away nothing in the thermal equivalent of exhaust steam velocity. So important is this matter in turbine installations that if condenser surface efficiency depended upon velocity of flow of the steam, a portion of that efficiency would be unhesitatingly sacrificed, and the balance again struck by using the lower velocity and a greater sur-
face at increased capital cost. The table of thermal efficiencies in the first portion of the paper may be consulted in this connection.

Exhaust sections have been enormously enlarged in recent years, but even that does not fix the velocity of vapour impact, which, in water-tube condensers, depends upon the yet greater cross sectional area of the condenser casing. At subsequent stages wide spacing of the first tube ranges and passages between the nests serve to keep down the velocity. None of these points appear in the critique.

The nett result is, that in a high-class condenser the vapour meets the tubes with an initial mean velocity less than that of a breath expired from the human lungs with moderate rapidity, and diminishes to a general mean rate of progression of less than one inch per second before entering the air-pump branch.

The following example applies to an actual type to be found in use in modern stations abroad:—

**Example**: 1,000 KW turbine; 18,000 lbs steam per hour; exhaust pipe area 28.27 sq. feet; condenser casing cross sectional area 71 sq. feet; volume of steam entering condenser = 300 cub. feet per lb.; mean velocity of steam impact upon tubes = 21 feet per second. Air pump capacity per one lb. of steam used = 0.6 cub. feet; consequent final mean velocity of flow between last range of tubes = 2 of one inch per second.

In this connection it is instructive to watch, through the observation windows of large jet condensers, the action of steam entering at velocities in excess of the velocities in high type condensers. The rain of drops, descending under the influence of gravity alone from a perforated tray, is scarcely deflected from the vertical.

Thus it is evident that in the more modern products of condenser evolution there is not, in the ordinary significance of the term, considerable velocity, rapid rush, and so forth.

It remains to consider the sufficiency of the velocity to disturb, sensibly, the conditions limiting condensation.

The critic assumes that thermal retardation is, in the condenser, due to the simple presence of air diffused in a sensibly uniform manner throughout the environment of any individual tube, whilst he holds that there is a progressive concentration towards the tube surface in the experimental apparatus. The latter matter will be dealt with in the next section. He does not attempt to show that for equal mean pressures the results would differ.

The critic ignores all other, or molecular effects; but it must be remembered that at whatever rate or angle the mixed steam and gas meets the refrigerating surface, it is there, at the actual film of condensation, that the diffused gas is rejected as a residuum. It is in that region of limited thickness that condenser steam diminishes some 20,000 fold in volume, and parts with some 850,000 foot pounds of energy per one pound of steam condensed.

The matter is expressed in the paper thus:—

"Logically it follows that the condensation of pure steam, and of steam mixed with a permanent gas with which it does not combine chemically, is radically diverse.

"Steam progresses continuously towards the surface where it be-
comes water, passing away in that dense state, almost without resis-
tance, as drops or a gliding film. The inter-diffused gaseous mole-
cules are impelled towards the same surface, but in that direction
they cannot escape.

"Dalton and Graham's laws of partial pressures and diffusion
make for homogeneity of mixture, but they are rigidly true only when
the volume is statically constant; here the conditions are dynamic.
The result is ultimate equilibrium, but not homogeneity.

"Since the steam condensation is chiefly evident in a thin lamina
of minute thickness, at the condensing surface, it follows that, given
a moderately rapid motion of general approach, then, although the
gas may at first be highly tenuous, a very slight retardation of dif-
fusion may permit of an accumulation of molecules at that laminar
focus of energy change, there their impedance is a maximum."

It is probable that these phenomena, and their results upon trans-
mission, are sensibly unaffected by the limited average velocities of
general translation of the vapour, shown to exist.

(b) Applicability of the experimental apparatus—

The critic apparently assumes that the mean condition of the ex-
perimental apparatus is to be taken as the mean for the whole volume
of a surface condenser, whereas it must, obviously, be considered,
during any one experiment, as referring to a region only of a con-
denser. The graphically delineated results give the means of dealing
with any region.

The gas distribution in differing types of condenser may be very
diverse, and must be fully considered in deciding upon their merits.
In some it may be uniformly progressive, in others eddy and vortex
whirls (from turbine blades, for instance), with centrifugal distribu-
tion, or a uniform mechanical initial diffusion of the mixture amongst
separate nests of tubes, will lead to a short, relatively pure steam
phase. The distance through which molecular diffusion must pro-
gress is also a factor. The characteristics of the vapour mixture
will vary in each particular case. These, and many other points, the
engineer engaged upon design must recognise, but they in no sense
negative the application of the experimentally-derived data.

Just as in a multitude of other engineering matters, it is requisi-
te to deal with the surface, not as an entity, but as composed of
unit areas, determining the co-efficient for each component, and
averaging the aggregate.

To the extent of my facilities I have endeavoured to supply funda-
damental data applicable to component unit areas.

The critic appears to concept the apparatus as small tubes at a
considerable distance from a perfectly quiescent water surface, no
disturbing factors, and a regular variation of vapour density. The
actual case is that the tubes are at a maximum distance of 2½ inches
only from the water surface from whence the steam is disengaged.

The distance was fixed, after actual test of closer approach, as
the least at which actual contact with the agitated surface could be
avoided. It must be remembered that the water is nearly air free,
and that it is under low pressure, hence its surface motion is considerable. A glance through the observation window shows the non-existence of the critic's premised conditions. Drip from the tubes would alone vitiate them.

The apparatus is simple, but is a product of evolution carried through a series of more complex antecedent forms; every detail, however trivial, has been carefully considered.

In reference to the preliminary illustration:—

"For instance, air equal in pressure to only 1-20th of an inch, by the mercury gauge will, at 90 deg. Fahr., reduce thermal transmission some 25 per cent., whilst 3-20ths will lower it about one-half. Before the special effects could be determined it was necessary to investigate the whole subject of surface-condensation." The comment is "...it must not, without further demonstration, be assumed that the proportion of air there stated would have anything like the same effect if the mixture was discharged into a surface condenser."

In the critique the mean condition of the residual vapours concentrated in the condenser after the steam has largely passed away as water, is confused with the initial condition of the vapour entering the condenser from the engine—a totally different matter.

It would be quite impossible to deal with such an initial mixture, say five of steam to one of air, in any supposable condenser, nor is it supposable that such a mixture could exist in practice; therefore, the comment is not relevant.

(c) Methods of measurement.

The statement that there is an "absence of all data" in regard to thermometer positions is incorrect. In Fig. 2 of the paper, thermometers in the steam space and in the boiling water are clearly shown, as are also those used for the determination of tube temperature grades and water temperature increments. An examination of the exhibited apparatus would have disclosed the existence of thermometer pockets in other positions. This, and much more, is inferred under the head "Basis of Experimental Research," as follows:—"After considerable tentative work, necessitating many hundreds of tests, the author rested in the construction and methods to be described." In the light of experiment the thermometers, placed as delineated, were deemed sufficient for the end in view.*

The suggested method of determining isothermals and isobars would be theoretically applicable, with an infinity of difficulty, in an ideal state which has no place in the apparatus. If solutions of these academic points are desired, then, predating the "steady state," they may be much more readily deduced by reference to mean air pressure and rate of condensation required.

Two entirely distinct hypotheses, based upon conditions that it is said the methods of thermal measurement advocated would have

* In a photo in "Engineering," vol. lxxxi., p. 395, three thermometer pockets, inserted along the top centre line of the lower apparatus, into the vapour space, are shown.
revealed, are given; alternatives are not submitted. Since one postulates superheat, the other saturation, one premise at least must be in error, hence the statement that the physical conditions are "well known" is not supported.

In this connection "The Engineer" (March 16th, 1906, p. 275) may be quoted:—"It is well to premise that a very dense ignorance exists as to what steam really is, how it is made, and what, under certain circumstances, it will do . . . whilst the way in which it parts with its heat to solids under various conditions is complex to a degree . . . the laws of condensation as a whole are little understood, and the design of surface condensers is purely empirical."

(d) Physical Laws

I am accused of casting "doubt on the truth and generality of Dalton's law," and it is said:—"It will be admitted by all that the utmost pains should be taken to thoroughly grasp the true meaning of the generally-accepted statement of the law before doubting its truth. If after such careful study uncertainty still remains, nothing but the most thorough and rigidly accurate investigation can justify the investigator in publicly assailing it, and even then his views must not be accepted until his conclusions have been verified by others.

"Dalton's law states that the total pressure equals the sum of the pressure of the several gases or vapours acting separately.

"The author has nowhere shown that this statement is not true under every condition, other than when one or more of the components is near the critical temperature and pressure, or when chemical affinities exist between them."

In condenser work, the applicability of the law to condensing steam as well as to gases must be considered.

A reference to the works of the great investigators who have, from Regnault onwards, considered the law, would have revealed to the critic that it had not been reserved to me to discover its limitations. For instance, to quote those references at hand:—

Ostwald writes:—

"As the statement does not hold good for liquids, gases show deviations from it the nearer they are brought to the liquid condition."

Preston:—

"This result [the law] was discovered experimentally by Dalton, and is true, of course, only so long as the molecules do not sensibly obstruct each other."

Again:—"In the case of vapours, the law is approximately obeyed within certain limits and with certain restrictions."

Again:—"The result of his [Andrews'] investigation led him to conclude that under high pressures Dalton's law is largely deviated from, and is probably only strictly true for gases in the so-called perfect state."

These and other authorities have also shown that the divergence is not confined to the "critical point," but is a function of temperature.

Dalton himself knew the limits, but perhaps a final quotation,
extracted from a readily accessible work—"Watson's Handbook of Physics"—may suffice:

"Experiments by Andrews show that, in the case of the mixture of two gases [or vapours], Dalton's law only holds if the gases are far removed from their point of liquefaction—i.e., are practically in the condition of perfect gases."

The hypothesis enunciated by the critic is not Dalton's law, as comparison will show. Here are Dalton's own words:

"When any two or more mixed gases acquire an equilibrium, the elastic energy of each against the surface of the vessel, or of any liquid, is precisely the same as if it were the only gas present occupying the whole space, and all the rest were withdrawn."

There is a note of deeper meaning in this, frequently lost in attempts to abridge or simplify it.

A knowledge of Dalton's law, its limitations, and its genesis, tends greatly to facilitate comprehension of the subject, which is essentially molecular. The molecules of any one medium which may pervade the common space, simultaneously pervaded by the molecules of other media, have no action upon these latter except by direct collision. Since the volume of an individual molecule, compared with the volume which it pervades, in a "perfect gas," is negligibly small, it follows that collisions are relatively infrequent, and the law is sensibly accurate. But when change of state is in question, as, for instance, in a condensing vapour, the conditions are different.

It has been shown that condenser steam decreases some 20,000 fold in volume when it becomes water, hence the collisions, and consequent deviations from Dalton's law, become so great that the law can no longer be depended upon, even approximately, to determine a priori the phenomena at that stage. The law changes from that of the solution of a gas in a vapour to that of gaseous solution in a liquid.

The graphics in the paper show that the first minute addition of gas to pure steam has a very much greater effect than an equal amount following a considerable increment. This indicates the operation of disturbing effects at the region of condensation, and they cannot be ignored, as they have been in the critique.

My remarks in relation to the law in another phase are quoted without their specific limitations, explanations, or illustrations. They are under a distinctive heading—"Apparatus: Method of use. Determination of Gas Pressure by Gauge"—and must be considered in this relation (when the true meaning is obvious), not as detached therefrom.

The use of the law, under suitable conditions, is enjoined, its truth under similar reservations affirmed, and warnings against its misuse—all too frequent—given.

The extension of the warning for the benefit of those who, without professing to be physicists, are often responsible for the efficient management of installations, was not unneeded.

The engineering text-books are silent upon the pitfalls of application, and practical engineers know how seldom testing apparatus, other than the vacuum gauge, is supplied with condensers. When
thermometer pockets exist, rarely indeed are they found elsewhere than in the exhaust inlet and air pump branch, and this holds, in some cases at least, when the apparatus has been specially constructed for experimental work.

In an absence of knowledge of the limitations, the curious efficiencies or non-efficiencies that may be attributed to condenser or pump are apparent.

The question of homogeneity of mixture certainly had not escaped me. I do not, however, attach the weight to it that the critic does. It is referred to under "Physical Basis of Condensation," etc. Were this the sole cause of retardation, and were the law absolute and the state steady, and the other premises of the critic admitted, it would follow that the condensation of steam at 140 deg., when reduced 50 per cent. by the addition of air, would require the existence of a temperature difference of more than 30 deg. F., and a pressure difference of more than 15 lbs. per square inch in the interval of 2½ inches separating the refrigerating from the steam generating surface. No such difference can be found.

As to the comments, "Are we to understand that the author seriously desires to throw doubt on the results of the labours of Regnault and others who have investigated the properties of vapours?" and "The author has shown no reason for doubting that where a vapour is in contact with its fluid, the temperatures are the same."

Nowhere have I thrown doubt upon the work of Regnault, as the critic would have ascertained had he perused the classic memoirs of that great investigator. Again, it has not been left to me to discover that a vapour in contact with its liquid may differ in temperature therefrom.

It has been recorded by all who have investigated the subject of solid or gaseous solution in a liquid, that the temperature of the vapour may vary greatly, even more than 100 deg.—from the temperature of the liquid generating it.

It must be remembered that condenser water differs from ordinary water in that it is, chemically, a weaker solution of atmospheric gases, and that, in the cases cited, there is also a mixture of vapour and gas incumbent upon it.

Inter alia, the meanings attached to the word contact in relation to a liquid and vapour by Regnault and the critic appear to differ. In Regnault's work, upon which modern steam tables are based, the temperature of the vapour was taken by a thermometer immersed in it at four inches from the water surface from whence the steam was disengaged, and rather more than that distance from a surface in which it was in progress of active condensation. The sufficiency of a thermometer so placed has not, hitherto, been questioned. The arrangement is figured and described in the original memoir.

That the hot well-water or the surface or jet condenser spray does frequently differ in temperature from the vapour at any part of the condenser is a thoroughly substantiated engineering fact. The investigator must consider the environment, and deduce his conclusions with discretion. The critic has ignored the factor of time in relation to change of condition.
In connection with air determination, certain statements are made:

I. "... a simple knowledge of the mass of the air present and the volume of the space in which it is enclosed is of little or no value."

II. "... It is obviously impracticable to determine by this means with sufficient accuracy the distribution of air pressure throughout the steam space, and consequently its mean pressure cannot be determined."

III. "If the author had taken the trouble to experimentally determine the form and dimensions of the steam space over which the partial air pressure was constant and known; he could then have found roughly the total mass of air present and its mean pressure."

In "I.,” the knowledge that the mass of air and the volume is known is displayed; in “II.,” it is denied; in “III.,” a method is advanced to determine it “roughly.”

The answer is simple. The critic erroneously supposes that the mass can only be determined during the progress of an experiment, and by the method he elaborates. As a matter of fact, definite masses of gas are introduced into the containing vessel, previously gas free, at atmospheric temperature (corrections for temperature—about one-fifth of 1 per cent. per deg.—have been given) before the test is commenced.

The procedure is fully described in the paper, and it was demonstrated at a meeting at which Mr. Weymouth was present.

By Boyle’s law the pressure is at once arrived at; but as both the gaseous mass and the containing volume are constant, by the law and its corollaries this pressure is the mean pressure of the gas, however the distribution of internal pressure or aggregation may be varied.

Thus the critic’s assumption that the mean gas, or air, pressure cannot be determined is fallacious, and the numerous conclusions—including the final one—resting upon it, fail.

Dr. Stanton’s work, which I appreciate, does not deal with the presence of air or other gases, and there is no similarity in our methods in other respects; neither does he claim to enunciate a general law connecting the matters referred to.

In conclusion, it has been necessary to reply to the positive statements contained in the critique, in some detail. But the results given in the paper are, as therein stated (under “Physical Basis”), based solely upon observed facts, and are in no case deduced from physical laws or formulae, therefore the accuracy or otherwise of these latter in no way affects the data, or their relation to the purposes of the practical engineer.

The President said Mr. Smith had dealt very fully indeed with the various criticisms on his paper. They had had a very animated discussion on the question, and he thought it would be of use to them in directing their attention to the behaviour of steam in the condenser. Points had arisen which he had not considered before.

There was much worth study concerning surface condensers, and he felt sure they were all indebted to those who had discussed the matter, the discussion upon which was now closed.
Author/s:
Smith, James Alexander

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